TRANSACTIONS

of the

TWENTIETH NORTH AMERICAN WILDLIFE CONFERENCE

March 14, 15, and 16, 1955

Sheraton-Mount Royal Hotel

Montreal, Quebec

Edited by James B. Trefethen

Published by the
WILDLIFE MANAGEMENT INSTITUTE
WIRE BUILDING
WASHINGTON 5, D. C.
1955

Additional copies may be procured from
WILDLIFE MANAGEMENT INSTITUTE,
Wire Building, Washington 5, D. C.
for $2.75 Postpaid
(U. S. Currency or its equivalent)

PRINTED IN
U. S. A.

Printed by
MONUMENTAL PRINTING CO.
Baltimore, Md.

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THE WILDLIFE MANAGEMENT INSTITUTE wishes to express its appreciation to The Wildlife Society and the many individuals, organizations, and agencies that contributed to the success of the Nineteenth North American Wildlife Conference.

THE NORTH AMERICAN WILDLIFE CONFERENCES

The Twentieth North American Wildlife Conference marked the end of the second decade since the first of these international meetings was called by President Franklin D. Roosevelt in 1936. Since that year, the profession of wildlife management and the management of the renewable basic natural resources upon which wildlife depends, have come a long way. The past 20 years have seen the growth of the federal, state, and provincial agencies in Canada and the United States staffed by competent men trained in their respective fields, the extension and expansion of university training in the natural sciences, and the development of new concepts in the restoration and management of natural resources. It also has seen the development of new concepts, not the least of which has been the increased recognition that the management of natural resources has international as well as national implications.

For this reason it was felt fitting to hold this milestone conference in Canada, which annually has sent large numbers of her fine technicians and administrators to Conferences scheduled in the United States. One other Conference, in 1942, was held in the Dominion at Toronto.

One of the significant features of the Twentieth North American Wildlife Conference was the following announcement made at the annual banquet by Dr. Ira N. Gabrielson, president of the Wildlife Management Institute.

Canadian Industries Limited Establishes Wildlife Scholarship Fund

It is a real privilege to announce that Canadian Industries (1954), Ltd., has established a fund to provide for scholarships and fellowships for Canadian students in Canadian universities for those who wish to take college training in the field of wildlife management. This program will provide assistance both at the undergraduate and graduate level, and it will be flexible enough to enable assistance to be provided to qualified students in various schools in Canada.

I am sure that it is not necessary for me to emphasize to this audience the far-reaching significance of this action. The hundreds of technicians who are present at this meeting are aware of the tremendous upsurge in the development of trained wildlife men that followed the establishment of a somewhat similar program in the United States in 1935. The CIL plan is similar but was adapted to meet existing Canadian conditions. These scholarships and fellowships will be provided at selected colleges and universities where adequate training is available or can be developed.

Canada's population is increasing rapidly, and this increase is bound to result in increased demands upon the wildlife resources and also increasing problems for the responsible authorities. These demands will require additional trained men to meet the problems. There is no question but what this program will give great impetus to the training of Canadians to meet the rapidly growing needs of the provincial and dominion wildlife departments for men.

The grants will be made on an actual project basis, and emphasis will be placed on problems affecting the management of Canadian wildlife species. The Committee which will screen the projects will consist of S. J. Smart of Canada Industries (1954), Ltd., who has been instrumental in working out this program, Hoyes Lloyd of Ottawa, and myself. I need not tell you that I am happy indeed to be associated with this great step forward and will do my utmost to help it succeed.

The Twentieth North American Wildlife Conferences, in terms of registered attendance, was one of the smaller recent Conferences, but attendance at the individual sessions—a surer criterion of the success of any meeting—was high. Of the 709 registrants at the Conference, 506 attended the annual banquet, the largest proportionate attendance of this annual function ever recorded.

The Wildlife Management Institute is indebted to many individuals, in Canada and the United States who contributed to the success of the Conference. Special mention must be made of the contribution of C. Gordon Fredine, who represented The Wildlife Society as Chairman of the Technical Program Committee; and Dr. Ian McTaggart Cowan for his stimulating Appraisal of the entire Conference Program.

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PART I

GENERAL SESSIONS

GENERAL SESSIONS

Monday Morning—March 14

Chairman: A. W. Trueman
Government Film Commissioner, National Film Board, Ottawa, Canada

Vice-Chairman: Stanley G. Fontanna
Dean, School of Natural Resources, University of Michigan, Ann Arbor, Michigan

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TWENTY YEARS OF RESOURCE PROGRESS?

The First General Session of the Twentieth North American Wildlife Conference convened in the Ballroom of the Sheraton-Mount Royal Hotel, Montreal, Quebec, Canada, at 9:20 a.m., Dr. A. W. Trueman presiding.

FORMAL OPENING

Ira N. Gabrielson
President, Wildlife Management Institute, Washington, D. C.

For the second time since the First North American Wildlife Conference in 1936 we are meeting in Canada. It is always a pleasure for me personally to come to Canada. Maybe that is one reason why we're here.

However, we have another very good reason. Aside from liking to be in Canada and liking our Canadian friends, I know that we have a new generation of wildlife men in the States who need some education, and we use these conferences and move them around so that the new men can be educated.

I have heard some comment about how cold it is, and about the snow, and maybe some of these boys never saw snow before except that made in the movies with torn-up paper; so we thought that it was very fine, fitting, and desirable that we come to a country where they have real snow that wasn't made by a movie prop man.

I think you will enjoy your stay in Canada in spite of the cold weather. Don't forget, those of you from the deep South, that residents of this part of the continent dislike the heat down there as much as you dislike the chilly mornings that we find up in this country occasionally.

So, we're glad to be in Canada and glad to be meeting with our Canadian friends.

We have, as you know, been carrying on these conferences for many years; but it is your conference, not ours. The Wildlife Management Institute sponsors it, does the necessary legwork, and makes the mechanical arrangements; but if those of you who are attending your first Wildlife Conference, will look on the inside of the front cover of your program you will see that the program is made up by people drawn from many organizations—from Mexico, Canada, and my own country—and representing many different phases and interests of the conservation movement. It is a program prepared by the conservation groups of this continent.

That is the General Program Committee. The technical programs, which you will also find listed, are prepared and organized by The Wildlife Society, an organization which has members in all of the countries of this continent.

For the benefit of those who are here for the first time I have to bore the oldtimers by repeating again the statement that this is a conference. It is not a convention of an organization of delegates. It is a conference made up of officials of various countries and various provinces and states, a conference made up of representatives of organizations and of individuals. Everyone who is interested in conservation is welcomed to any of these meetings, or to all of them that he cares to attend.

It is a conference where you have an opportunity to meet other people and to hear the latest developments in the wildlife field and the other things that are going on in the conservation world, no resolutions are passed, no motions are made, and none is adopted. This is a meeting for an exchange of information and views. Such action as may come out of it will come through the actions of the various organizations which participate and which pick up such information as they may get here and fit into their own programs. So each chairman of these sessions will be instructed not to entertain motions nor to present any resolutions.

With that very brief statement of the purpose and aims and program of this Conference, I am now opening the Twentieth North American Wildlife Conference and turning the first session over to Dr. A. W. Trueman, the Commissioner of the Government Film Board of Canada.

INTRODUCTORY REMARKS

A. W. TRUEMAN
Government Film Commissioner, National Film Board, Ottawa, Canada

Mr. President, Mr. Minister, Ladies and Gentlemen, Members of the Conference: In the course of a good many years I have attended a good many conferences, and sat, it seems to me, on innumerable committees; and as I look back over this long schedule of planning and discussion, I am tempted to form the judgment that a distressingly large part of it wasn't worth while; but it would be impossible, I am sure, to form such a judgment about this Conference.

It must be a source of satisfaction to us all to reflect that we are taking part in a project of such magnitude and of such extreme significance. I take it that our deliberations in this Conference will have immediate practical value, and also, that it may well have a kind of enduring philosophical, or perhaps ethical, significance.

Certainly, one legitimate concern of many of those who deal in one way or another with the great national and natural resources of this continent must surely be quite simply to insure the possibility of staying in business. There can be nothing much wrong with that.

In fact, I think there is everything right with that; but I am equally certain that another concern of us all is to insure the possibility that our children and our children's children may stay in business: In other words, that generations to come may enjoy the advantages, for business purposes, for recreation purposes, with which nature has so lavishly endowed this continent; and that concern, however you look at it, involves us in adopting and in persuading others to adopt some kind of what I referred to as adequate philosophical or ethical attitudes.

In assuming long-range responsibilities, in the exercising not only of our technical skills but of our imaginations—a quality which has frequently been in short supply when it came to dealing with our great national resources—in holding steadily before our eyes the concept of stewardship, we not only confer benefits on our fellows but, I am inclined to think, we enhance our own stature as human beings.

As I have said, one can only take pride in being associated with a movement of this kind, and one can feel only gratitude, I think, to the Wildlife Management Institute for its program of activities in the opening of this Conference and its related meetings. I wish, certainly, to say for myself that I am extremely proud and honored to have been asked to act as chairman.

It's a great pleasure for me to be associated in the act of chairmanship with Dean Fontanna of the School of Natural Resources of the University of Michigan in Ann Arbor. Dean Fontanna is going to take charge of whatever discussion there may arise after the various papers are read, and he will, I am sure, give adequate advice and instructions to those who may wish to take part in the discussion.

Now, ladies and gentlemen, we have a very full program ahead of us this morning, and as chairman I'm going to try to efface myself as much as possible and to introduce all the speakers with whatever degree of brevity may be consistent with courtesy.

Now, ladies and gentlemen, I am very happy to have the honor of introducing to you this morning the Honorable Jean Lesage, Minister of Northern Affairs and National Resources in the Federal Government of this country. Mr. Lesage has been in his high office only a comparatively short time, but it is altogether fair and just to say that his vigorous and imaginative prosecution of the affairs of this department have already given him an admired reputation in this country.

He speaks the two major languages of Canada, his own language, French, and English, with equal facility, and I hope that he will give us a sample of both in the course of his remarks.

I can think of no one better qualified, or whom it would have been more appropriate to ask to extend a welcome to this Conference on behalf of Canada. Therefore, without more ado I am going to ask the Honorable Mr. Jean Lesage, Minister of Northern Affairs and National Resources in Canada, to welcome this Conference. Mr. Minister!

CANADA WELCOMES THIS RESOURCES CONFERENCE

THE HONORABLE JEAN LESAGE
Minister of Northern Affairs and National Resources, Ottawa, Canada

Ladies and Gentlemen: It is a great honour for me to welcome to Canada the Twentieth North American Wildlife Conference. It is also a personal pleasure, because I had the privilege of attending the Nineteenth Conference in Chicago last year, and I am happy to see before me today many whose acquaintance I made for the first time at that meeting. I am looking forward to greeting old friends and to making new friends.

When I was in Chicago last spring, I mentioned to the executive officers in charge of the Conference that it had not been held in Canada since 1942; and I insisted that 1955 would be a good year to travel north. I wish to tell you, gentlemen, how much my fellow Canadians and I appreciate your coming here. I am especially happy because it gives to a much greater number of Canadians the advantage of participating in the Conference.

(*The next five paragraphs were delivered in French*)

I am especially pleased that the Metropolis of Canada was chosen and that the Conference is being held in my native province. With its diversified topography and vegetation and its numerous lakes and rivers, the Province of Quebec is the home of such a great number of wild birds and animals that it may truly be called a paradise for wildlife.

Its well-stocked rivers and lakes have established for the province an unquestionable reputation. Our forests are filled with game of which I will mention only moose, deer, caribou, hare and partridge. Among the fur-bearing animals are found: beaver, bear, mink, otter, muskrat and many more.

Feathered game is another of our important game resources. The Gulf of St. Lawrence is filled with sea birds of all kinds which provide a favorite territory for ornithologists. Brant and other ducks are plentiful everywhere. Sixty thousand greater snow geese live for five months of the year on both shores of the St. Lawrence, east of Quebec City and on the islands opposite Montmagny-L'Islet, providing a unique attraction: in fact, it is the only flock of this subspecies in the world.

I once had the wonderful experience of witnessing the arrival over Cap Tourmente of migrating geese flying at a height of a few thousand feet and in such numbers that they clouded the rising sun. I then saw them circle down gracefully to alight on beaches overgrown with reeds among which they fed; this was a unique and never-to-be-forgotten spectacle indeed!

Yes, Quebec can be truly considered as a paradise for wildlife which is given adequate protection under satisfactory conditions, thanks to the efforts of the government authorities and to the co-operation of our sportsmen. They understand that such an important natural resource can be preserved only by means of a well-organized conservation policy which is adhered to by all.

Ladies and gentlemen, I wish to mention, at this point, that we are now meeting in the home city of one of the oldest associations of its kind in North America, the Province of Quebec Association for the Protection of Fish and Game. That Association was founded in 1859; and at the age of 96, if I may use a Scriptural phrase, "its eye is not dim, nor its natural force abated."

This Conference, at the age of 20, may be considered to be in its prime. We generally assume that a man who has completed 20 years of life has at last reached the age of discretion, but perhaps I should not carry the comparison too far. Certainly the Conference was a very lusty infant when last it visited Canada 13 years ago. Not only has it increased mightily in wisdom and stature, but it shows every promise of continuing to grow and flourish for many years to come.

This Conference is the successor of the American Game Conference which began in 1914, but its roots go much farther back than that. It has developed out of a long-established community of aims and principles in our sister nations, Canada and the United States. In that sense the Conference is older than most of us here, and we may be certain that it will long outlive the youngest among us.

It may be appropriate if I can glance back half a century, to see how far we have advanced. Fifty years ago we were still to a great extent inspired by the pioneer spirit, the urge to fight and subdue Nature. That was a wonderful spirit in the early days when man's foothold on the many frontiers of this continent was still insecure. At the turn of the century many thoughtful persons knew that the time had come to work with Nature rather than to fight her, but the new gospel—which we now call conservation—was making only slow and difficult progress.

In the wildlife field especially, it was difficult to get the word "inexhaustible" out of people's minds. Comparatively few persons, for instance, realized that the vast flocks of ducks and geese which they then saw might disappear unless positive action to conserve them was taken, not only by state and provincial governments, not even by national governments acting singly, but by the governments of nations working in harmony. Fortunately for us, those few clear-sighted individuals were strong and tireless fighters. In 1916 they won their fight, when the Migratory Birds Treaty was signed.

The Migratory Birds Treaty is now approaching its fortieth year. In the modern world it is rather unusual for a treaty to survive unchanged for forty years. The survival of the Migratory Birds Treaty is due partly to its own merits, although it has its weak spots, but more to the spirit of friendly cooperation in which both sides have lived up to it.

Friendly cooperation is, indeed, the keynote of relations between the United States and Canada in all wildlife matters. This Conference itself is one illustration of that. In the field, the annual joint survey of breeding waterfowl is perhaps the world's greatest example of regularly repeated scientific team-work by a large number of experts of two countries.

In bird banding, also, our countries work in close harmony. It is a happy coincidence that in 1955, when Canada greets this international Conference, we celebrate the fiftieth anniversary of bird banding in Canada. On September 24, 1905, the late James Henry Fleming placed his Band No. 1 on the foot of a robin in Toronto.

From the modest beginnings of 50 years ago, bird banding has become a powerful instrument in the hands of the wildlife biologist. The recorded number of birds banded in Canada will, by the end of this year, approach one million. One young biologist of my Department alone banded 8,000 blue and snow geese in one season on their breeding grounds near the Arctic Circle. Many other scientists of both Canada and the United States have traveled into remote parts of the North to locate the breeding grounds of waterfowl and to trace their life history by banding scores, or hundreds, or thousands of young birds.

The idea of sanctuaries for wildlife is another in which our countries have advanced on parallel lines. Canada's oldest waterfowl sanctuary, at Last Mountain Lake in Saskatchewan, was established in 1887—sixty-eight years ago—and still flourishes. Today we have more than seven hundred areas set aside in Canada for some form of wildlife protection. Their total area is more than a million square miles; they are found from the Atlantic to the Pacific, and from the International Boundary to the farthest north Arctic islands.

In my remarks so far I have covered a span of time which exceeds the personal memories of most of us. It is worth while to look briefly at the increased tempo of developments in Canada since the Conference last visited this country, in 1942.

In the federal sphere, the Canadian Wildlife Service was created in 1947. Its staff of full-time technical officers has expanded from six to twenty-two in the last seven years. In 1947, our knowledge of wildlife in the Northwest Territories, where wildlife is the principal natural resource for the natives, was sketchy and discontinuous; today we have seven biologists who carry on field work in the North during the proper seasons.

During the last dozen years several of the provinces also have expanded their wildlife services. Cooperation between federal and provincial wildlife administrations is excellent, and provincial services stand high in the esteem of federal wildlife officials.

In 1942 wildlife management was a subject almost unknown in educational institutions in Canada, and for several years thereafter it was customary for Canadian youths intending to enter a career in wildlife management to take their academic training in the United States. Today very fine courses in that field are given by Canadian universities, and some of the best wildlife biologists in the government services come from the ranks of their graduates.

A minute or two ago I was speaking on the theme of cooperation: perhaps I should now put the idea into practice, and cooperate by closing my remarks so that you may proceed with the business of the day. That is one point, at least, on which we can have no difference of opinion. No doubt, during the next three days, other occasions will arise when the sparks will fly; but we must not forget that sparks are often the origin of light in dark places.

With that thought in mind, I wish you a vigorous and successful meeting.

DISCUSSION

[Chairman Trueman, speaking in French, thanked Mr. Lesage for his interesting and very kind welcome.]

CHAIRMAN TRUEMAN:

Now, we shall proceed with the other papers which we have for this morning's session. It gives me great satisfaction to introduce to you at this time Mr. Paul M. Dunn. As you may know, Mr. Dunn has only recently joined the St. Regis Paper Company of New York, which has large operations in Canada. He was formerly the Dean of the School of Forestry in Oregon State College, and before that Dean of the School of Forestry, Wildlife, and Range Management in Utah State College, Logan, Utah.

It gives me great pleasure to call upon Mr. Dunn to speak to us at this time. Mr. Dunn!

OUR RESOURCE ESTATE: WHY CONSERVE IT?

PAUL M. DUNN
Technical Director of Forestry, St. Regis Paper Company, New York, New York

As I read the proposed title for this paper, I wondered whether it was intended as a question or a statement of fact. I decided that the thought behind the words might be akin to the thinking back of one of the periodic assemblies of our church, when the theme of the meeting was announced as "A Reaffirmation of Faith."

The major assets of each and every nation of this great world are its resource values: the human and the physical. The wealth and the development of any country depend on the kind and supply of physical or natural resources, and the ability of her people to develop, manage and utilize them, wisely. For the purpose of this paper, I shall refer to the natural resources group, only; those usually called the organic or the renewable resources.

Over the past many years, the people of communities, states and the nation have capitalized on the wealth of the land, the productivity of the soil and of the climate, through annual or periodic crops. Currently, the situation has not changed, and I feel certain that the future will continue to be the same.

The words "conserve" and "conservation" have broad and varied meanings; it is often a matter of interpretation. Interest in the topic is evidenced by the controversial points of view. One can find many ideas on the subject appearing in articles and books, almost daily. Each expounds certain aspects of the words and their usage by various persons and groups, teachers of English, biology, and economics, representatives of public agencies, private companies or associations, and laymen; all experts in their specific fields of endeavor.

If you use the Latin derivation of the word Conservation, it means "To Keep Guard." However, in Spanish, *Conservacion* means "With Service." I prefer the latter.

A recent definition that appealed to me is: "Conservation might be likened to a double-lane highway to the far end of preparedness; one lane supporting the military organization by maintaining the required supplies of food, fiber, lumber and materials; the other supporting the faith and confidence of the people in democracy by making the land a more productive and pleasant place in which to live."

To whom and to what would we apply the term?

William Van Dersal recently made this statement: "Conservation has been applied to all the natural resources: wildlife, forests, grass, minerals, oil, water, food, and other materials such as forces, beings, money, electricity, energy and matter." Professor Emanual Fritz of California would include in the list, "The Nation's and the individual's time and efforts." I agree with this thought.

Around the turn of the century, our thinking on the subject stressed the "keeping the guard" idea, and it still is paramount in the minds of some people. To most of us, the word means wise use of our resource values, the restoration and careful management of the renewable values, and the establishment of a workable program that will make them serve as a whole and to the fullest advantage, perpetually.

We accept the fact that there are differences of opinion on the subject; even amongst the group here today. As someone said, "That is what makes a horse-trade"; or as the old Piute Indian said, "No like-um all men think same, then all want my squaw." However, we all will agree that we need a better understanding; not only of the basic problems, but of each other's point of view. We need information.

Owen Young, a business statesman in the industrial and natural resource field, was quoted as saying, "Facts are the least developed of our natural resources." Certainly that is true. Essential facts are frequently overlooked or misconstrued in the consideration and the debate of our natural resources, particularly when they are related to so-called conservation issues. This becomes doubly true when we advance the basic American theory that these resources should be put to wise and conservative use through our private enterprise system; and, wherever possible in the public interest, private ownership as well as private use should be promoted in order to build a productive, taxpaying economy. The right kind of facts are important. Josh Billings once commented in this vein: "It ain't that people are ignorant, it's that they know so much that ain't so."

We are here today to take a new look at some of the oft-repeated facts, review the basic problems, and listen to reports of progress.

I believe that it is safe to say that we could all agree that a sound conservation policy for any nation would contemplate the use of its natural resources with due regard for the present public need, the rights of posterity, reasonable preservation of natural beauty, and the long-term productive requirements of the people for food and other materials. The solution of problems as they arise should, in my opinion, be approached from a business viewpoint and not from a selfish or a provincial standpoint. We should accept a certain amount of sentiment in the operation, but not to a major degree. Solutions should be based first of all on what is best for the whole country over a long term without doing violence to local economies which conform to historic patterns. I can see some minimizing of the importance of national borders, even.

Multiple use of the public lands which encompass these natural resources should be the watchword; *i.e.,* timber, forage, water, wildlife and recreational uses. Local and state governments should have a prominent part, along with the users, in formulating and regulating the productive use of these resources. While private enterprise is endowed with certain automatic controls, the ultimate promotion of good conservation practices must be accepted by all private resource owners.

The ownership of the lands does create certain problems, but they are not really paramount. Ownership just identifies the major stockholders and places the responsibility. The objective should be to administer and manage both types of ownerships, public and private, with all single-purpose programs modified in most instances. Only in this way shall we leave our descendants a fully productive land in which they can continue to make and enjoy a living rather than one pock-marked with extensive tracts of unmanaged cut-over or uncut areas.

Full and complete information is essential to the best and most complete development of the resources as well as proper utilization. This is particularly important in view of our national defense measures. In respect to this point, I believe that we agree that we are thinking of a hemispheric defense. In the opinion of many, the Western Hemisphere could be self-sustaining from the standpoint of strategic materials. If this is a probability, we should be certain, and then protect our resources accordingly. Just as the matter of a dependable national inventory of our forest resources is deemed essential to resolving many of the pertinent questions, so it is important that an early attempt be made to re-classify many of the public lands. Over the period of years, since many of the current uses were designated, the situations, both economic and social, in many parts of the country have undergone a change. The demands of both public interest and private industry are not the same today that they were 50, or even 20 years ago.

In answer to the question WHY? Here are some answers:

Patrick Henry had a second vital statement: "Since the achievement of Independence, he is the greatest patriot who stops the most gullies."

Bernhard Fernow once said: "A nation may cease to exist as well by decay of its resources as by the extinction of its patriotic spirit.'

Ex-Governor Sherman Adams of New Hampshire stated: "The practice of conservation is an act of patriotism, and the understanding of it, the preaching of it, and the contribution to it, are parts of the fundamental duties of a citizen of a free society."

Exploitation is not the only means of resource destruction or depletion. By non-use, the values may lose their utility. Hoarding may contribute to improper conservation. The problems associated with the use of a resource with the resulting non-use of certain phases of the resource must be recognized. This non-use may not necessarily create a waste of values. The deferred capitalization may not be unwise as it may be very necessary in respect to national development and national welfare. If we, as a people, wish to maintain and perhaps improve our standard of living, we must appreciate the probable cost. Two disastrous world wars have been fought and won within the past 40 years. Our natural resources were important factors in the successful ending of those conflicts. All of us must realize fully the vital importance of the necessary resource materials and the need for their perpetual conservation.

Nature generally maintains an equilibrium between the land and the living things that it produces, whether animals, grass or trees. Man, by proper understanding the capacities of the soil can utilize fully the various crops. Periodic accounting or checks will determine the maximum utilization of the various organic resources. A game census, a timber growth check, a grazing survey or water measurement will provide adequate facts for the periodic harvests. It is soon evident if the owner is disregarding the land's resources potential. Several very vivid examples in past civilizations, namely China and North Africa, have told their stories of over-use and land abuse.

What is needed is a reorientation of thinking, and a re-examination of the obligations of a citizen to his state. It would appear that at times there has been a misunderstanding of the meaning of democracy. It does not mean irresponsible freedom. It means a way of life in which the freedom of the individual is secured by his unselfish support of the whole group, that is, of the state. One of the most important functions of this support is the proper stewardship of natural resources by the individual for his own welfare and with others for the welfare of the nation. However, nothing but chaos can result from the efforts of the various individuals working his or her separate scheme of resource utilization. It requires, instead, full coordination of efforts by all. This can be brought about by proper planning and education.

There is no question in my mind that the solution of our many problems in the field of conservation rests in the realm of education. Basically, the problems relate to a proper appreciation of our various resource values in respect to our present and future needs. That appreciation can best come through better understanding; it is a matter of education of all the people and chiefly of our youth. The young people are unusually responsive and will benefit most from the conservation measures.

So, the answer to the question is this: The rise and fall of certain Nations was closely correlated with the treatment accorded their natural resources. We have concrete evidence of outstanding development on the North American continent during the past three centuries due to bountiful resources. The forecasts of population increases, public land-use trends and industrial expansion within the next 20 years, make it imperative that we conserve our Resource Estate, so we and our children may maintain the values and have the opportunity to continue to enjoy our currently high level of civilization.

PROGRESS IN SOIL CONSERVATION [1]

J. S. RUSSELL
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First of all, let me say how much I appreciate this opportunity to discuss with you some of our mutual problems. The over-all problem of conservation is one that concerns all of us whether we live in the city or on the land, whether we live in Canada, in the United States or elsewhere on this hemisphere, and whether our primary interest is in wildlife or in conservation of the soil for future agricultural production.

And let me also bring greetings to this meeting from the soil conservation groups in the United States, although in organizations like the Soil Conservation Society of America, the mutual interest on both sides of the United States-Canadian border has long been recognized.

The last 20 years has been a period of real progress in conservation and particularly in soil conservation. This has been particularly true in North America with its six millions of acres of land ranging from Arctic to Tropic in its climate and involving a continent in which settlement and development and expansion have caused thoughtful persons to have real concern for the preservation of natural resources.

[1] In the absence of the author, this paper was read in title.

The first big step in progress in the last two decades has been that of recognition that we must take positive steps to protect these valuable resources in the form of soil fertility, forests, water resources and wild life.

We have begun to realize that if we were to avoid the pitfalls that have befallen the ancient civilizations in Asia and the Middle East where neglect led to loss of fertility and decay, then we must take positive action. Let me say here and now that if any doubt the need for protecting soil, just let him fly over Saudi Arabia and other areas of the Middle East and southern Asia as I have a couple of times within the last three years and I am certain that he would be convinced of the need of conservation and control of soil and water. He could see the tremendous pressure of population on the land which is responsible for some of the unrest among the teeming millions of Asia.

There is need for more attention to conservation throughout the world, although the visitor to some areas is impressed with how well the European farmer takes care of his land and how much the Japanese farmers have learned about terracing to make use of land and water even on the steep mountainsides. But the need of better rotations and better land use are quite apparent in countries like India where food is a problem.

This all goes to prove how much this whole problem is of world-wide importance, how necessary it is to protect our natural resources if we are to make them of greatest use to the people. It is not a matter just of local concern, but of world importance. The fact that organized soil conservation programs now exist in 40 countries would seem to indicate that the problem of conservation is being attacked on a world-wide basis. We have a real opportunity for us here in North America to demonstrate that we can handle our resources wisely enough to enable the population to have a high standard of living. We want also to enjoy the wildlife and advantages of recreation and at the same time make sure that our resources will be adequate a few years from now to feed our increased population.

The most important step in our progress toward soil conservation was the formation of the soil erosion service in the United States government and the setting up soon afterward, just 20 years ago, the Soil Conservation Service. We owe an undying debt of gratitude to Dr. Hugh H. Bennett, the father of the soil conservation movement in North America, for his leadership in getting this work started.

And we owe a lot to the work over the years and the leadership in the over-all field of conservation to Jay N. Darling, a man with whom

I have had the privilege of being associated on the same newspaper for many years.

In consideration of the progress of soil conservation in North America, suppose we examine the status of this work in the United States.

Since most of our conservation problems have been created by man, we must depend on man to correct some of his own errors. This is not a matter of the Federal Government assuming responsibility for soil conservation, but rather of setting up the machinery and providing technical assistance for landowners to do it themselves.

Progress the last year or so in the United States consists of recognition that water and soil conservation and control go hand in hand. Legislation making watersheds a unit for application of an over-all control and conservation program will be helpful, too. U. S. legislation also has made possible the financing of water and soil projects with federal funds under certain conditions.

More than 80 per cent of all the farms and ranches of the United States, covering 90 per cent of the country's farm lands, are now within organized soil conservation districts—Missouri and California are lagging notably in the organization of local districts.

Needed conservation treatments as determined by scientific study of each farm, are now being used on 34 per cent of the nation's privately owned agricultural land.

Here are some of the figures on the status of the soil conservation program in the United States as shown by the report of Administrator Don Williams of the Soil Conservation Service on last July 1:

Soil conservation districts organized	2,618
Area in soil conservation districts	1,441,495,683 acres
Conservation surveys completed	455,896,394 acres
Range surveys completed	22,376,626 acres
Farmers and ranchers applying conservation	1,454,287
Land on which conservation was being applied	423,690,086
No. of farms on which all conservation work was completed	162,336

Major soil and water conservation practices which had been applied, to safeguard land under different uses, are as follows:

CROPLAND

Contour farming	28,686,930 acres
Cover cropping	13,026,851 acres
Stubble mulching	29,523,163 acres
Strip cropping	8,389,187 acres

GRASSLAND	
Conservation use	71,647,598 acres
Deferred grazing	16,460,089 acres
Rotation grazing	22,046,979 acres
Seeding	18,352,097 acres
WOODLAND	
Stand improvement	9,424,474 acres
Tree planting	1,548,850 acres
Windbreak planting	18,390 miles
Woodland protection	26,701,877 acres
WILDLIFE AREAS	
Hedgerow plantings	69,966,988 linear feet
Marsh improvement	68,235 acres
Wildlife area improvement	2,472,650 acres

In some parts of the country soil and water conservation work was seriously handicapped by drought during the year. Approximately 13½ million acres of cultivated land and 4,800,000 acres of range land in the Southern Great Plains were damaged by wind erosion in 1954. Studies of the blowing area revealed that the long-range conservation program should be speeded up and that some substantial changes in land use should be made. It was estimated that about 7 or 8 million acres not suitable for cultivation should be converted to grass and carefully maintained as permanent grazing land as soon as possible.

Now, just a word about the philosophy and the type of organization for getting the conservation job done.

As long as the farmer thinks that the conservation work belongs to the government, he does not accept the responsibility for it. When landowners accept the responsibility themselves and they look at government assistance in soil conservation as merely helping them do the things they cannot do for themselves, then the job will be completed.

The outstanding organization for soil and water conservation that has been developed on this continent or in the world is the soil conservation district, which I mentioned earlier. The district is a local unit of government organized under state law but under complete control of the farmers themselves.

Not all the soil and water conservation jobs of the continent can be treated on an individual farm basis and so the techniques of getting conservation done becomes a public responsibility.

There are several approaches other than those of preventing erosion. Drainage is a part of the land-use program and irrigation is bringing additional land into production and improving production on that imperfectly watered land. Many areas will be brought under irrigation in the next several decades in North America.

A large acreage of our most productive cropland lies in the valleys of our rivers and small streams. Each year much of this land is subject to floods. Not only do floods damage crops, but frequently destroy property and even cause deaths. Surveys of flood damage in the upper reaches of streams in the United States indicate that 70 per cent of all the damage caused by floods is on the lands surrounding the small streams. Flood prevention work to alleviate damage along small streams and the uplands is properly a part of the conservation program. The work started in some of the watersheds will bear close scrutiny, and results in some areas are very encouraging. Demand for agricultural products on this continent is expected to increase 50 per cent in the next 25 years.

If we are to meet these demands for food and fiber, we must depend largely on increased per-acre yields. Increased per-acre yields call for the rapid application of soil and water conservation, good soil management, and the use of all the latest technological improvements in agriculture. This means that we must speed up our programs of soil and water conservation if we are to remain a "have" instead of a "have not" continent.

The fact that most of the comment has been regarding soil conservation program and projects in the United States is because of the enormity of the problem in my own country. Techniques developed there contribute to an approach in other countries in North America, too.

Mexico, for instance, very badly needs to protect natural resources as visitors to that nation can readily see. But Mexico is taking some steps to save the soil. The federal department of soil conservation has responsibility at this level, but 17 large units have been set up for the carrying out of the community approach, being used for both operations and education. Several private organizations exist for furthering interest in and actual steps toward soil conservation.

In Canada the Prairie Farm Rehabilitation Administration operates in Manitoba, Saskatchewan and Alberta provinces while provincial soil conservation departments in Quebec, Ontario and the Maritime provinces assist farmers, forest owners and watershed operations in meeting conservation problems. In Ontario alone the 15 watershed associations are helping meet the problem, and the nearly 11,000 miles in conservation authorities provide for surveys to be made when two-thirds of the citizens in a watershed approve a program.

In the prairie areas are a number of community pasture and grazing associations which are governed by local advisory committees.

The Canadian conservation association has campaigned for centralization of conservation projects into a national program.

Now, I would like to add just a few words about what we have been able to do in the United States to further the progress of soil conservation and also some obstacles to further progress.

We certainly could use some coordination and consolidation and more stability in our approach to conservation.

The progress we have made has been in spite of much confusion, contradiction and reorganization—and threats of more.

Sometimes we seem to spend so much time feuding, lobbying, politicking and washing our dirty linen in public that we forget to tackle the job of conservation as aggressively as we could and should.

The argument as to whether the extension service can do the conservation job better than the Soil Conservation Service working through the soil conservation districts has touched off some hard feeling both in the Congress and throughout the nation. The reorganization of the Soil Conservation Service upset progress for the time being and no sooner was the work back in the groove again than further reorganization was suggested. The conservation people and the army engineers fail to see eye to eye with each other on the matter of flood control. We have the folks who want the Federal Government to handle soil conservation while others think it is a state responsibility. The farmers and the men who seek recreation in hunting and fishing and favor more emphasis on wildlife sometimes fail to agree. The advocates of forestry sometimes feel that reforestation and tree planting generally needs a better break—and so it goes.

But in spite of all this, we have made real progress in conservation and we will make further gains.

We can point with pride to what we have accomplished such as:

1. The amount of land now in soil conservation districts.

2. The use of the soil district program to make soil conservation work.

3. Use of the Soil Conservation Service to give technical help in the agricultural conservation program (ACP) thus helping resolve what might be a duplication.

4. Recognition of the soil conservation in connection with loans under water facilities act and also for computation of federal income tax payments.

5. Adoption of small watersheds as units of applying water and soil conservation and control practices.

6. Recognition of the need for evaluation of land capabilities—so that each acre can be used for what it is best adapted.

7. Forestation as part of the land-use and conservation program.

8. We are beginning to learn that it pays to take some of our land out of crop production, and this in turn may make more land available for game cover—in other words, the farmers and the sportsmen do have some mutual interests.

9. Farmers are finding out that they can't afford not to adopt conservation farming methods. They get better yields even though they may cultivate fewer acres.

10. No program for agriculture ever has had wider acceptance among city folks.

11. Both farmers and urban residents can see some sense in a program that conserves not only our soil but our fertility against the day when, with increased population, we will need all we are producing now and more, too.

And so if we who believe in conservation are inclined to get discouraged because we aren't getting along faster and because we let minor differences of opinion get in the way, let me say that we have made marvelous progress during the last 20 years. Much of this has come about within just the last few years.

We have a long way to go but we must accept the challenge for the future.

Certainly based on our past experience we can and will proceed to practice conservation at a greatly accelerated rate in the years just ahead.

TWENTY YEARS OF PROGRESS IN WATER MANAGEMENT

EDWARD A. ACKERMAN
Resources for the Future, Inc., Washington, D. C.

The changes which have taken place since 1933 in water development and management make a tremendous and moving story. They form a fitting chapter in a description of the growth of enterprise and industry on this continent. This period of about 20 years has brought some magnificent engineering works, and those works have been accompanied by a growth of organization and technology.

A good way to understand readily the progress which has been made in 20 years is to recall the state of our development and knowledge in the early 1930's. In speaking of this, I shall refer primarily to the United Staes, with which I am most familiar.

In the late twenties and early thirties a major share of existing water facilities had been constructed strictly by private enterprise. The full financial and organizational resources of government had not been brought to bear generally upon water management problems.

Several important parts of the United States were covered with eroding lands which loaded streams with sediment. Reservoirs constructed in some areas, as on the Carolina Piedmont, were filling rapidly with sediment. Not a few already had very little capacity left. Furthermore, in the southeastern part of the country there was serious question as to whether any more reservoirs could be constructed because of malaria.

Multiple-purpose construction, in which not only major purposes but minor functions could be served, was hardly recognized. It is true that Boulder Dam on the Colorado had been constructed as a multiple-purpose project. Irrigation and power development were both foreseen in the planning of this major new work. However, even dual-purpose planning was not accepted elsewhere in the nation, and both planning and construction were mainly on a single-purpose basis. This was no less true of works on a major stream like the Mississippi, with its pressing flood control problems, than it was of small tributaries, with single purpose power developments by private utilities.

It was even possible for a government to undertake a major development in which it prohibited by law multiple-purpose use of river control works. That actually happened on the Muskingum River in Ohio. Twenty years ago the planning and management of water resources on a basin-wide pattern was an unaccepted principle. A project-by-project approach was still dominant, although surveys on a full-stream basis by the United States Corps of Engineers were getting under way. Furthermore, where systems of dams had been planned, as on the Tennessee, extremely conservative developments were the recommended course. Those plans could and did foreclose upon the potentialities of the future.

A conservative view was taken of the potential demands which might be served by the development of streams. We had little concept as to what the size of an efficiently organized electric utility system amounted to, and we were content to allow more than half our farms to be without electricity. Indeed, most farms were financially discouraged from seeking electricity, even if they were interested in it. Finally, the need for ground-water management and the potentialities for the use of irrigation water in humid areas were largely unconsidered.

The picture is far different today. Water development in the United States today is really big business. Between 1946 and 1953 the Federal Government alone invested approximately five billion dollars in the dams, reservoirs, canals, levees, electric transmission systems and other river works. During the same period hundreds of millions more were invested by private enterprise and by local governments or government agencies. During these 20 years we have seen the erection of such impressive works as the Grand Coulee Dam and the associated Columbia Basin project in the state of Washington; the Central Valley project in the state of California; the 20 dams of the TVA in the Tennessee Valley States, and the great dams on the Missouri. Canada has paralleled the United States in the striking Kitimat development in British Columbia. During the same period we have seen some very helpful changes in the agricultural landscape of the United States and Canada through various forms of soil conservation. While my country may never again reach a day when many of its streams will run clear throughout their length, many solid contributions have been made toward sediment control and assistance in solving local flood problems. In addition, the use of electricity has been made available now to 92.3 per cent of the farms in the country (June 30, 1954); and an electrical transmission and generation system of more than ten million kilowatts capacity under a single regional management soon will be in existence.

However, it is not of cubic yards of concrete, miles of terraces, or miles of aluminum wire, or other physical things that I most should like to speak. To me the most important part of the story lies in progress which we have made in organization, ideas and techniques. The works themselves represent something more than engineering accomplishment, interesting and significant as our construction progress has been.

If I were to note any single accomplishment deserving of special mention in the twenty-odd years since 1933, I should choose the acceptance of multiple-purpose planning and management. The principle that surface waters should be planned for and managed with recognition of all the purposes they can serve now seems accepted beyond any professional doubt. I know that many of you who are acquainted with present-day water problems will think of imperfections in practice. In many instances planning does not give adequate weight to the so-called secondary purposes (all too often recreation, fisheries, and wildlife). Nonetheless our present approval of multiple-purpose planning and management is an enormous step forward from our situation of two decades ago. If the practice is not yet satisfactory from the point of view of recreation, fisheries, and wildlife interests, future improvements are questions of degree rather than the establishment of an entirely new principle of development.

Closely associated with the development of the multiple-purpose view has been basin-wide planning, basin-wide water control, and the integration of water development with that of other resources. Of the three, basin-wide planning now is probably the best understood. We also understand the need for considering water and land resources together. After the experience on the Tennessee and on other streams, no serious student or practitioner of water development would question the gain from basin-wide planning and development. We have a number of projects whose values are almost twice as great because they were planned as part of a full system rather than as a single facility.

In the United States we now also are emerging into an era when basin-wide planning includes not only the surface waters but also the underground supply. In some areas adjacent to the ocean even the waters of the sea are being brought into the complete planning which ultimately should prevail.

Within this period also we have emerged to the point where we think in even broader horizons than basin-wide planning for our water. At least in western United States we now think in terms of inter-basin diversions and whole chains of water exchanges. The Colorado diversion into Southern California, of course, was the first important one. But now the Trinity River diversion from Northern to Central California seriously is being considered, and ultimately the waters of the great Columbia may fall into service outside its basin.

We now have come to a time when water planning and development must be and are approached on an international basis. The United States and Mexico have commenced the development of the Rio Grande, while Canada and the United States have finally commenced the long overdue St. Lawrence River project. The international development of the Columbia also is coming under consideration, although we have little progress to report there as yet.

Among the gains which may be cited in water development patterns during this 20-year period is acceptance of the idea of stewardship. By this I mean the principle that resources be developed and used in a manner which will preserve their usefulness for other years and other generations. An important part of this is not foreclosing upon future potentialities. The incomplete use of a power site is a good example, although many examples might also be drawn from the preservation of wildlife habitat. Here and there we find a throwback, as in the present substitute plans for the Hells Canyon developement in the Pacific Northwest of the United States. But in general I believe that both public and professional opinion has a much broader understanding of the idea of stewardship in water development than at any time before in the history of the United States. This must be counted a great gain since it can be a strong support to efficient development.

One other idea which has emerged and which has been tested during this period also deserves mention. That is the idea of demonstration. In any field where progress depends not only upon the action of many units of government but also upon many companies and millions of individuals, the idea of leadership through physical demonstrations is a valuable one. It was first used in the United States in agricultural extension to pass along knowledge of improved planning practices to farmers. The second significant application of the principle came in the activities of the Forest Service.

During the 1930's application of the principle of demonstration was extended into the water field through the programs of the Tennessee Valley Authority and the Soil Conservation Service. The TVA demonstrations are particularly interesting because they have embraced water development, land treatment and associated regional development in their most complex aspects. They include, among others, the large-scale organization of water control under a single water master, the economies of scale afforded by a large integrated power system, the organization and management of reservoir-side parks, local flood control, and mineral development. Through these demonstrations we have found that public leadership under a democratic system is capable of bringing out the full potential of a given set of resources in the public interest.

I have said little thus far of scientific developments which can lead us into an interesting technical future. We may be on the threshold of a time when we shall have important new tools for dealing with water supply problems. Among these, cloud seeding already has achieved a certain amount of fame and notoriety. We have as yet a very inadequate knowledge of the physical principles involved in artificial precipitation, but we are likely to hear more of it in the future. The best water-control planning and management of the future may well be three dimensional. While weather control may never have universal application, it seems reasonable to believe that it may have at least local importance. In those situations where it does become useful, we may have to think of new types of storage, and alterations in our pattern of reservoir construction.

Most of you undoubtedly also have read of experiments on the desalting of water from the sea or other saline water bodies. The cost of desalting already has been reduced markedly. While our present rate of experiment may not be designed to yield quick results, our continuously widening knowledge of chemistry and improving techniques of producing energy give hope that desalting eventually may be applied practically. Even more than cloud seeding, it may have limited local application. However, those local applications may be extremely important. Think of the meaning of applying such a source of supply to the water requirement problems of Southern California, or the Gulf Coast of Texas, or parts of Mexico.

There are other tools which are coming into our knowledge, perhaps less spectacular, but still with interesting possibilities for great usefulness. Consider, for instance, a recent proposal for using tritium as a tracer in determining the movements and consumption of ground water. Tritium, you know, is an isotope of hydrogen and is produced naturally through the action of cosmic rays. It comes to earth in rain, and enters the ground water supply with infiltering rainwater. It decays with time and thus may be a means of determining the age of ground-water supplies which come into use. The process of dating such water in turn may enable us to adjust the exploitation of ground water to the available supply.

Some of our discoveries in this 20-year period are still more prosaic and less exciting to the imagination. Nonetheless they are of great importance. I mentioned that we by no means were certain in the early thirties that extensive reservoir developments were possible in southeastern United States. We were equally uncertain about such possibilities anywhere else in tropical or subtropical areas. Malaria was a deathly companion to water impoundments. Had malaria control not been possible by economical means, water development in these areas would have taken another and much less profitable course than it did. Fortunately, TVA discovered not long after filling its first important reservoirs that a combination of reservoir-level fluctuation, control of banks vegetation, and insecticide use gave complete malarial protection at very low cost. We now know beyond any doubt that it is just as practical to have a reservoir in a malarial area as anywhere else in the world.

As our knowledge and our techniques grow, our problems also mount. Foremost among these are the increasing demands for water and the sharp localization of those demands. In the United States and elsewhere in the world, human beings are becoming more and more urban in their habits. Not even the very grave threat which comes with our new-found methods of warfare has caused any slight reversal in the trend toward urbanization. Urban people are water consumers, not only in their luxury needs, as for air conditioning or garbage disposalls, but also for the industries from which they draw their employment. Industries are two-fold problems: for the disposal of the waste which many of them generate in their processes, and for the quality and quantity of water which they need in carrying on their operations. Quality requirements are becoming more and more strict, not only as to mineral content but also as to temperature. Those who plan for and manage the water supply of a great urban industrial area have a complex task on their hands.

Hand in hand with the growth of urban demands are coming increasing rural demands for water. The situation in western United States, where billion-dollar irrigation programs are not thought extraordinary, gives us some indication of the shape of things to come. Within the last 10 years, the potentialities for irrigation agriculture in eastern United States have leaped into prominence. Now in eastern United States suppliers of irrigation equipment have found it difficult for the last two or three years to keep up with the demand for their products. And the rise of irrigation has been imposing enough to start a number of serious investigations of the adequacy of state laws on water rights. So long as crop growing in the soil remains an important source of good and industrial raw materials, we may expect increasingly greater attention to be devoted to irrigation both east and west. It is an old remark that water is the best form of fertilizer. As the nation fills with people, and as our crop varieties improve in their productiveness, the truth of this adage is becoming more and more appreciated.

Our problems are not limited to those of demand and supply. In no small way, they relate to organization for development and management. In addition, there are some philosophical problems which we have to solve before we can ever have effective national policy. An excellent example of the latter is the salmon-versus-dams story in the Pacific Northwest of the United States. Here we appear to have made a choice in favor of developing a resource to produce a service of great immediate value, but still one of limited life. This is the electric power. At the same time we have foreclosed on another resource, the salmon, which presumably would be available in perpetuity, although it now is less valuable than the power. While the reasons advanced for the choice in the Northwest are good, I am inclined to believe that a question of this kind needs much deeper thinking than we have given it before we settle upon such a policy as a matter of general practice.

Another problem which we have not settled, and which also requires some philosophical exploration, is what I have called the compartmentalizing of our water resources. This in part arises from the distinct incompatibility between the Federal political system developed in the United States, and the continent's physical geography. We have divided our area into 48 states, but there are only a relatively few major river basins, and therefore major sources of water supply. As water supply quantities become critical, the tendency is to divide the available supply among the states by negotiation and compact, as in the case of the Colorado. But such decisions have led to long-continued and bitter conflict, and actually have discouraged efficient development and use of the water resource, when considered from a regional or national point of view. I know that you are familiar with the controversy concerning the proposed Echo Park Dam and the Dinosaur National Monument. Part of the origin of this bitterly fought issue lay in the compartmentalizing of the waters of the Colorado.

Our most serious problem of the moment, I believe, lies in the organization of planning and management on a large-scale basis for the increasingly complex task of efficient development of water resources. To complete the task of water resource development of the United States, we eventually may require an investment as high as two hundred billion dollars. This sounds like an astronomical sum, but when one considers all of the small and large works which will be involved, I do not consider it an overestimate. But we have organizational disputes in the center of the public stage at present upon which a decision one way or the other can make a difference of hundreds of millions of dollars in the costs of these developments. Those decisions also can make a great difference in the rapidity with which we proceed and in the sureness and fairness of the results. Yet we continue to temporize and to depend on outmoded forms of organization.

For any specialized group such as this, whose main interest touches one aspect of water resource development, these problems make the future difficult to foresee. Being interested in the things which are of Nature, you tend to view the good things of the future in terms of the good things of the past. You are interested in the preservation of a natural environment as Nature has produced it, and that is sound procedure. However, insofar as water resources and their development enter your planning for the future, it may be wise to think in somewhat less orthodox terms than in the past. For the United States at least, ever tighter control and ever greater utilization of our available waters are certain to prevail. I believe this can be a source of strength in developing or preserving other resources, like wildlife. That is where good organization and good planning come in. For the best use of all resources as they relate to water, we need still broader concepts and practice in multiple purpose planning. Toward this end we need the active interest and support of everybody.

DISCUSSION

VICE CHAIRMAN FONTANNA: Dr. Ackerman has handled a very difficult subject beautifully. Nevertheless, somebody might want to say a word, add something, or disagree with something he said.

MR. RICHARD DORER [Supervisor, Bureau of Wildlife Management, Minnestoa]: I was deeply interested in what Dr. Ackerman said, and I think it's marvelous that we have someone looking after our water resources and developing their fullest utilization, but I want to tell you what is going on in the State of Minnesota.

I wanted to wait until Mr. Russell had completed his remarks. Unfortunately, he's not here. And then, after Dr. Ackerman finished, I wanted to combine two things: What the SCS and PMA are doing to Minnesota's resources, and what we're doing to combat the destruction of those resources.

Minnesota is known as the "Land of the Lakes." If you travel over it in an airplane at 5000 feet, your eye is greeted by one vast expanse of water; but today, because of draining, we are practically being forced to the brink of agricultural and industrial suicide.

I know that we had difficulty in convincing those at the Wildlife Federation Conference that the PMA was carrying on one of the greatest destructive programs so far as water is concerned, of any that has ever been carried on in the United States. At the present time Minnesota's wetland areas are being drained out by government subsidies and with the assistance of exploiters and opportunists at the rate of 32,000 small water areas per year, while other states are spending billions of dollars to build up an adequate water supply for their expanding populations and for their expanding industries. Our water is being squandered. This is the most serious problem in conservation with which we have ever been confronted.

Finally, as a last resort we have organized, and consequently now are engaged in the purchase of 290,000 acres located in 900 townships of 55 counties of the agricultural districts at an estimated cost of $6,500,000.

No one realizes what this means, the tremendous struggle of making the people conscious of the value of water and sensitive to the necessity of preserving it. We have finally convinced the public that the most priceless resource, that the people depend on water—depend on water for their very existence—and there you have a weapon that could be adequately used against these insidious processes which are uneconomical and definitely un-American.

We devised a definition of water conservation, and it is used now by nine-tenths of the people in our state:

> ''Water is conserved when it is retained at its source, or retarded in its progress, that it may serve man and beast to the limits of its capacity in an unpolluted condition before it passes from the land.''

On the pretext of urgently needing additional pasture land, that definition is being violated.

The greatest help that we have received in carrying on our program in an effort to preserve some small segment of Minnesota's tremendous water areas came from the President of the United States, when he told us that we had $7 billion 800 million worth of surplus farm commodities in storage, and that storage was costing our taxpayers $32,000 a minute for rentals. Then the people started to get the true significance of what we call the monstrous monuments to mismanagement, the warehouses that dot the countryside.

Gentlemen, we need your help in Minnesota. We want to save this water heritage. If we don't save it, we'll be destroyed.

To the north of us from that great, young, powerful giant, we are going to receive fuel oil and natural gas; from the west of us we are going to receive fuel oil. We have billions of tons of taconite which we're just starting to utilize. In order to process one ton of good iron ore into a ton of steel requires 150 tons of water. In order to raise one bushel of wheat requires 4000 gallons of water. In order to raise and put one pound of beefsteak into the butcher shop requires 1300 gallons of water. In order to raise the wool that goes into a three-piece wool suit requires one ton of water.

The water tables are dropping in our great dairy country. In the little town of Watkins the wells went dry, not by an act of God, but through drainage.

It requires 30 pounds of water to process one pound of butter after the cream has been delivered at the dairy.

I want to bring this to your attention. I have no quarrel with the SCS and the PMA as long as they adhere to the principles of soil conservation as laid down by Dr. Bennett. At the inception of the SCS program we received him with open arms and established him on a firm footing, and instead of carrying out the objectives that originally justified their existence, as soon as they got into operation they went into the business of drainage.

Now, I'm going to say two more things, and I'm going to give the floor to someone else, and I'll be very pleased to answer any questions concerning this.

First of all, five times this morning I have heard the word ''stewardship.'' I believe in stewardship. I'm a crusader for stewardship.

Stewardship means this: Your Creator has endowed the earth with all the things necessary to sustain the earth, and has found them to be good. While you dwell among mortals, you must protect them, use them wisely, guard them closely, squander them not. If you are untrue to this sacred trust, mankind shall not be perpetuated, but shall banish itself from the earth.

A philosopher once said, ''I cried because I had no shoes, until I saw a man who had no feet.'' In Minnesota we still have feet. We still have shoes. The soles of those shoes are being worn thin. Unless we get help to save at least some small segment of that tremendous water heritage with which we were blessed by the Creator, Minnesota will become a second- or third- or fourth-rate state. Thank you.

VICE-CHAIRMAN FONTANNA: The gentleman from Minnesota has made some strong statements. Maybe there is somebody on the other side of the picture.

MR. DORER: I'm prepared to back them up, sir. [Laughter]

VICE-CHAIRMAN FONTANNA: Does anybody care to speak to what he had to offer? [No one responded.]

He's a neighbor of mine. I think he's got a point. Are there any agriculturalists here who want to talk about that situation? It's rather a strong condemnation.

MR. LESAGE: Mr. Chairman, may I come back to Mr. Ackerman's very fine presentation? He has said, very rightly, that a trend has been moving from single-project planning to full-basin planning, and even to basin diversion.

Now, as he has also mentioned, the basins of our rivers in North America cover, in your country, more than one state, and in many instances they cover more than one country, and we have as examples the St. Lawrence—I'm picking only two—the St. Lawrence, which is being developed after long years of waiting, through international cooperation; and we have the Columbia River system.

Now, one of the things you said, sir, about the Columbia especially was that it had not been possible to obtain full development because of long and bitter conflicts between the states in the United States. As everybody knows, one of the difficulties, of course, is the sharing of downstream benefits to be derived from works in the upstream states, and I would go further and say in the case of the Columbia and a few other cases, for the works in the upstream country.

Now, I am sure, Mr. Ackerman, that you have given quite a lot of thought to this matter and discussed it with a lot of people, and I would like to have your opinion as to a possible basis for this sharing of downstream benefits.

DR. ACKERMAN: Minister Lesage, you have, of course, brought up a most timely and most interesting problem from a technical as well as from a political point of view. My answer will be brief, but I would still like to stress that I'm speaking as a private citizen of the United States, and my answer is in that capacity only.

DR. ACKERMAN: I think the comments which I made about the compartmentalization of the resources, or the water resources, of some of our rivers, because of the federal system in the United States—I think that Minister Lesage is perfectly right in extending the principle of those remarks to an international stream like the Columbia.

Now, if my remarks carried some implication of disparagement about what was happening on the Columbia, I think that I can say that the United States bears equal responsibility with Canada in that—possibly more.

However, it is an excellent illustration of the need for planning on a basin wide basis. We happen to have got started in thinking about the Columbia, per haps, a little ahead of what you did in Canada. Now, maybe you can correct me, if that is not right.

MR. LESAGE: Oh, that is correct.

DR. ACKERMAN: The development, therefore, started from downstream, and it was on planning which was downstream planning, rather than considering the benefits which might be derived for both countries from all the streams.

Now, it seems to me that we are at a point at which it is a little more difficult to plan the whole Columbia sytsem for the benefit of both countries than it would have been 10 years ago. However, I regard it as far from being impossible.

I think it is a necessity that benefits to both countries be considered, and I think in the future we should be able to arrive at a reasonable settlement of it.

MR. OLLIE FINK [Executive Secretary of the Friends of the Land]: I would like to raise one little question about two comments by Mr. Ackerman. First, I think when we talk about multiple-purpose projects we have to spell them out in a great deal more detail than Mr. Ackerman has. What I mean by that is this. If you would go out on to the lower Missouri and talk about power and irrigation dams, those people want those dams empty at the time that they can catch water to prevent floods on the lower river.

Of course, if you have two or three advantages, like wildlife and power and irrigation, they all go beautifully together, but if you try to include flood control and some other purposes in certain topographical conditions, you run into very serious problems.

So it's just a big question mark, and there is still a lot to be decided on what is feasible on the multiple purpose dam, particularly if you include the flood control, because even on the TVA we all know that many of those mountainsides down there, some of them so steep the cattle fall off, have been ploughed, and I have seen TVA land ploughed right to the edge of the water on a TVA ploughed hill.

We have the problem there that, when you conserve enough water in the reservoirs, you have buried the most valuable agricultural land in the Tennessee valley, and the best land is at the bottom of those reservoirs.

Some commentator made a comment in a national magazine—Mr. Anderson—the other week. He referred to the democratic philosophy that the TVA represents. What happens is that the TVA pretty well regulates the management of that entire area, and local management is pretty well gone, and we have a situation in which the TVA serves an area of seven states, but from a socialistic point of view it drains 41. Thank you.

VICE-CHAIRMAN FONTANNA: Mr. Ackerman, do you wish to make a comment?

DR. ACKERMAN: I can only say, in answer to the last speaker, that I'm sorry that this question of socialism has arisen in our conference, because it's one that you could continue speaking on for rest of the conference and never arrive at any satisfactory conclusion.

I spent about two and a half years with TVA as Assistant General Manager, and particularly I was interested in planning for the future of the Valley. I went down to the Valley with an open mind as to what I might find, looking for the bad things as well as the good, and I would like to say that nothing I found there in any way convinced me—in fact, quite the contrary—that TVA was against the system of free enterprise.

Free enterprise is stronger in the Tennessee Valley states than it was at any time, and when I mentioned demonstration this morning as a feature of the TVA and water development, I meant it.

TVA's ideas and the philosophy behind it are those of demonstration. As soon as the agency feels that a demonstration is completed, and another agency can take over even if it indicates only partial willingness, as in the case of the park system that was developed along the reservoirs, TVA steps out and the state agency steps in. TVA has worked constantly with other agencies—not federal—individuals and companies in the Valley.

I think if you want to make the argument that TVA is socialistic, you have to consider certain other things like the Post Office services and the highways and various other things that are undertaken by governments for us.

Now, that is the best I can say. I'm already launched, as you see, on a long argument, and the time is running short; but I would like to stress that there are two points of view on the question that the speaker has brought out.

VICE-CHAIRMAN FONTANNA: At this time, ladies and gentlemen, the time has run out. When you get into the business of water, you are into something that goes on ad infinitum, so we'll turn the meeting back to the Chairman.

TWENTY YEARS OF PROGRESS IN FORESTRY AND RANGE MANAGEMENT

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One of the most dramatic stories of American forestry occurred before the 20 years of progress we celebrate, but its strong contrasts bring into sharp focus the technical advancements we have made. That story is of an early ranger examination of the U. S. Forest Service. One question read:

> "What would you do if alone in the forest, on foot, 30 miles from the nearest neighbor, 'phone, or other source of help, without tools, and confronted by a crown-fire five miles wide and travelling 15 miles an hour?"

One answer was: "Run like hell and pray for rain!"

At the time of that examination, forest fire fighting consisted usually of three ingredients—a fire, a man, and a shovel. The man and the shovel, being two, to one of the fire, sooner or later prevailed—often much later. Today, modern instruments predict the weather before it happens. The certainty of forest fire ignition and the speed of burning—called "class of fire-danger day"—are anticipated with amazingly few errors. The forester travels little on foot. Automobiles and airplanes speed him. Parachutes deliver his first attack. Powerful and compact pumps, bulldozers, and other mechanized aids replace and augment the shovel. He now talks of removing lightning from clouds over forest areas. But we need not recite all details of progress in this most spectacular phase of forestry—of firefighting. Examine the record. During the past 20 years no historical forest fires have occurred with the single, uncurbed intensity of those which harrassed Canada and the United States in the Miramichi, Bagot, Pontiac, Peshtigo, Fernie, Great Idaho, and Tillamook conflagrations. And with our technical advancement, they probably never again will occur for as long as our various public and private firefighting organizations remain strong and effective.

Early in the century we faced forest-destroying insects and fungi whose life histories and methods of attack were little or incompletely known. During the past 20 years these mysteries have been solved. We now possess the knowledge and equipment with which to control these destroyers. Failure to do so will be the fault of politics and business—to sanction control and pay for it—and to refuse the demands of business firms that import cheap planting stock, cheap elm logs, or exotic plants. If we now fail to handle these less dramatic threats to our forests, it will be because we are too primitively political, or too advanced in national decadence.

During the past two decades, we have solved most remaining mysteries of how to reproduce our forests. And we have devised pictured representations of tree health and quality—as in Keen's and Taylor's crown classifications—which teach dependable understanding to young foresters whose earlier ineffectiveness baffled us.

The broad ax which hewed our ties during the first century of railroading in North America—1829 to 1929—gave way to the portable sawmill. No longer is it necessary to leave mature, sound, trees in the woods—too large for the tie hack to hew into ties. The sawmill handles them easily and makes room for vigorous young trees.

Little more than 20 years ago, vast forests of western hemlock, the balsams and firs, the jack and lodgepole pines, and the gums, were unmerchantable. For lumber, these were inferior woods. Some of them we could not give away, though our nations needed more wood supplies, and though many lands they occupied produced neither fiber to make us comfortable nor reverence to keep us democratic. In the deep South, pine lands which had been cut over for lumber, seemed worthless for anything else. Today we convert small and low-value trees to pulp, paper, rayon, cellophane, veneer, varnishes, and a host of other products whose variety and origins confuse and amaze us. Most forest lands are now profitable to operate.

Not much more than 20 years ago, good forestry was a dream believed in mostly by those in the colleges and forest services of our respective nations and commonwealths. It was scoffed at by lumbermen who were exploiting remnants of the great forests which Nature had strewn across the continent. Today, favored by better utilization, fairer taxes, and conversion of low-quality wood to useful products, private forestry becomes a wonderful and admirable giant, though scarcely of age. Perhaps its youthfulness explains in part why it now with occasional impertinence claims it does a better job than government foresters upon their poorer lands, whose idealism it formerly ridiculed.

Twenty years ago we still were chanting—although with waning conviction—a chorus predicting a timber famine, sung during the previous 50 years to prompt forestry thinking. We now are sure there will be no timber famine. No longer are northern forests blue with smoke all summer. Even the low-value woods now serve us. Forest lands can be restocked except where we lack political courage or individual honesty with which to restrain destructive competing uses.

In range management 20 years ago, one of the worst abused areas was the 142 million acres of public domain in western United States. It had been held for homesteading, but it was too poor for many to want. It was the "free" or "open range." Freedom of the "open range" was a quality that virile men will long hold close to their hearts, if only in memory, picture, and story. But the open range was in sorry condition. Stockmen herded their livestock to these areas with little concern for the welfare of the land. They hurried to beat others to the first grass of spring. Then the Taylor Grazing Act of 1934 placed these lands under allotment. Scientists who have watched these lands have thought them still too heavily grazed, but most of them are healing.

During the 20 years just past, knowledge of vegetative succession and soil permeability as measures of range condition and carrying capacity has matured into a capable science. Those unfamiliar with scientific techniques and disciplines which govern scientists, repeatedly have challenged scientific discoveries. This is understandable. The successful non-scientist dealing with natural resources usually is an opportunist. If need be, he stretches the truth to meet his own ends. An example is the rancher who told his young relative that a rancher could steal another man's water and grass and still enter heaven. Naturally he thinks any scientist whose opinions oppose his desires likewise works to prove a contention. The result has been conflict between range scientists and stockmen. Scientists have stated little in such arguments except their interpretations of range condition. Politicians and ranchers, in contrast, have talked about "college-bred young punks," about distant bureaucrats in Washington establishing policies for lands they never have seen, and of the impossibility of learning to run a ranch from books.

Judging from the increasing numbers attending our colleges, they must have something besides athletics. As for distant bureaucrats, they establish only broad principles for details worked out upon the ground by local technicians. Comparably, we might think of a mother with an ailing child. She knows the child better than the young M.D. she summons. He may never have seen the infant before. But his science recognizes symptoms and prescribes cures. As for books upon range management, where is a published list of them which ranchers have read and found foolish? We may say of books that a quite complete, definitive literature upon forest and range management has developed during these 20 years.

In fairness to the rancher, we know that it is impossible quickly to expand or reduce herds to fit range conditions or the needs of family economics. Furthermore, the exigencies of ranching—weather, disease, employees, and the notions of range livestock—demand that the rancher be a vigorous opportunist. But, despite charge and countercharge as viewed from the sidelines—not the bleachers—we have progressed during the last 20 years. Understanding grows. Ranges as a whole improve. Tiny ranches, with too little acreage to provide a good living, quite naturally often are evilly abused. But ranchers traditionally always are eager to expand their holdings, and many with extensive ranges are demonstrating the findings of science.

Taylor Grazing lands, now Bureau of Land Management lands, show distinct improvement in comparison with their 1935 condition. U. S. national forests, placed under allotment 50 years ago, vary in range condition. Management upon them began before modern range science had developed. They suffered the increased grazing pressures of two world wars. Upon some forests, where herds were diminished to halt overgrazing, reductions were not great enough to stop deterioration. Some of the Forest Service's ten administrative regions did not reduce livestock numbers when prices were low. They understandably met opposition when they tried it with prices high. Cattle and sheep men for decades enjoyed a mutual animosity. They abandon this tradition. Their spokesmen now upon occasion unite against the Forest Service. Complicating the affair are recent political beliefs, which result in political appointments within the scientific services. The result is a kind of emulsion of political desires and scientific verities. Hence some needed range management is being postponed.

Had someone at the first North American Wildlife Conference predicted the 20 years of progress herein recounted in the economic, mechanical, and technical phases of forest and range management as they affect tree and grass production, we would have been delighted—but we scarcely could have believed him. What if he also had predicted our great and continuing wars, our national debts, our loss of national, family, and individual honor, our frequent lack of fairness and understanding in respect to land ownership and management, and our inability to define and protect important wildlife and recreational values within our national parks and wilderness areas? We again could scarcely have believed him. We would have been shocked!

Should foresters and range managers declare that these human ills are the province of psychologists and sociologists; that our duties are to raise trees and grass? No! We can apply our sciences only when political and legal conditions are favorable. The ills mentioned are ecological, and none can think more ecologically than those who constitute this Conference. Finally, scientists of nations content to follow whatever rulers they may have—the Chinese and Germans for example—inherit dictators. We dare not evade these human aspects of forest and range management.

Let us hypothesize. We become overwhelmed by the Frankenstein of a material and mechanical civilization while losing the contact with Mother Earth which long kept us reverent, idealistic, and honorable. We in the United States are farther along this trend than Canada and Mexico. For centuries we were a nation of small farm owners, highly independent, self-reliant, outspoken, hence democratic. We were a bit superstitious about the natural phenomena which engulfed us and modified our livelihoods. But we were reverent, and perhaps fortunately, not very mobile. Of this Jefferson wrote from Paris in 1787: ". . . our governments will remain virtuous for . . . as long as they are chiefly agricultural. . . . When they get piled upon one another in large cities as in Europe, they will become corrupt, as in Europe." Likewise Dr. Webb (1951), eminent historian, recently concluded that democracy is a frontier phenomenon.

Today we are a nation of business men, with increasingly little understanding of natural phenomena and of basic enjoyments within them. When we were a nation of farmers we talked chiefly about the weather—and were not positive in our pronouncements concerning it. Today we talk of the economy, and with much assurance about expanding populations, expenditures, and development of any resource not already exploited.

We in the wildland and wildlife fields sampled this new medicine. We used economics to impress business men and legislators with our need for larger appropriations. We compiled huge figures showing spending by hunters, fishermen, and other recreationists. This was worth trying, but it did not work. We can claim that huge military needs crowded out non-military appropriations, but this is not convincing.

Men said we needed business in government—and business men. We got them. Every profession has both a strength and a weakness. I admit those of the teaching profession, but teachers are not calling the tune on natural resource management now. The strength of business men is their discipline. They have a sure sense for what will make dollars. They use only two colors of ink—black and red—and a good business man is not color blind when it comes to ink. Furthermore these business men have vigor, and a powerful tool in their Chambers of Commerce. What is their weakness? Their ignorance—biological, ecological, and recreational.

What are these men doing? They attempt to place a mechanized cableway to the top of Mount Rainier, in Mount Rainier National Park. They sought to install a permanent, non-lowerable, ski-lift in Rocky Mountain National Park. They seek to dam the Green and Yampa Rivers in the magnificent national monument in northwestern Colorado which mistakenly still bears the name of Dinosaur. They advocated placing a highway along the C & O canal. They propose and support irrigation and other works which will destroy national wildernesses and remnant herds of grizzly bears. They protest when game and fish departments reduce creel limits and restrict fishing methods under the pressure of new residents and tourists brought by their advertising. The list could continue.

Most of the schemes which destroy recreational values of forest and range are described in newspapers which carry the business man's advertisements and promote his projects, and are written by journalists with scant biological and ecological background.

Business men believe national park recreation is merely sight-seeing with all possible comfort and ease. Their idea of good hunting is success sufficient to impress the neighbors, gained with the latest mechanical aids, imported from distant factories, for use against animals still primitive.

A seasonal ranger who recently worked in Yellowstone National Park sensed the great change in wildland recreation. He complained that each summer more men came in ordinary business suits. Realize what he meant! Today's rule for vacationing is to take half as many clothes as one thinks are needed, twice as much money, and start. Good roads, filling stations and garages, motels, and restaurants remove most need for self-reliance. But this young seasonal ranger realized that the measure of an outdoorsman is the preparation he undergoes, the discipline he accepts, the occasional hardships he meets, his adventures with little curbed nature, and his courage under real trial—not what he has seen.

Business men foster a myth about unspoiled recreational values. When they find an area with wildlife or recreation superior to that of other regions, they seek to capitalize upon it and to advertize it as unspoiled, or unlimited. You in Canada realize what I mean. You advertise "Canada! Vacations Unlimited," and tell us of your uncrowded space. Compared with the quite crowded United States you have more, but you who know the history of Canada's wildlife and outdoor adventure since the beginnings of your Dominion, know that values are limited and that some are forever gone. In respect to your advertisements within magazines of the United States, please get your "Vacations Unlimited" together with your Dominion Ten Canadian Whisky which advertises—". . . like all fine things, in limited supply."

What have these business men failed to do? When the present administration in the United States took office—and I voted for it —it promised economy. We approved, with the expectation that we would accept curtailed expenditures, tighten our belts, and reduce the national debt. Accordingly, national park staffs are smaller than formerly, and national forest campgrounds are meagerly maintained. But visitor records are permitted to reach an all-time high, while we make slums of areas we hold in trust for future generations. Making the affair ludicrous is the belief that people are more prosperous than ever before, and that they are able to pay their way. The increasing popularity of toll roads attests the public's willingness to pay for reasonable service at its source. We have business men in government now. We should expect them to devise a system by which visitors pay needed costs.

Who is at fault and what shall we do about it? Business men obviously are much to blame. Circumstances of civilization are. But we who here assume responsibility for wildlife and recreational resources upon forest and range lands must accept our full share. True, wildlife and recreational management came into existence belatedly during the evolution from agricultural simplicity to urban materialistic complexity, and had not completed their basic inventories and techniques before the need for national philosophies had arrived. Hence much of our writing implies that we are a kind of animal husbandrymen, dealing with wild rather than domestic species, but with the purpose of producing sustained crops of meat. Forgotten is the rest of Leopold's (1933) definition, ". . . for recreational use." We forget that our principal crop is wholesome adventure—adventure that uses the whole man and not just his eyes in sight-seeing, and that gives reward beyond meat.

We face four great tasks. The first is to determine philosophical objectives of wildlife and recreational management—to serve us until growing techniques make verities of hypotheses like those of Jefferson and Webb.

The second is to inform the business men concerning limitations of resources they would capitalize. We would not weaken the power of business in an advanced civilization, but we need to invade their newspapers with more than the arts of wildlife predation and the successes of ardent human predators. Things like short histories of all one-time and remaining wildlife species and outdoor activities peculiar to each state and province should help; especially if each analysis discusses the potential restoration and future of each resource. It is such complete knowledge of the past, present, and future of each wild creature and sport which gives the mature expert his administrative and philosophical ability.

The third labor is to create desire for primitive outdoor experiences —not to boast of what can be seen, photographed, or harvested with ease. Forest and range lands are adventure lands. They are the homes of big and wilderness game. They are places where pioneer arts of travel and living still are valid. We have time or weath with which to afford the finest experiences. Why should we be satisfied with soft kinds of outdoor living and pretend to be better than we are?

The fourth great task is to provide adequate funds for management. We need earmarked funds like the duck stamp and hunting and fishing licenses to finance our management, and to show competing land uses the values we defend.

In wood-forestry, the consumer pays upon a very tangible basis— the board foot, cord, or ton. Big game licenses also are upon a unit basis. Small-game and fishing licenses are based upon a multi-unit principle, with discounts based upon probabilities for failure. In range management, the fee is difficult to assess precisely in respect to forage volume, but is definitely limited to persons, areas, and animal numbers. In the recreational use of lands with less material harvests, we have sought to tap general appropriations, or even a percentage of monies which lumbermen and cattlemen have paid into the national treasury. Passing years prove these attempts unrealistic. Admission fees for areas where many can be admitted through few gates, or licenses covering scattered use, with costs geared to needs, are a better basis. Within states or provinces wherein visitors use a variety of wild lands under various national and local jurisdictions, one license for the gross area, with funds distributed equitably according to intensity of use and management costs, would lessen the irritation resulting from being charged at every turn.

We need particularly to provide funds expressing our interests in lands few visit, but which give satisfaction from their very existence. Just as Mexico City, Ottawa, and Washington are accepted as centers of government by people who never have visited them, wild lands are highly regarded by men who like outdoor adventure, though they have not yet or may never visit them. We must contrive to support these wild places with contributions in the way we do our national capitols with our taxes,

We should remind those who think lands worthless unless actually swarming with people, of the effort and money expended as an interest in heaven, by people who never have been there.

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PROGRESS IN WILDLIFE RESEARCH AND TRAINING

CLARENCE COTTAM

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To summarize adequately two decades of wildlife research and training would require a far better crystal ball than I have been able to find. After contacting 40 prominent leaders in this field from different parts of the United States and Canada, I am led to the conclusion that the profession of wildlife management is too much in its infancy, if not still in its embryonic development, for the clearly defined trends and outstanding contributions to be unmistakably recognizable. These interviews show that with thinking people there are about as many different shades and variations in conservation faith as there are in the philosophies of religion and politics.

AREAS OF AGREEMENT

A few generalized convictions stand out:

1. We must have more and better research, and the investigations must be thoroughly objective and performed in an atmosphere conducive to sound research.

2. While much of the research when critically reviewed may not be profound and some even mediocre or worse, because of its volume and diversity it is comparable to the great wealth accumulated from Woolworth's numerous "Five and Dime" stores. Much has been accomplished, yet infinitely more remains to be uncovered.

3. Our greatest obstacle and handicap is our current inability to sell our wares and train Mr. and Mrs. John Q. Public and their ever-increasing progency to be conservation conscious. A renewable resource, like a great painting or a vein of uranium ore before the

atomic age was ushered in, is of public value only as it is used or enjoyed. Our success in research and training will be mediocre indeed, until the public is made aware of its benefits in helping America to live more abundantly. The greatest challenge confronting conservation forces is that of improving public relations through an effective program of salesmanship and a demonstration of our good works. Sound wildlife and fishery management can never be a reality until there is first a reasonable semblance of human management.

This amazing age of artificial photosynthesis, nuclear fission, guided missiles, jet engines, radar, hybrid corn, anti-biotics, TV, and X-rays has sold the American public on the necessity of research in war, industry, agriculture, and medicine. Yet public vision hasn't been sufficiently clear to see the necessity of research and sound training in the field of wildlife conservation management.

Wyoming's Commissioner, Lester Bagley, succinctly summarized the situation when he recently editorialized in *Wyoming Wildlife:* "We fish with nylon leaders, with fiberglass rods, with reels made of new alloys. Scientific research has given us new powder for our cartridges and new . . . metals insure rifle barrels with life-long accuracy. We live in a wonderful age of scientific advancement and we take full advantage of it in the fields of medicine and industry.

"Isn't it odd then, that the modern-day sportsman sometimes expects game management to ride in a buggy and yet keep up with present heavy demands on fish and game? . . . They regard scientific research in wildlife matters as a stupid innovation.

"If the public wishes to maintain its wildlife resources, it must be as ready to give serious though to the findings of the wildlife biologist as it is to accept the products of the industrial workers (Bagley, 1954)."

EXTENT OF RESEARCH FINDINGS

If one questions that wildlife research and management have made significant progress, he should review the recent imperial octavo 435-page *Wildlife Abstracts* compiled by Neil Hotchkiss (1954) of the Patuxent Wildlife Research Laboratory. It contains the briefest abstract or title and pagination of more than 10,000 articles, books, and bulletins—all published during a 16-year period from 1935 to 1951 on wildlife research and management or on closely related subjects that have application to wildlife conservation. The Cooperative Wildlife Research Units at only 17 universities (originally there were nine) have already published some 2,300 papers, and they are now producing 150 more annually. Some of these are books, bulletins, and lengthy research papers of great significance and value. The volume of important published or duplicated papers is increasing rapidly, and many other papers of great value are given at regional, state, or local conferences. Many significant accomplishments are not getting into permanent publications. Obviously, because of the large amount of research findings that are already in print, only a small part of even the more important contributions can be mentioned here.

Perhaps a quarter of a century hence we can review more accurately the accomplishments and trends of the past two decades. While our relative ratings and listings of contributions may differ somewhat, most will agree that much progress has been made. Unfortunately this progress has not been uniform or constant and not without mistakes and periods of retrogression in certain lines.

WATERFOWL RESEARCH

Possibly because of my own past experience in waterfowl research and management, I believe that two of the most important recent research contributions have been the development of the breeding ground survey of migratory waterfowl, and the flyway concept of waterfowl migration (Lincoln, 1935) and management (Cottam 1951; and Day 1949, 1952). The breeding ground survey, until recently directed largely by C. S. Williams (1947-53), and reports compiled by him or Walter Crissey (1949-54), were important not only because of the much-needed technical information it gave us as a background for improved management, but also because they more or less committed both Federal and State wildlife administrations to formulate decisions and procedures (*i.e.* regulations) on the basis of scientific facts. These waterfowl studies were of major importance because of the cooperative teamwork they developed with common objectives and procedures, and the large number of competent waterfowl workers which were brought together on a common problem from private and public organizations of both Canada and the United States. Until 1947 the respective states, with few exceptions, had contributed relatively little towards fact-finding or management of migratory waterfowl because they felt that waterfowl management was primarily the responsibility and duty of the federal agency, the Fish and Wildlife Service.

Since then, every state and province which supports a significant number of breeding waterfowl, and a number of other states besides, has effectively participated in the waterfowl breeding ground surveys; and all states have contributed to the winter or other surveys. A number of states and provinces and competent personnel from private agencies such as the Wildlife Management Institute, Ducks Unlimited, and private clubs, have been and are now rendering great service in other phases of waterfowl research. This united attack on an important but complex and controversial problem by government and private agencies of both Canada and the United States helped immeasurably to convince the duck hunter and the lay public that the program was honest and was operating in their interest. With so many participating in and explaining the program, its objectives and results were made understandable to the sportsmen, the lay public, and the administrators. Thus the data obtained were largely translated into policy. There can be no question but that the program has greatly favored the resource.

This cooperative partnership program of waterfowl fact-finding gave birth, in large measure, to the flyway councils and planning committees, and it pointed the way to more effective protection and wiser use of our migratory waterfowl resources. In my judgement this opened the door to the greatest opportunity the U. S. Fish and Wildlife Service has ever had to get the states and the sporting public generally to sell America a sound program of waterfowl conservation, and this without tint of subsidy or "give-away"! Good public relations, wise leadership, and effective cooperation in this instance could not help but be equally effective throughout the broad field of wildlife conservation. The success of the program and the stature of its leadership must now be measured by present trends and accomplishments. It is sincerely to be hoped that a regression has not set in, in which politics, lack of vision, poor leadership or low morale will prevent the unusually competent staff of field technicians and researchers from gathering the basic facts of waterfowl population dynamics and management that are needed to effectively handle this great continental renewable resource which is constantly threatened.

Analysis of planned mass bandings of waterfowl, largely initiated by C. S. Williams and colleagues, officials of the Canadian Wildlife Service, and further developed by Joseph Hickey (1952), John Aldrich (Aldrich *et al,* 1949), Seth Low, Earl Atwood and others, has done much to untangle questions of migration, distribution, population dynamics, annual harvest, and other mortality factors.

Largely as a result of the cooperative federal, state, provincial, and private waterfowl activity that had its inception in the middle 1940's were the effective and successful waterfowl research and management programs that are now being conducted by many of the states under federal aid. Many outstanding contributions from the states could be listed. Among these there should certainly be mentioned the excellent basic and applied research on waterfowl by Bellrose (Bellrose and Chase, 1950; Bellrose, 1952, 1953) and others of the Illinois Natural History Survey, the small marsh studies and development of New York (Wells, 1953), the splendid marsh, lake, and river programs of Michigan, Wisconsin, Utah, Alabama, and Tennessee (Steenis, 1950) and the coastal marsh research and management of New Jersey directed by MacNamara(1952, 1953).

Through fluctuations and artificial manipulations of water levels accompanied by a minimum of clearing and planting, many of the previously semi-sterile black or amber-colored lakes, ponds and deep marshes of the East are being made highly attractive to waterfowl. This work is based on sound research and demonstration by many, including that of Francis Uhler at the Patuxent Research Refuge in Maryland, L. G. MacNamara (1953) in New Jersey, and John Steenis and others (1954) in Delaware.

A decade ago important marsh ecological studies were conducted by C. S. Williams and William Marshall (1938) in Utah and John J. Lynch in Louisiana, J. B. Low in Iowa (1945) and by many federal workers under the supervision of J. Clark Salyer (1945) and Richard Griffith (1948) on national refuges in many parts of the country. Refuge workers, particularly Wm. Baldwin, T. Z. Atkeson (1952-54), Robert Smith, Chester Markley, and Merrill Hammond (1940), have given us essential knowledge and demonstration of successful waterfowl and upland game management in impounded river valleys. Martin (1953) and many others have contributed to our knowledge of pest plant control. These and many related studies have rendered inestimable benefits to waterfowl, and they should help to transfer the emphasis of waterfowl management from harvest only to that of production.

It would be an error to underrate the importance of the national wetland inventory that has just been completed by cooperative efforts of the Federal Service and the 48 states in appraising the value of these wet lands and showing their importance to the national economy and to the cause of wildlife conservation. (Anon., 1955)

HARVEST AND HABITAT

A significant advance in our profession is the improved philosophy of management based on harvest of available crop and recognition of the values in all resources and not solely the game value of the huntable species.

An important trend is the increasing array of data showing that when habitat conditions are favorable it is difficult to overharvest most species of small upland game by standard sporting means. This is especially true of such small game animals as rabbits, squirrels, pheasants, grouse, and under some conditions, even the migratory mourning dove. Drs. Allen (1947), Dale (1954), Stokes (1952, 1954), Glading (1954), and Cottam (1954,b) have given convincing proof of this with pheasants; while Uhlig and Wilson (1950) of West Virginia have shown that the same thing is true of squirrels; and Frank Winston (1954) and many other southern researchers have reached similar conclusions regarding mourning doves. R. W. Eschmeyer (1949) gave similar proof of this in sport fishing on TVA lakes. Favorable environment and good protection, of course, must be insured during the important reproductive period. Continued protection of most small game on relatively large refuges or by highly restrictive regulations is now known to be wasteful of the resource, and fortunately it is being replaced by wiser and more generous regulations governing harvest. Basic to all of this, of course, is the clearer understanding that research is providing of the concept of annual turnover and high mortality rate.

Research facts make clear that the more frequent and serious mistakes have been made in underharvesting rather than in overharvesting deer and other big game. Michigan's study at the George Reserve (Chase and O'Roke, 1955; O'Roke and Hamerstrom, 1948) showed that more than a third of the deer crop can be removed annually and the herd maintained at carrying capacity and in a very healthy condition. Leopold, *et al.* (1947) showed that deer were underharvested in many places in the United States. Deer have been seriously underharvested in Pennsylvania (Latham, 1950, 1953), Texas (Taylor & Hahn 1947), California (Leopold, *et al.* 1951), Wisconsin (Swift, 1946), and in many other states. The innovation of doe harvest where and when needed certainly has been in the best interest of the public, particularly the sportsmen, and also that of the game and the range.

Notable progress has been made during the past decade in developing techniques and procedures on range revegetation and range management both for browsing and grazing species. Better selection and development of strains through genetical and sylvicultural approach suggest possibilities for further improvement.

The value of suitable habitat is generally accepted today, and we are finding out what a complicated and costly business it is. It is well that critical appraisal of this work should be given. Better methods of judging results of habitat improvements on the wildlife itself are urgently needed. Two unusually good studies by Marshall (1953) and Shaffer (1953) have recently appeared.

I believe the recent improvement in the ecological approach and concept of both research and training has been a major contribution. The notion of the synthesis of ecology into a manageable science as explained by Odum in his new book on ecology (Odum, 1953), is a noteworthy example.

POPULATION DYNAMICS

Knowledge obtained through research on the operation of population increases is of outstanding importance. The brilliant research of Cheatum (1947, 1949) and Severinghaus (Cheatum and Severinghaus, 1950), showing that the fertility and productivity of young of white-tailed deer is closely associated with the quality and quantity of food supply and habitat conditions generally, was a contribution of the first order. They also showed that under good food conditions the fawn of the year produces her first offspring when she is only 12 or 13 months old. Robinette and his colleagues in Utah (Robinette and Gashwiler, 1955), workers in Michigan and California, and many from other states have conducted splended studies showing the relation of deer reproduction and herd increase to range and forage conditions. Starker Leopold and Frazer-Darling's wildlife studies in Alaska (1953) did much to focus attention on range problems and carrying capacity and of forage competition by different species of big game, particularly caribou and reindeer. Present improved concepts of habitat management are paying big dividends in increased production. Probably one of the most important recent volumes on animal dynamics is the symposium by a number of authors summarized in the January 1954 issue of the *Journal of Wildlife Management*.

Another notable advance in population studies has been the application of statistical methods to game research, and especially to inventorying. Through dint of necessity, fishery biologists long ago became efficient biometricians and used a far better statistical basis for their experiments than did most wildlife workers. This improved statistical approach gives us far better tools for measuring survival, harvest, and life equations. The work of Joseph Hickey (1952), Andrewartha (1954), Lack (1950), Chitty (1937), Elton (Chitty and Elton, 1937), Leslie (Leslie and Chitty, 1951), Christian (1953a; 1953b), Strecker and Emlen (1953), and others is providing a promising framework of fact and theory that cannot be ignored by wildlife biologists. Interpetation of data obtained in wildlife studies has been advanced by techniques of analysis proposed by Kelker (1946, 1947), Petrides (1949), Gill (1953), Hayne (1949), and Eberhardt (Hayne and Eberhardt, 1952). Their publications make significant and fundamental additions to wildlife literature.

TECHNIQUES

During the past two decades a large unmber of important innovations in the line of improved techniques have been developed. Some of them have not only improved management, but have made possible a much larger population of game and other wildlife. Certainly the "Gallinaceous Guzzler" (Rutherford, 1953) which is a device to provide water in the desert and which was developed through "P.-R." research of California, has immeasurably added to the game and wildlife of desert regions. Incidentally this is reflected in the harvest of many thousands of birds and other game. Studies of limiting population factors revealed that there was plenty of food and cover in drier regions of the state, even during the hot dry summers, but that water was lacking. Metal and concrete water tanks were constructed to provide water for wildlife and to catch runoff during the wet season. These "Guzzlers" are partially placed underground to reduce evaporation. Upwards of 3,000 of them are now in successful operation in California, and modifications of many more are in use in Arizona, Nevada, Utah, New Mexico, Texas, and in other states throughout the xerophytic west.

Other studies showed that quail in California demanded aerial roosts and were limited in the treeless areas of the state because of this lack of suitable environment. MacMillan (Rutherford, 1953) of the California "P.-R." staff merely made aerial brushpiles by tying down a pile of brush on top of four uprights. This served the quail admirably and the population of these desert birds promptly increased.

Other new and improved techniques and devices appear steadily. Some of these are helps in research and management, while others directly favor the wildlife. While many notable contributions have been made, special mention should be given to Hanson's ingenious work in determining incubation stages of eggs (Hanson, 1954) Hale *et al.* (1954) on determining sex and age groups in ruffed grouse; Petrides on age and upland game birds (1942) and Evans (1951) on methods of color marking young waterfowl by aseptically injecting aniline dyes into incubating eggs. Other useful techniques that aid in determining age, and help in appraising juvenile and adult survival, longevity, turnover, and productivity have been devised.

One of the most useful devices for measuring hunting pressure is the scholarly fluoroscopic method devised by Dr. Wm. H. Elder that readily shows the amount and position of lead shot in the body or alimentary tract of birds, and so points to the extent of lead poisoning and hunting pressure (Elder, 1950).

CONTROLS

In recent years only have chemical controls become of more than local significance to wildlife and fisheries. Over the past few years astronomical quantities of highly poisonous insecticides, herbicides, fungicides, and rodenticides have been widely distributed over marsh, forest, farm, and garden, and much damage to fish and wildlife resources has resulted. With the exception of D.D.T., relatively little study has been made of the effects of these chemical control agents upon wildlife and fishery resources. Papers by Springer (1955; Springer and Haugen, 1953) Linduska (Linduska and Springer, 1949), Cottam (1954a; and Cottam and Higgins, 1946), Fielding and Baldwin (1955), and many others have given summaries of work accomplished and results attained. The importance of the subject demands much more consideration than has yet appeared. DeWitt's studies at Patuxent show that the cumulative effects on bob-white quail produced by intermittent feeding upon chlorinated hydrocarbon insecticides may be much more severe than those produced by continuous feeding at the same level. It was found that ingestion of sub-lethal doses of some of these insecticides had serious indirect effects and resulted in significant decreases in hatchability of eggs and in viability of chicks (Dewitt, 1955).

Progress recently has been made on research of depredation and control of blackbirds (Mitchell, 1954) and on waterfowl by personnel of the Canadian and United States Wildlife services, although many critical and complex problems still await solution.

Significant and fundamental studies by Welch (1954), Crabtree (Crabtree and Robinson, 1953), and by Don and Jack Spencer and others have resulted in lessened emphasis on reductional control in favor of repellents, including chemicals, visual materials, electric, sonic, and supersonic devices. Repellents have been used mainly in packaging and against commensal rodents, but they may also be used against temporary depredations of normally beneficial and desirable forms of wildlife. Repellents are of particular value in plant propagation and especially in direct forest seeding. Don Spencer's current research (Spencer, 1954) in this important field gives much promise.

Missouri's study of predator control by Sampson is noteworthy as it involves selection of offending individual predators and extension trapping through which the landowner is taught how to solve his own problems (Sampson, 1953). The approach under Missouri conditions seems to be both economical and effective.

Weldon Robinson's important studies on coyote and bobcat tagging are important because they are basic to a sound reductional predatory control program (Robinson, 1951). Sperry's noteworthy and detailed food habit studies likewise are fundamental to the economics of reductional control (Sperry, 1941). Kalmbach's discovery that rodents do not differentiate colors while birds are highly sensitive to color helps to make rodent control programs much more selective against rodents and less of a hazard to bird life (Kalmbach, 1943). Paul Errington's *Predation and Vertebrate Population* probably is the most significant and basic contribution on predation yet to appear (Errington, 1946).

FOOD HABITS, NUTRITION, AND DISEASE

During the past few years a number of fundamental studies have been conducted on nutritional requirements of various wildlife species. Nestler's basic work (Nestler, 1946, 1949; Nestler *et al.*, 1949) showing the importance of vitamin A in winter as a requirement of bobwhite diet was of special significance. It helped to explain why quail frequently suffer a die-off in cold winters of heavy snow.

Einarson showed that there were nutritive deficiencies in the diets of black-tailed deer in the dense Oregon forests (Einarson, 1946). Nutrition studies by DeWitt and Derby, 1955), Lehmann (1953), and many others are basic to sound management.

Food habit studies properly balanced with laboratory and field work are fundamental to an understanding of wildlife and its proper management. Many important publications, including those of Sperry (1940), Cottam (1939), McAtee (1935), Kalmbach (1940), Mendall (1949), Neff (1947), and Munro (Munro and Clemens, 1937), summarize basic studies in this field. Possibly the most significant single study on this subject during the past decade is Martin, Zim, and Nelson's *American Wildlife and Plants* which summarizes 60 years of food habit data in the extensive files of the U. S. Fish and Wildlife Service laboratory (Martin *et al.*, 1951). Likewise, Martin and Uhler's *Food of Game Ducks in the United States* has been a great help in planting programs and in the management of our waterfowl resources (Martin and Uhler, 1939). An understanding and fundamental contribution to the subject of disease is Kalmbach's Botulism report (Kalmbach, 1934).

FEDERAL AID PROGRAMS

Dr. Gabrielson (Rutherford, 1953) succinctly summarized the role of "P.-R." projects in national research as well as in management when he wrote, "P.-R. has become the backbone—or perhaps, more correctly, the spinal column—of our state wildlife programs." Under provisions of the program (research and investigations) applied research was to be stressed. While some of the investigative work probably has been mediocre, many results of high caliber have come out of this program. The research and trained men in the departments of the respective states have helped to broaden the vision and raise the standards of conservation practices. The program has served like yeast in a baker's loaf. Viewed on a yearly basis, the results, with few exceptions, might not be particularly impressive, but when compared and measured over a decade, the outcome has been spectacularly gratifying.

Surveys and investigations amounted to 32 per cent of the total program in the first decade and about 19 per cent from 1949 to 1953. Surveys and census work originally could well be considered as research, but now methods are largely standardized and it should therefore be regarded as operational or routine.

Appropriately, much of the early "research" was directed at finding out the extent of the states' wildlife resources and what improvements and procedures were needed to do an effective job of management. Studies directed toward measuring the success of stocking and artificial propagation programs were very important in helping to eliminate most of the costly and unproductive procedures. Studies on habitat improvement while normally unspectacular, have resulted in more effective work of the departments. Life history and management studies on most game and some non-game species by one state or another have resulted in great improvement in our over-all knowledge of wildlife conservation in America. Many new and important techniques have been worked out and management made very much more efficient. The statistical approach of Dr. Vincent Shultz's wildlife survey (Shultz, 1952) of Tennessee has helped to establish sound procedures in making needed surveys in other states. Ted O'Neill's study of the ecology and management of muskrats in the gulf coast marshes is of great value and far transcends the state line of Louisiana (O'Neil, 1949). Much valuable information has been obtained from studies of the bob-white in the different types of habitat in which it is found throughout southern and eastern United States. Management of the deer problem is made firmer because of the detailed researches on these big game animals in relation to range carrying capacities.

While some long range fundamental studies have been conducted, the majority have been largely short-term applied research. A great variety of subjects have been undertaken, including surveys, food habits, disease research, tagging, banding, management of impounded waters, color markings, and detailed studies of a great number of wildlife species. Studies on techniques and devices have resulted in rotary mowers, air thrust boats, improved harrows and other farm equipment, ditchers and weed cutters for shallow marshes that were overgrown with undesirable vegetation, and many other tools and procedures that have application to workers in all states. Perhaps more studies should now be directed toward fundamental problems, and particularly those where a cooperative approach is called for. Some cooperative attempts on a regional or sectionwide basis have been undertaken and have been reasonably successful. The cooperative mourning dove study is a good illustration, as is also the splendid state waterfowl work carried on throughout the United States and Canada. These studies have resulted in the acquisition of much fundamental information essential to sound management and harvest and in extended improvement in wildlife habitats available for specific species.

Studies on pest plant control already have made many acres of waste marsh and aquatic land productive for wildlife.

The three-year-old "D.-J." program (Anon., 1954, and Eschmeyer 1954) already has produced notable accomplishments in knowledge of American fishery rseources and in the improvement of several techniques—particularly electricity in the sampling of fish populations. Funds for this program are being used in studying improved management and restoration of many areas for specific species of fish. More than half the "D.-J." funds are earmarked for research, investigations, and surveys. Appropriately, an appraisal is being made of the effectiveness of the stocking program. Studies are under way on fish populations, tagging, marking, habitat improvement, and management. Farm pond management and stocking programs are under study. Research on impounded waters and small lakes likewise is an important topic. Because the program is new it is to be expected that much effort will be made to provide general surveys, harvest studies and to obtain basic data that will serve as a foundation for future work. In coastal areas much attention is given to marine and anadromous fishes. Because of the extensive farm ponds and stocking programs, much attention is directed to fish culture, diets, and the improvement of fish stock. Disease studies are especially important.

Control of carp and other "trash" fish is also a subject of investigation. Likewise, weed control is as important in inland fisheries as it is in wildlife management. Even though this program is new, already notable accomplishments have been made and much better planning for sound programs is now possible.

OTHER FISHERY PROGRAMS

Fishery research, at least that which is federally sponsored, is older in America than is wildlife research. As would be expected, greater emphasis has been placed on palegic and anadromous species, and many great accomplishments have been made (See Eschmeyer, 1954). In recent years much cooperative study has been given to the serious and destructive sea lamprey of the Great Lakes. Detailed life histories and many control procedures have been worked out for the lamprey under the leadership of Moffitt and Applegate (Applegate, 1950).

Other recent research efforts have included extensive studies of fishery resources in the mid and western Pacific. It is amazing that so little public support has been directed to this great unexplored resource until only recently. Emphasis of the current study, under the able direction of O. E. Seatte of the U. S. Fish and Wildlife Service, is on the tuna their life history, exploitation, and management.

Because of repeated outbreaks of red-tide, the public has insisted on a study of this complex, annoying, and destructive phenomenon. Some progress has been made in learning why *Gymnodinium brevis* blooms so frequently on Florida's west coast. Apparently substances from the adjacent coastal swamps stimulate its rapid reproduction.

Other recent research accomplishments include:

1. Farm pond studies and management, especially the outstanding research of Swingle and Smith.
2. Survival studies of hatchery-reared rainbow trout in comparison with wild trout of streams.
3. Development of electronic screens that are effective in guiding salmon in streams.
4. Development of an infra-red "snooper-scope" that is used by researchers to pry into the behavior of fishes—especially as they approach electronic counting devices; electronic screens and equipment designed to guide fish safely past dams and turbines. A "sea scanar" was also developed which will locate groups of fish at a distance and thus aid the fishing industry.
5. Basic research has shown, through the use of radio isotopes, that there is absorption of minerals from water by fish. Calcium, phos-

phorus, and cobalt can be absorbed directly from water without going through the food chain.

6. Many important studies on specific species of fish and shell fish have been made. The striped bass, shad, and salmon have been given special consideration for several years.

COOPERATIVE RESEARCH UNITS

In my opinion it would be extremely difficult to overestimate the important contributions that have resulted from the Cooperative Wildlife Research Units with universities (Leedy 1955). These were started under the dynamic leadership of J. N. (Ding) Darling in 1935. The objectives were to: (1) train competent leadership in wildlife work, (2) carry out research, (3) extend education in the field of wildlife conservation and management through demonstrations, lectures, and publications, and (4) to render technical assistance to state fish and game departments.

The program was established at a time when it was sorely needed. There were few trained wildlife specialists since no schools at that time gave formal courses in wildlife management and procedures. Only the basic sciences of ornithology, mammology, and other courses in zoology and botany were taught. Many urgent research problems were calling for solution. Darling conceived a three-way cooperative approach consisting of the Federal Service, State Fish and Game Department, and the state land grant college or university. The Wildlife Management Institute (or its predecessor) also has continuously been a contributor.

In 19 years since its inception, 2,187 majors have graduated from seventeen of these cooperative universities including 1,581 with B.S. degrees, 545 with Master's Degrees, and 61 with Ph.D. degrees. At the present time other schools recognize the importance of wildlife and fishery conservation so that now these seventeen designated "cooperative unit" universities represent only about one-third of the academic fish and wildlife offerings now available. About three-fourths of the wildlife graduates are now employed by the several states, 10 to 15 per cent by the Federal Service, and 5 to 10 per cent are employed by private agencies engaged in wildlife research or management. These graduates are now employed in wildlife work in every state of the union. Canada has two universities giving a full wildlife academic program and several others giving some courses.

The first "Unit" school graduates were available the first year the Federal-Aid program started. There seems little doubt that these "P.-R." and "D.-J." programs have been very much more successful because of the trained young men that were available for this specialized employment. That the graduates have been successful may be shown from the fact that currently graduates are directors of 7 state game departments, 2 are assistant directors, 12 are "P.-R." coordinators, 15 head divisions of the game and fish departments, 6 are university Unit leaders, more than 50 are on college faculties and several serve as heads of departments, and many are in responsible positions in Federal services.

Much outstanding research on nearly all phases of wildlife—ecology, life history, and management—has come from these workers. While all of the work has not been of monumental proportions, or of lasting merit, the over-all benefits have been of inestimable value. Certainly this program cannot now be emasculated or given a lingering strangulation by having its management transferred to regional directors who neither want it nor are prepared to handle it. A coordinated nationwide program of this size needs unified management and administration.

PUBLICATIONS

The list of books, bulletins, and articles published in the field of wildlife and fisheries during the past twenty years is simply astronomical. Many of them are outstanding and will live. Others were good for their day, but will be superceded by subsequent advancement and works of far better quality. Time and space can permit only passing reference to very few of even the most praiseworthy of these contributions in addition to those already referred to. Because of the mature philosophies expressed, excellence of writing, and patterns set for later workers, Leopold's *Game Management* (1933) has perhaps wielded the greatest influence. Likewise Stoddard's *The Bob-white Quail* (1931) was a monumental milepost. The series of books by Dr. Ira N. Gabrielson on *Wildlife Management,* (1951), the *Wildlife Refuges* (1943), and *Wildlife Conservation* (1941) certainly will live because of the influence they have had in shaping policies and procedures and in developing a national concept of the importance of wildlife. Perhaps no one else has been so effective in making America conservation-conscious as has J. N. Darling and his little essay on *Poverty or Conservation* (1935). This should never be forgotten. Walter Cottam's *Is Utah Sahara Bound?* (1947) is an outstanding analysis of a state problem, and Vogt's *Road to Survival* (1948) is unexcelled as a world-wide analysis. As a state publication on the effects of cover and habitat destruction the bulletin by Scott is well worthy of mention (Scott, 1951).

In my opinion America has produced no lovelier natural history literature nor one which expressed more worthwhile esthetic values of conservation than that contained in Aldo Leopold's *Sand County Almanac* (1949). I should like to have the book rechristened with a more fitting title. This title was chosen by the publishers after Leopold's untimely teath.

Technical procedures or life histories are well summarized in such worthy books as Bent's *Life Histories of North American Birds* (1946-53), Seton's *Lives of Game Animals* (1929), Howard's *Territory in Bird Life* (1948), Kortright's *The Ducks, Geese, and Swans of North America* (1942), and Trippensee's *Wildlife Management* (1948 and 1953). Dr. Durward Allen's *Our Wildlife Legacy* (1954) is one of the best written and most understandable volumes yet to appear on the over-all problem and philosophy of wildlife conservation. His *Michigan Fox Squirrel* (Allen, 1943) is a model for clear writing and treatment of a species, and his handbook for Boy Scouts (Allen, 1952) probably will ultimately wield as favorable an influence in conservation as anything yet written. A good treatment of a species may be found in Hochbaum's *Canvasback* (1944), Murie's *Elk* (1951), and Einarson's *The Pronghorn Antelope* (1948). I am less competent to appraise the great number of good-to-excellent fishery papers, but am favorably impressed with the scholarly text, *Fishery Science* by Rounsefell and Everhart (1953).

Perhaps the greatest influence may come from the great array of shorter articles such as those printed in the *Journal of Wildlife Management,* the *Transactions* of North American Wildlife Conferences, and special reports and state magazines or papers, bulletins, and monographs by universities or the Federal Service.

NEEDS

I believe our greatest unsolved problem and therefore our greatest need in the wildlife conservation field is to devise more effective means of selling conservation to the public. Our problem basically is the same that industry has to face but they have done a better job in selling their product than we have. John D. Rockefeller once said, "I will pay more for the ability to deal with people than for any other ability under the sun." Certainly we must recognize that wildlife management involves far more than meeting biological needs of wildlife. It requires the management of human activities which affect wildlife resources. Good public relations are fully as important as sound programs and brilliant research.

In-service training is sorely needed to help develop this ability to deal with people and to get across to the personnel the research findings and their application to management. Such training is needed to develop a feeling of unity within the organization and to afford the membership opportunity to improve the organization and its program. Until the membership are themselves properly informed concerning policies and findings, they are not in a position to teach these to the public. Good in-service training is essential to good teamwork.

More support is needed in educational departments of most wildlife agencies. In my opinion the Missouri Conservation Commission has developed a very effective program in this field. They have ten college-trained field staff members appropriately distributed over the state, working directly with schools on all levels assisting teachers and administrators in planning and integrating conservation education in the present-day curriculum. Through workshops they combine lectures with field work. In addition they have a staff of eleven field service agents whose principal functions are to hold adult meetings, give field demonstrations and render field assistance to related groups such as county agents, Future Farmers of America, 4-H Clubs, Scouts, vocational agricultural groups, and others. All of the conservation agents, who represent the law-enforcement arm of the Commission, also carry on a sizable program of informational and education work with schools and with youth and adult groups.

Through radio programs, TV, news releases and direct contacts with clubs and conservation groups, the Commission constantly preaches the gospel of conservation. A splendid monthly magazine with a circulation of 86,000 keeps conservation problems and programs constantly before an interested public. The colored motion pictures are outstanding and they are reviewed by about 45,000 people per month. Exhibits, pictures, and library materials are of high quality and are in constant use. A similar program in other states surely would pay dividends.

The conservation story is in the public interest and it is worth selling. Despite the size of the problems ahead of us, great strides have been made. One might feel discouraged if he considered only the size of the job ahead, but to compare our present knowledge, attitudes, and the public understanding and support of today with conditions 20 years ago should make confirmed optimists of all.

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GENERAL SESSIONS

Tuesday Afternoon—March 15

Chairman: Frank L. Cambell
Executive Director, American Institute of Biological Sciences, Washington, D. C.

Vice-Chairman: Edward G. Pleva
Professor of Geography, University of Western Ontario, London, Ontario

CURRENT PROBLEMS AND FUTURE NEEDS

INTRODUCTORY REMARKS

Frank L. Campbell

Ladies and gentlemen, let us now begin the Second General Session on Current Problems and Future Needs.

I wish to introduce myself very briefly by introducing the Institute that I represent, because I am here only because of the position that I hold with the American Institute of Biological Sciences.

A number of you in this room are members of the American Institute of Biological Sciences, but many of you, I'm sure, are not, and therefore I think I should say a few words about it before we go on.

The American Institute of Biological Sciences was founded in 1948 as a part of the National Research Council of the United States. The Institute was felt to be necessary in order to bring together for common action the many relatively small biological societies, and so the Institute is a federation of biological societies, now numbering 19.

The member society that is nearest to the interests of this group is the Ecological Society of America, but the wildlife societies are not now members.

The American Institute of Biological Sciences exists to perform services for its members societies. For example, one of its services is the preparation of a register of biologists. That is done with the support of the National Science Foundation, which supports the same activity in other scientific organizations, so that a national register of scientific personnel will be produced.

Very recently the American Institute of Biological Sciences, which, as I said, was founded and set up in the National Research Council, voted to separate from the National Research Council and become an entirely independent organization. This was accomplished about two months ago by incorporation of the Institute. Therefore, there will be a new executive director of the Institute in the near future, and I will remain with the Division of Biology and Agriculture of the National Research Council, as its executive secretary.

Now, I sincerely feel very fortunate and grateful to be here today, because I need much education in the field of conservation, and it happens that the Division of Biology and Agriculture is about to go into the conservation field seriously, and hopes to play a significant role in the development of conservation programs with the support of Resources for the Future.

The first meeting of the National Research Council on the organization of the Conservation Board will take place this coming Thursday, and so you can see how much I needed to know what I have been learning here and how much I need to get acquainted with the people in this field.

This session will be conducted very much as was the first General Session. I will introduce the speakers on the program, and at the conclusion of each speaker's talk the discussion will be called for by the Vice Chairman, Dr. Edward G. Pleva, Professor of Geography, in the Department of Geography of the University of Western Ontario, at London. He will encourage discussion, and also control it as to time.

Now we are ready to proceed with the first paper on the program, by Dr. Keith R. Kelson, who is the biologist in the Division of Scientific Personnel and Education of the National Science Foundation. Prior to coming to Washington to take this position, Dr. Kelson was a research biologist at the University of Kansas, and before that on the staff of the Department of Biology at the University of Utah.

RESEARCH—A KEY TO IMPROVED RESOURCE MANAGEMENT

KEITH R. KELSON
Division of Scientific Personnel and Education, National Science Foundation, Washington, D. C.

Once upon a time there was a man who wrote of his own experiments that they were of no more significance than a child's play. Among the things he did, many seemed at the time ridiculous or at least naïve. The results of his "foolish" activities were carefully recorded and eventually filled many notebooks. That man was Michael Faraday. Much of what is known about the primary behavior of electricity is to be found in those notebooks. And so it is with all of science. Ignorance's outpost line of resistance is always first by the unorthodox simply seeking information.

The foundations of science are a solid monolith of objective and empirical fact. It is the basic researcher that builds that foundation. Facts are, for all practical purposes, bits of truth and the discovery of these facts—these truths—is a solemn responsibility. For without them science cannot exist. Further, to belabor the metaphor, continuous growth of science's structure, requires continuous additions to the foundation. Even within the enlightened period of the last quarter-century we have seen in some areas major structural failures directly attributable to the cessation of basic research. Unquestionably this was an important reason for the collapse of the superb German technology during World War II. Occasionally—perhaps even rarely—facts are discerned in a flash of insight, but more often, as most of us are uncomfortably aware, they are perceived only after the most laborious search. One often hears that the real advances in science are made by a comparatively few geniuses. Like many other dicta, this is but a half-truth. As Ernest Nagel put it: "Such accounts are a serious misrepresentation of the actual course of history, and of the nature of scientific inquiry. . . . They ignore the work of a multitude of unnamed inquirers, each laboriously exploring some small and apparently insignificant terrain in order to produce competent evidence, for or against some system of ideas or in order to prepare materials for such a system." Nor should we look with utter and complete disdain toward those who fail in their task. For, again as Nagel said: ". . . the failures and apparent cranks in the quest for knowledge have not all been fools or ignoramuses." When thinking of some persons who have failed, few of us can repress the guilty thought: "There but for the grace of God go I." And there have been times when some of us, with reddening ears, could not say even that. Be that as it may, many of us here have spent so many apparently fruitless hours wielding our own little intellectual grubbing hoes. It was necessary but noisome, benumbing toil of research to which Edison alluded when he stated that science is the result of 10 per cent inspiration and 90 per cent perspiration. Even that mental condition which recognizes truths in a flash of insight, as it were, is the result of long conditioning and cultivation. It is most unlikely, for example, that I, as a biologist, will intuitively discover any new understanding of the nature of the nuclear meson.

Once a presumed fact has been adduced (and I think that the way in which this is done is relatively inconsequential), the very quintessence of the methodology of science is brought into play. Our bit of datum must then be subjected to careful scrutiny in the harshest, coldest, most revealing and unflattering light possible. This is what is really meant, in the broadest sense, by experimentation. Then, and only then, if no flaws are detected, may we assume that before us lies a bit of naked truth. But this experimentation and testing, too, is the province of the researcher. And it is a very high order of research indeed.

The whole spectrum of science is predicated upon the assumption that nature is fundamentally orderly. We must make this assumption for otherwise science becomes chaotic and completely nonsensical. There is no evidence as yet, however, that we need have fears on this score; our assumption seems wholly tenable in the face of pragmatic experience. If we grant the validity of our assumption it follows, then, that each bit of datum is unique; each occupies its own special niche and is irreplaceable. It also follows that if we are dealing with actual truths, no two of these can be antithetical. There can be no scientific paradoxes. The appearance of a paradox must be taken as *prima facie* evidence that either a human interpretation is in error or at least one of the elements involved is not a fact. It is most curious how often this simple and obvious point has been overlooked by the unwary.

Each fact is a keyhole through which we may obtain a fragmentary impression of Nature's structure. Some keyholes permit a glimpse of the foundations; others, a glimpse of the framework or even the facade. In the field of resource management we are suffering from an acute need of more and bigger keyholes with more sharp eyes peering through them. None of the keyholes is unimportant. Each offers us a unique view, even though restricted, of a structure that we affirm exists. Can we dispense with facts that we have called keyholes; can we proceed further without the researcher who discovers the facts? Can we possibly understand the nature of the structure without knowledge of its parts? Now I challenge you—to quote a famous phrase—what is so mere about a fact—any fact? And what is so indispensible as the researcher?

May I digress a moment to point out that there may be kinds of knowledge other than objective? Even so, granting an orderliness of nature and natural processes, there can be no conflict between knowledge obtained objectively and that obtained intuitively. Rather, it may be possible to think in terms of two kinds of openings through which the edifice may be seen. Recognition of this concept might have forestalled many an acrimonious debate between the scientist and the theologian.

But there is more to the structure of science than its foundation of objective knowledge. And there is more to the management of resources than the accumulation of data. Quite as important is the superstructure of theory and generalization. If one looks through enough keyholes one begins to form some notion of the nature of the structure on the other side. One almost automatically forms ideas about the probable relationships of parts seen separately. And is not a prime objective of our myopic peering the understanding of the structure at large? Can we be content, either as a body scientific or as a body politic, only to search for new and larger keyholes? It is the vogue in certain circles today to decry as idle speculation any attempts to interpret or extrapolate beyond data. True, some attempts are indeed no more than speculation, but I must vigorously protest that all or even most are necessarily idle. Would we have been content with Galileo had he confined himself to observing the results of balls rolling down inclined planes? Or Darwin had he confined himself to discovering and collecting facts about the variation and geographic distribution of organisms? Or Newton whose work was very largely interpretive? Or Faraday who sought constantly for the meaning of his facts?—a search that led to the formulation of the laws of electrical induction, electrostatics, and magnetics—a search that led on the one hand to the invention of the dynamo and on the other to an almost unbelievable amount of further investigation. I think it is abundantly clear that the gathering of the facts, necessary as it is, is, by itself, a sterile pursuit. Let me hasten to say, however, that I am not pleading for a special license to theorize and generalize indiscriminately. Nor can we permit ourselves to dash ahead too hastily. We must always submit ourselves to the restrictions imposed by the rules of evidence and rigorously logical intellectual processes. Nonetheless, theories and generalizations, either as interpretations extrapolated beyond the bare facts or as instigators and guides of further research, are quite as respectable as the facts themselves. It was this that Whitehead had in mind when he said that nothing is as eminently practical as theory. His was perhaps an overstatement, but still I think we must try to weave the threads of fact into a fabric of understanding. Let me also say that, although I do not welcome mistakes in theory and generalization any more than I welcome misapprehended fact, still even those mistakes have a certain value. Inevitably they serve as the focus of a later, more sophisticated inquiry. It is well that we respectfully inter such blunders in more than Boot Hill. And we would be well-advised that we defer interrment for a decent interval. For, difficult as it is to know what is fact and what is not, it is even more difficult to predict the nature of facts as yet unknown and the course of future knowledge and development. Think back for a moment over the major scientific developments of the 19th century. In that century two important concepts were completely, ignominiously, and irrevocably laid to rest—the theory of the mutability of the elements and the corpuscular theory of light. Lo! the dead have risen. And many another toe is beginning to show on Boot Hill.

Research and science, however, do have their finite limitations. We, possibly more than other scientific bodies, should be aware of this. For our field in its manifold ramifications touches the very quick of human welfare. The facts may well indicate what can be done, how it can be done, and ideally, what the consequences might be, but neither facts nor science at large can ever positively decide what *ought* to be done. Let me illustrate with an anecdote. In December of 1953 some 1,600 persons met in Washington to take a long hard look at our resources for the future. In one of the sectional meetings a most competent group of specialists were discussing management of wetland areas. After listening for some time to various conflicting points of view, Mr. Van Valkenburg rose to the occasion. "I am from Holland," he said. "At home we had to decide whether our country was to be for waterfowl or Dutchmen; we selected Dutchmen." His brief statement contains the essence of an important idea and one which we tend to misplace now and then. How shall we think of the hordes of bison that once populated our western plains? Although we might regard the manner of their virtual extirpation with distaste, should we bemoan their eventual fate? Would it be better if they were still with us? Better for whom? The big-game hunter? The biologist? The wheat farmer? But the bison is past history. How shall we look at the wolf of the northlands today? We must also think in specifics as well as at a policy level. What are we to say about balance in nature to a stockman who has just lost a dozen blooded ewes to a coyote? How are we to look at the problem of commercialization of the National Parks? They are *national* parks, after all. Surely involved here is a matter of esthetics—of value judgments—as well as of scientific knowledge. At a certain forest conservation meeting, as reported in an issue of *The Weekly Digest* last year, one of the speakers was a big, heavy-shouldered outdoorsy type. He plunged into his subject with vim and vigor and, after berating his audience for an hour for their forest misdemeanors, demanded of them, "Just what have you done to help us preserve our forests?" His audience sat in shamed silence. Then the inevitable small voice was heard in the back of the room: "I shot a woodpecker once." Only thinly veiled by the humor of the remark is the fact that a value judgment was involved.

Science alone cannot solve all our problems and we who are aware of at least the scientific aspect of resource management must be the first to understand this. An important part of the successful management of natural resources is the successful management of humans and its corollary, good public relations. We do believe, however, that science must provide facts so that value judgments will be judgments in fact and not guesses; that is to say, the worth of a judgment is dependent upon both the perspicacity and disinterestedness of the intellect that produced it and the quantity of fact that shaped it. Then, too, human values changes, and quite properly so; what we may regard, in the phraseology of Pangloss, as the best of all possible actions today may not be the best tomorrow.

I have already noted that resource management, like the field of education, touches everyone. Everyone, therefore, is his own expert. How often have you heard the solution to a complex problem in big-game management propounded by a clerk who each year spends three days afield with a gun. Or the sportsman-angler who *knows* that planting more fish is *the* answer to declining catches. Or the farmer whose own land is eroding away as he argues with the greatest conviction the evils—or virtues—of erosion-control techniques. Such arguments and panaceas, too often based on ignorance and its partner prejudice, are of great concern to those dealing with the management of resources. Under democratic systems the ultimate judgments of what is good and what is bad about the utilization of resources and the goals thereof are products of public opinion. Thus the level of public knowledge and prejudice cannot but be a controlling influence or management policy. One thing, then, is crystal clear. The resource researcher has the solemn and irrefutable responsibility of not only adducing facts and their implications but also of disseminating his information. The public at large—or at least the active part of it—has a right to expect and must have sound data upon which to base its conclusions.

There is a currently popular belief that society should be willing to support science for its own sake. This belief seems to me to be wholly untenable. It is tantamount to saying that society should be willing to support science because science is a good way of life for scientists. I do not see how we can accept such a concept as a sound basis for science or any other major human activity. Science has long since obtruded itself into the very stuff and substance of human affairs. It must now accept the grave responsibilities inherent in its central position. I do not wish in any way to imply that we should eschew basic research in favor of something more esoteric; I have already described basic research as fundamental to science's structure. I do affirm that scientific research cannot be thought of as a tranquil pool quite removed from the riptides of society's activities.

And so we return full circle to the researcher. Sound resource management requires, in sequence, basic research, applied research and experimentation, public education, and finally, the application of those techniques the results of which are judged to be good. It follows that the individual researcher must accept the weighty mantle of social responsibility that his basic, authoritative position confers upon him. At this point I would enter the strongest plea that the prospective researcher be afforded the broadest possible education as well as thorough training in the technical skills. Further, if we accept the principle that knowledge tends to remove prejudice and improve judgments, then we must also accord him the privilege of expressing his own judgments.

We have seen that the researcher and his facts and theories are a *sine qua non* of resource management. But true mundane wealth consists of but two ingredients—human intellect and natural resources of other kinds. It is our goal to bring the one to bear upon the other. The stakes for which the resource manager plays are far, far greater than any other known to man because, in the final analysis, we are dependent upon the land and its resources. Nature exacts an inexorable retribution for our blunders. Soil lost cannot be replaced tomorrow; a forest area, once denuded, cannot easily be restored to its pristine condition; a wilderness area, once invaded, is no longer a wilderness area; a dry well or an exhausted mine is its own silent epitaph. How much of nature's resources are we to leave to posterity? Do we have a moral right to squander our wealth? Clearly, the resource researcher is also a *sine qua non* of future progress. Without him we shall be condemned to wallow in the bliss of our profound ignorance, and, to quote Dr. Kirk, President of Columbia University, "That ignorance which may be bliss is no proper basis for the operation of a modern democratic society. . . . Unless able men lead us, unable men will."

DISCUSSION

DR. J. V. K. WAGAR [Colorado]: Our speaker raised an interesting point as to the relative value of bison and human beings. He spoke of science and introduced, I think, without labeling his statements that he was edging on sentiment, a philosophy. I shan't speak of ducks versus Dutchmen, although I have a good deal of Dutch blood in my veins, but when it comes to bison and human beings, we have in some parts of the West more people than we know what to do with.

We suspect some of our people have less value than the bison. We'd like to get rid of a few people, and have a few more bison.

I think we are raising a question of values, namely, how many of each do we need, and what is the over-all fact?

I solve nothing by this comment, I think. I merely prolong.

DR. KELSON: It's self-answering. Thank you.

CHAIRMAN CAMPBELL: The planners of the program of this session had a most brilliant thought when they chose Mrs. Katherine Jackson to be the speaker on "Public Understanding of Resource Values." I think they had in mind that there should be a layman on the program, and Mrs. Jackson regards herself as an interested layman.

I can tell you that she is not only interested, but she does have a great deal of knowledge about the subject, because I had the pleasure of meeting her this morning and talking with her.

Mrs. Jackson is the Chairman of the New Hampshire Natural Resources Council, Peterborough, New Hampshire. She just completed a term in the New Hampshire State Senate, and she asked me to be sure to mention the fact that she is a member of the Board of the American Forestry Association.

PUBLIC UNDERSTANDING OF RESOURCE VALUES

KATHARINE JACKSON[1]
New Hampshire Natural Resources Council, Peterborough, New Hampshire

Your speakers for this three-day session and all of you here attending the North American Wildlife Conference are scientists or specialists in your own particular field of activity, and are also knowledgeable in all the broader phases of conservation. You have studied the history of our land and water, and you can envision the future, this future of the atomic age. In your midst I am just a layman, but very happy to be here to give you a layman's approach to my subject, "Public Understanding of Resource Values."

Of course, for the successful application of all your knowledge you must have the cooperation of the people, and public understanding is of utmost importance. In the last few years interest in conservation has been growing, but generally there is very little understanding of the whys and wherefores of our natural resources.

The great question is, then: How best to further public understanding that will give our people the desire to cooperate with you? This means *education in its broadest sense, founded upon the knowledge of the interrelationship of all our natural resources.* This understanding of the interdependence of all your specialized fields, call it ecological relationship if you will, is the modern approach to all natural resource problems.

A little boy was walking through the woods with me in the springtime when he spied a painted trillium growing on a bank nearby. "Oh, see that beautiful flower", he exclaimed. "Is it wild?" "Yes," I said. And he replied with disappointment, "Too bad! Then I can't pick it!" This child understood that this flower must be left there to go to seed or there would be no more next year. He had still to learn about all the factors in this flower's environment that are necessary to its growth.

The sportsman is interested in carrying home his full bag of fish and birds and deer; the farmer wants more dollars from his acres; the forest owner wants immediate returns from his investment; the city dweller demands all the water he can use. All of these men, with apparently different interests—How can you best bring them to understand that they are all dependent upon the same natural resources, soil and water?

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[1]Mrs. William K. Jackson.

Formal conservation education is well underway in probably every state in the Union, in primary schools, high schools, colleges and postgraduate courses, camps, and workshops. These must be broadened and intensified and are foundation-stones upon which to build. But they are only a beginning in this broader education. Your attempts to arouse the interest of all our people, I feel, can be attained only by using the following three essential methods:

First, you must talk the simple language of the people.

As a State Senator I attended a conference of all the teachers at the Keene Teachers College to discuss a bill introduced in the New Hampshire Legislature to provide tape recordings of lessons for use in our schools. After the meeting, one of the teachers asked me, "What field of education have you specialized in?" I answered, "None". And the teacher replied, "Then you could scarcely have understood what we were talking about." Probably every one of you, in talking to each other, use words unintelligible to the average person—a manner of speech popularly called "professional jargon." To reach the average man, you must talk his language, not yours.

Second, conservation must be given popular appeal.

It can be made dramatic and human. All of you wildlife specialists have a jump on the gun, as animals make a natural appeal to almost everyone. For generations children have delighted in the tales of Thornton Burgess's animals with natural attributes and human voices, and have wept at the vicissitudes of Ernest Thompson Seton's creatures of the wild. More recently several eminent writers like Fairfield Osborn, William Vogt and Neil M. Clark have dramatized conservation with such startling titles as "Are We Starving To Death," "Hunger At the Peace Table," and "Our Plundered Planet." Louis Bromfield, with his "Malabar Farm" and "Pleasant Valley," has put romance into the land. The forestry societies dramatize the horrors and waste of forest fires; the Audubon Societies appeal with the beauty of birds; the Izaak Walton League delights the imagination with pictures of fish leaping from cool streams; the Connecticut River Valley Association's slogan is "Your Valley—Your Future"; the Wildlife Federation heads a pamphlet "A Desert in Your Own Back Yard." All this is making conservation dramatic and human.

The *third* means of arousing public interest, and after all the most effective, is to *show concrete results that add up in terms of dollars and cents.*

The man who owns the land in the watershed is the one who controls the water. He, like the man from Missouri, must be shown the interrelationship of all life in his valley, his own immediate part in it, and the profit there may be to him in cooperation. This method is being wisely used by Soil Conservation Districts and such watershed groups as the Brandywine Valley Association and the Muskingum Conservancy of Ohio.

These three ways of spreading conservation knowledge must be more widely used by the vast number of organizations carrying the banner of conservation. The sportsmen are well organized and vocal, and they represent a crosssection of the community. They can be brought easily to understand the value of the forests, soil conservation and clean water. The Women's Clubs and Garden Clubs have come a long way since the days when the annual lecturer on birds summed up his talk by advising each woman to go home and put a bird-bath in her garden and a bell on her cat. More recently, with modern thinking, watershed organizations are doing a practical job of teaching the complete interrelationship of all the life in their valley.

The new Hope-Aiken Law, providing for federal aid for works of development and conservation of water and soil in small watershed units, offers the United States tremendous possibilities in the immediate years ahead in those areas where there is general understanding of its purpose and the support of the people.

State-wide councils bring together organized groups interested in various phases of conservation for the purpose of stimulating and coordinating their work. Such a one is the recently organized New Hampshire Natural Resources Council. It includes in its membership state-wide, regional, and local groups, industries and individuals. The meetings, with open discussion, are the principal activity of this Council. Panel speakers on different sides of the same subject are invited to each meeting to answer questions, thereby increasing interest and understanding of New Hampshire's own local problems. The success of these monthly meetings I attribute also to the sociable, friendly atmosphere where each member is introduced, the supper preceding the 7 to 9 o'clock meeting, and the fact that although everyone will have a chance to speak, they are limited to a few minutes and can expect the meeting to end on time, so that those coming from a long distance can reach home that night.

This Council sponsored a book on conservation for elementary schoolteachers, prepared by the State Education Department, and have watched with interest the experiment of taking the whole sixth grade from some of our public schools to spend a week in camp to learn about the out-of-doors, at the same time continuing their usual studies.

It has sponsored the Youth Conservation Camp at Bear Brook State Park, and the boys to be sent to the Izaak Walton League's Conference in Chicago, cooperated with the State Fish and Game Department and the Sportsmen's Clubs in creating a better sportsman-landowner relationship, discussed with the State Development and Planning Commission and the Hotel Owners' Association the pros and cons of the recreation business as affecting the wise use of our State's natural resources. It has visited the Baboosic watershed being developed under the Federal pilot watershed program, and has held intensive discussions of the big dam versus little dam controversy.

The Council actively supports New Hampshire's Forestry Conservation and Timber Tax Law, and now has again a fight against its repeal in this session of the Legislature. This law exempts growing timber from taxation and provides, instead, a severance tax and reimbursement by the state to the towns for the loss of their former revenue. An abatement of the tax is offered for carrying out approved cutting practices. It is said that this law has taught the people of New Hampshire more about good forestry practices than they could ever have learned in any other way.

My time is about up now—but I want to urge every one of you to take time to integrate your specialized subjects into a picture, a rounded picture for the average citizen. This picture must be presented in attractive form, with human appeal, and be practical—practical in terms of dollars and cents. Only in these ways can we gain public understanding and cooperation in wise use of our natural resources.

DISCUSSION

VICE-CHAIRMAN PLEVA: Is there anyone who wishes to comment on this paper?

MR. J. L. VAN CAMP [Canadian Forestry Association, Montreal]: I would like to congratulate the Wildlife Management Institute on their wisdom in selecting Mrs. Jackson to address this meeting. I think it is a direction in which our Associations in Canada might well go.

The comment I wished to make was in relation to the demonstrations which Mrs. Jackson mentioned, and which are so plentifully found in her own home state.

The one exception which I think was obvious to Canadians, and particularly to those of Ontario, is the group of authorities in Ontario developed by the Department of Planning and Development. I don't see any representative of that Department here, but I would like to say that they are certainly worth visiting. Their publications, including their very detailed records and their summary reports, are worth having in your files, and along with Dr. Pleva, who, I'm sure, knows them well, I would like to suggest that you acquaint yourselves with them.

MR. GUSTAVE PREVOST [Montreal]: I would like to have some figures on what would be the present worth in money that we should use for public understanding.

Supposing that you have a group capitalized at $100,000; how much should be spent for public understanding?

MRS. JACKSON: I don't think I'm the final authority on that kind of thing at all, who finds it hard to balance a checkbook. [Laughter] I think one of our scientists ought to be able to tell us, but scientific research is, of course, of tremendous importance.

I really don't grasp the question as I should. Is the question what percentage of the $100,000 should be used for scientific research, and the rest for public understanding? Is that it?

MR. PREVOST: Yes.

MRS. JACKSON: I think that Dr. Campbell and I are great friends, and I would not want to answer that question, because I think so much of it should go to public understanding, and I'm sure he'd give a great deal more to scientific research. Will you answer that?

CHAIRMAN CAMPBELL: I would be glad to relieve Mrs. Jackson of the responsibility of standing before the microphone, but if I had $100,000 to dispose of, I could guess what portion to spend on that, but it would be my guess, and not based on anything worthy of your consideration, I believe.

You see, I am on the spot now. [Laughter] How can I guess on that? There are so many factors involved. It would depend, would it not, on the use to be made of the money, for what purpose, on the extent to which research was necessary. If you were dealing with a subject in which one merely had to make use of information already at hand, the percentage devoted to public information would be very large. Maybe it would be worth while to throw the whole $100,000 into the pot. That would be my answer to it.

VICE CHAIRMAN PLEVA: I wish to commend Dr. Prevost for asking that question, because, as I looked around, I believe everyone was making mental calculations and trying to decide how he would divide that sum.

MR. FAIRCHILD OSBORN [New York]: I'd like to ask the chairman, Is the money a reality or is this imaginary money? [Laughter]

I'd like to point out earnestly the following few observations. First, I think it's brutal that man—civilized man—has at all times during his history known more of the methods of agriculture—and I will use that field—than he has practiced.

I would further remark that in our own organization, the Conservation Foundation, we are active in two fields. We endeavor to keep them reasonably balanced. One is research; one is education. On the research end of our work, we are, of course, as is any research division, tremendously aware of the presently existing voids of knowledge. On the other hand, we are still, I maintain, not as a people living up to the knowledges that we already have, and until we do, the advancement of research beyond that of broad general practical understanding—the lag lies over on the left, rather than on the right.

I would divide the budget accordingly. For the year 1956 I would apply $37,500 to research, and the balance to education. [Laughter and applause.]

VICE-CHAIRMAN PLEVA: I would like to ask Mrs. Jackson a question. In Chicago last November I heard a woman member of the Legislature of New Hampshire speaking about this forestry law as a triumph in adult education. I am a little alarmed, because Mrs. Jackson intimated that this law is in danger. I wonder if she could tell us what has happened.

MRS. JACKSON: The law, as it was drawn up and passed originally, was a compromise. As you know, in New Hampshire our towns have a great deal of authority, and they also have to have their revenue. Therefore, practically every town father opposed the law originally, fearing that they would lose this income that they had been receiving from taxing growing timber—land with growing timber on it.

So this provision was put into the law, to read that from this severance tax the towns would be repaid, and they were to be repaid on the basis of the value of that growing timber in those towns at the time the law was passed.

Well, you can imagine the complexities that have arisen since. and in carrying out this law it has been very, very difficult. It has cost the state a great deal more than was expected, and some towns have been getting far more than they deserve, and others are getting less, and there is a great deal of discontent.

There are those who are very anxious to repeal the law, and I happen to have lived in a town that has a member of the Legislature who says that he is determined to get the law repealed, and it has very, very bitter opponents.

VICE-CHAIRMAN PLEVA: Any further questions?

MR. PREVOST: I am very pleased with what my colleague said about that $100,000, but maybe not everyone here is aware of an investigation that has been made of a few months ago among different departments in the states about how much money they were spending on education; and if I remember well, the figures were about from 2 to 8 per cent, and everybody complained about the amount, so I think 25 per cent would suit most of them now.

MRS. JACKSON: I would like to introduce to you John Dodge of our Fish and Game Department. He's sitting right over there, and he's in charge of conservation education in our state, and I think he could very well answer that question. Come on, John! [Laughter]

MR. JOHN DODGE [New Hampshire]: Nothing like being put on the spot by your chairman! [Laughter]

I would answer the question in two ways. Dr. Prevost has told you that the most recent surveys show 2 to 8 per cent as being what is expended now in the states across the nation. I believe that 6 per cent plus is regarded as a very fair allotment to the actual information and education divisions, but I would like to point out that in every area in which there is management work or propagation work being done by a state department that is in a very real sense education too, and therefore the research that is being carried on must take its place in line with the other aspects of the department's budget, so that the percentage is not actually as small which goes into moving public opinion as might have been indicated by the figures.

MR. LOWELL BESLEY [American Forestry Association, Washington, D. C.]: I certainly have been interested in the discussion of Dr. Prevost's question. I think there is one thing that we are not realistic about, as far as our publications are concerned, with respect to how much we are actually spending on the educational phases of getting research results across.

I think that if you consult anybody who is employed full-time on research in your Canadian Government or any of the provincial governments or the Federal Government or the state governments, you will find that a great deal of their time is spent in writing letters and answering people who want to know what they have found out, and anybody who is engaged in research and is alert is actually spending quite a bit of his time in educating the public, but that doesn't show up in the budget.

It is a definite part of the researcher's job. In fact I would say that the majority of researchers, certainly after they get to have any administrative position at all, are spending well over half their time in making available the results of their research.

VICE-CHAIRMAN PLEVA: Our time is up, and we will now proceed with the next topic.

CHAIRMAN CAMPBELL: I'd like to remind the speakers on the platform here that they are privileged to ask questions also, just as Dr. Pleva did during the recent session.

Now, Dr. Fairfield Osborne pointed out that we aren't using everything we know, and in doing so led directly to the subject of the next talk: "Are We Using What We Know?" That subject will be discussed by Mr. Bernard L. Orell, Vice President of the Weyerhaeuser Sales Company, St. Paul, Minnesota.

MR. BERNARD L. ORELL: Dr. Campbell, Members of the Panel, Members of the Conference, Ladies and Gentlemen: It certainly is a privilege to be here today to discuss this subject with you. Before going actually into my subject, however, I cannot help but comment on the discussion led by the two previous speakers.

As most of my life has been spent in the Pacific Northwest, I can't help but comment, Mrs. Jackson, on the fact that I now realize that there are two corners in God's country, one in the Pacific Northwest, and one in New Hampshire.

Dr. Kelson, I was interested in your story about the outdoorsy man who berated his audience with regard to what that audience had done in forest fire prevention activities and forest protection, and this small voice said that he had aided by shooting the woodpecker. Unfortunately, that man didn't do as well as he had thought he had done, because the woodpeckers are very important in controlling certain highly destructive insects.

RESEARCH—ARE WE USING WHAT WE KNOW?

BERNARD L. ORELL

Vice President, Weyerhaeuser Sales Company, St. Paul, Minnesota

The development of this subject must of necessity be in the nature of generalizations, because it is obviously so broad and covers so many aspects of natural renewable resources. Previous speakers have emphasized clearly the importance of research and the necessity for public understanding. This presentation probably will not bring anything startlingly new to this group, but it is hoped it will call to mind some essential elements which all of us have known but which have receded into the back of our minds or have been forgotten. During the course of the presentation an attempt will be made to answer the question, give specific examples, state the reasons, and establish the interrelation between the various aspects of the natural renewable resource field. The human relations problem as the foundation of wise use of the natural resources will be developed specifically.

Research has provided us with a wealth of basic material, though we will never have sufficient to handle the dynamic changes constantly taking place. This, coupled with experience, gives us a good foundation for application of principles. Are we using what we know? To a point, yes, but to a substantial and unfortunate degree, an emphatic no! This includes not only the technical fields, but that of the human resource as well.

Among the reasons for the lack of use of knowledge and experience available to the managers of resource facilities are insufficient funds, insufficient economic justification, the lack of concerted action on the part of interested groups, unfortunate public pressures along unreasonable lines, lack of specific research into an immediate problem, a decision favoring some other activity, and pressure to meet a specific local problem.

There are, of course, many ramifications of each. In general, however, any or all may apply to a particular resource field. Some specific examples serve to illustrate.

In the field of forestry there is adequate knowledge to justify the establishment of organized protection on all forest lands. Research and statistical experience show that fire losses on unprotected land are 25 times those receiving even extensive forest fire prevention and control measures. Despite this, some 60 million acres of forest land, principally in the southern states, do not have any protection worthy of the name. The reason? Lack of funds, an apathetic public, and a mistaken feeling among rural people that burning is essential.

Also in the field of forestry is the insect and disease problem, today taking a greater toll than fire. Examples of too little and too late are all too prevalent and include the spruce bark beetle of Colorado; the spruce budworm of Oregon, Idaho, and Montana; the Douglas fir bark beetle of the Pacific Coast; and many others. White pine blister rust was well on its way toward elimination of the North American white pines before adequate control measures were instigated. In some of these aerial spraying is the effective answer; in others, sanitation or salvage logging to remove the dying, dead, and down brood trees is essential; in still others the elimination of alternate hosts provide the only practical means of elimination. These are all means which are known to be effective through research into the life cycles of the various pests, yet delays due to lack of funds and inaccessibility resulted in untold losses. Important is the development of a system of access roads to presently inaccessible areas. While funds did come up sharply last year, we still are very far behind in the development of a system which will permit effective cleanup action on the epidemic stages existent in our forests today and to salvage those stands already killed.

Wildlife management and the poeple deeply interested in this field also have similar problems. Public pressures have forced artificial feeding of deer with the resulting over-stocking of range areas and the development of weak animals, ending in a probability of actual scarcity. Game biologists have generally established, and are generally agreed on the fact that seasons allowing the taking of either sex of big game animals are beneficial, yet too many states are still clinging to antiquated buck seasons. This is to the detriment of the herds as well as the lowering of game populations. Sportsmen, sincere in their beliefs, but unwilling to take the considered opinion of specialists in the field, are forcing the use of such antiquated game measures.

Among the most important of our natural resources is that of water.

Essential to all water usage and conservation are the watersheds at the headwaters of streams, as well as the lands downstream. These watersheds, made up of forest and agricultural lands, are terribly important to our entire economy as well as to hunters, fishermen, recreationists, and to industry. In the past and at the present time watershed control has placed its emphasis on structures rather than on the wise management of the basic watershed area. While structures may be important in some instances, and certainly specific examples for purposes of irrigation can very well be justified, generally speaking the real answer to our water problems is in the wise handling of the watershed areas. The reasons for the emphasis on structures? Again, public pressure, or the proposing of structures as a watershed or water-control measure when in actuality other reasons are really paramount.

In this same field there are others who would propose that a watershed area be inviolate so far as the use of the timber, grass, and soil is concerned. Water development then becomes its only purpose. This attitude we also know is not wise, not justifiable on the basis of our research and experience. Timber can be harvested, cattle can be grazed, and the soil values can be utilized without damage to the water-holding capacity of the watershed or the run-off variables that are involved.

These examples serve to illustrate the point that we are not using what we have gained from research, experimentation, and practical experience. There are many others that could be used with regard to waterfowl, upland game birds, soil, and other resources, but most of you are aware of these and further examples would be mere repretition.

Recognition of the interrelationship between the various aspects of any nation's natural renewable resources is paramount. Nearly everyone realizes this interrelationship, but too many, in the pursuit of a particular interest, fail to give the matter coherent thought. We all know soil is primary to all other resource management programs. The soil, along with the forests, provides the habitat for big game and much small game; the streams for fishing; the grasses and herbaceous material for forage; and much of the water for industrial, domestic, and recreational use. The management of any one phase when carried to the extreme without recognition of the total resource value could be detrimental to any one or all of the others. The carrying capacity of any given area for cattle or big game could be greately enhanced by complete cutting of all of the timber, following by successive burning on the other extreme. This would, however, be extremely undesirable from the standpoint of watershed value, other recreational values, and the economics of the region dependent on timber resources for continuing crops. On the other hand, wise use of forest lands for timber cropping through clear-cut or selective methods, depending on the species involved, provides a maximum of forest products for economic stability and opens the stands sufficiently to provide a reasonable carrying capacity for game.

One cannot discuss the wise use of our natural resources and the part that is played in the total of each phase without bringing into focus the most important resource in the use of knowledge and the results of application and experience. This is the element of human relationships.

We who are interested in resource development become intense about our beliefs and about proposals regarding the use of these natural benefits. As some are more interested in one phase than in others, there is bound to be a conflict of interests. These conflicting interests today are creating one of the most difficult problems in the orderly development of sound management programs.

The subject of human relationships is one about which more has been written but which is less utilized than any other resource we have available to us. The fundamental rules of dealing with others are common and well known. Yet too few apply them in their daily lives. Particularly is this true among the various factions dealing with natural resources. The basic element is as simple as that of treating others as we would be treated ourselves. We are dealing in things of nature. Nature works in concert. We humans who are deriving the benefits of nature must also work in concert if maximum benefits are to be obtained for all of our people.

There is no place in the conservation movement for blanket indictments of whole industries or of whole groups on the record of a few. The labeling of any group with such titles as "land grab gang," "special interests," or "exploiters of our natural resources," is as patently unfair as it is for a forest industry man to accuse all sportsmen or campers of being "irresponsible," "gun happy," "fire bugs," or "litter bugs," and therefore unfit to exercise the privilege of hunting on private land due to a few damaging lookouts, motorized equipment, and oil tanks, or setting dangerous forest fires. These blanket indictments thrown down from a rostrum or blared in the press make for fine sonics, but exclude any possibility of mutual interest or a hearing of the other point of view. Resentment becomes the order of the day, facts become distorted, and instead of working closely together in the solution of problems, the opposite is true. There is no place here for personal prejudice, suspicion, backbiting, dishonesty, or evasion.

There is a place in conservation, however, for controversy, criticism, straight talk, and direct action.

Many people decry controversy. Actually, it is good. It causes men to think, to develop better methods, and to draw sound conclusions. After all, adversity is the ladder up which men climb to greater achievement. We should look at controversy as healthy and as a proving ground for the ideas of a host of independent thinkers.

We all seem to feel we possess the inalienable right to criticize. Conversely, we almost invariably find criticism unpalatable. In presenting criticism, which is in fact our right and responsibility, it is extremely important that such criticism be presented with tact and diplomacy. If we are the recipients of criticism, it is absolutely essenial we profit by it no matter how the method of presentation. We should recognize the fact that when criticism is felt to be necessary, either misunderstanding or actual error was in evidence. This also indicates interest in what is being done. It is the duty of the conservationist to examine carefully every criticism rendered before rejecting it.

When discussing the matter of human relationships and their importance to the field that is paramount here today, a very important part of such relationships is the matter of bringing into the open false premises, unwarranted action, and unethical endeavor. Straight talk and straight thinking with regard to issues on which there is conflict and disagreement certainly are parts of progressive action in finding the actual solution. It is the moral responsibility of every individual to say what he thinks, forcibly, so that everyone knows what he thinks.

We have discussed the interrelationships of the natural renewable resources. Nearly everyone, regardless of his particular activity, is concerned with or interested in every phase of our great outdoors from an economic, aesthetic, and recreational standpoint. Individual human use is interrelated, also. Sincerity and a high ethical standard are traits most of use believe we possess. Shouldn't we assume the other person has these, too?

We are using a part of what we know, but there is a great deal of knowledge that has not been applied. This is particularly true with regard to relationships between individuals and groups representing various interests in the outdoors. Certainly even casual observation will make us realize that no one group has a monopoly on integrity, nor is any group completely free of persons who are unethical. We who are interested in natural renewable resources must realize that it is people—human beings—individuals—who will determine the course of the wise use of our resources on the North American continent. The reality of people is that they respond to sincerity, frankness, tact, interest, and facts.

The problem of working with people with divergent interests is a great deal like that of the young woman who rushed to her mother in tears three days before her wedding. "Mother," she said, "I cannot marry Joe. He is an atheist. He doesn't believe in hell." After a brief moment of thought, the mother replied, "Don't worry, honey. You go right ahead and marry him, and between the two of us we'll teach him the meaning of the word."

Working together with true recognition and understanding of the position taken by others, we can teach the "sons-in-law" in our public and in various groups the true meaning of conservation as wise use, and thereby develop an aggressive, active, coordinated program of resource management.

DISCUSSION

VICE-CHAIRMAN PLEVA: Mr. Orell's paper is open for discussion.

MR. STEPHEN RAUSHENBUSH [Washington, D. C.]: I'd like to ask this question. One of the things Mr. Orell thought we already knew and weren't applying was the fact that there was a great deal of upstream value from good soil conservation work, and that that therefore should lead to more of the upstream work and less of the downstream.

Well, I personally feel that way too, but I wonder what the actual facts are, as far as he knows or anybody in the audience.

Some eight years ago I asked the Soil Conservation Service of the Department of Agriculture of the United States whether they could prove that there was any particular upstream benefit from their soil conservation work, and at that time they didn't seem to be able to say that they had enough data to go on, so I wonder whether one could say that that is one of the facts that we have and are not using. Perhaps something has happened in the last two years when I haven't kept in touch with the problem.

MR. ORELL: Well, certainly there are some indications of the fact that proper soil conservation, both upstream and downstream, illustrate the importance of good conservation work. You can name any number of drainages in the West, as contrasted to drainages which have been mistreated, where the erosion has been almost nil, and it has been clearly demonstrated that the upstream effectiveness of conservation methods certainly works.

Now, actually part of what I was driving at in that particular part of my discussion was the fact that it seems to me that when we are talking about flood control measures, we ought to talk about structures for upstream flood control, and when we are talking about power we ought to talk about power, and when we talk about irrigation structures we ought to talk about irrigation structures.

It is my opinion that we have had too much justification for one reason when the other reason was paramount as the reason for building the structure, and I think this is particularly true in the West.

FUTURE POPULATION DEMANDS

WILLIAM VOGT
National Director, Planned Parenthood Federation of America, New York, New York

(Before reading this paper, Dr. Vogt ran a metronome in front of the microphone for one minute.)

At every one of the clicks you have just heard—50 to 60 in the last minute—the population of the world has increased by one hungry human being. This is not the number of babies born. It is the net increase—the excess of the number born over the number of people dying.

A consideration of future population demands, the subject assigned to me, involves more items than you would have to assemble, and more processes than you would have to coordinate, in planning a canoe trip to the Peace River Delta. Population is a function of geography, climate, genetics and ecology, in human beings as in other animals. And in mankind both population and demand are a function of thought and emotion as expressed by reactions to symbols in politics, economics, semantics, religions and a wide variety of superstitions. Obviously, 20 minutes allows time for discussion of only a few of them.

Of the many variables involved none, probably, carries a greater charge at the present time than the unprecedentedly rapid growth in human numbers. You have heard the metronome. A statement by P. K. Whelpton, of the Scripps Foundation, may help you further to visualize what is happening: he points out that if the world population had increased, since the beginning of the Christian era, at the rate it has during the past one hundred years, alongside every single human being on earth, there would now be approximately one million more! The world population is now 2.5 billion; by 1980 it may reach 4 billion.

The *distribution* of population increases (there are not enough population decreases to be worth talking about) and resource use—in time and space—are of enormous importance in this connection.

Contrary to the forecasts of economists and demographers, birth rates have not fallen automatically as material standards of living rose. In the depression years 1930-34, in the United States, the crude birth rate was 17.6 per thousand—in 1952, a year of peak prosperity, 24.5, or an increase of 39%. Holland, which has one of the world's highest living standards, in the same year had a birth rate of 22.4. Australia's was 23.3 and white New Zealand's 24.8 Ceylon, which is considered "highly urbanized," had a birth rate of 39.5 and Japan, despite about one million legal abortions a year, 25.4. The birth rate of the Province of Quebec, certainly another one of the high living standard areas of the world, stood between that of Australia and Ceylon at 30.3.

In relation to population and future needs, we cannot let our thinking stop at national boundaries. As this paper is written, the United States Government is projecting an annual expenditure of $3,500,000,000 for foreign aid. An important factor in creating the need for foreign aid, especially in Asia, Africa, Latin America and such backward countries as Italy and Spain, is overpopulation, or more people than available resources can cope with.

When Yellowstone has too many elk, the herd can be thinned out. When Japan has too many people, they must be cared for somehow. Japan, with an area smaller than California's, will probably have 100,000,000 people by 1970. Those 100,000,000 are almost certain to swing into the Soviet orbit unless they are heavily subsidized by the West. Subsidizing them will probably be less costly than would be defense of the Pacific with Japan a Russian ally.

I wish there were more time to go into these population trends. Mexico, for example, from which every year 2,000,000 to 3,000,000 "wetbacks"[1] cross into the United States looking for work because they can't get it at home, will in 25 years probably double its population to 50,000,000! El Salvador, just to the south, now living on an almost East Indian level, will double its numbers of people in 22 years. Hawaii, which may be admitted as our forty-ninth or fiftieth state, is out-Puerto-Ricoing Puerto Rico, and may double in 28 years! American Samoa will probably double in 21 years; Formosa (which we both defend and help to support at a cost of hundreds of millions a year) will double in 19 years, Ceylon in 28—and Latin America as a whole in perhaps 30 to 35. All these estimates are based on current rates of growth. Some may slow down—others will assuredly increase.

Let us consider for a moment the problems we should be facing in the United States if we were growing at the rate of Mexico—doubling in 25 years. Where, by 1980, could we find for 328,000,000 people—nearly the present population of India—available water for not only the new population, but increased supplies per capita that our growth of industry and irrigation is demanding? What would it cost in terms of increased capital and labor input to maintain our present

[1]*America*, "Current Comment," March 6, 1954, p. 587.

diet? Could we provide for 164,000,000 additional people in 25 years —homes, hospitals, doctors, nurses, highways, schools, teachers, forest products, maintenance for the vast portion? (We must remember that most of these people would be unproductive until they were 18 or more —not to mention the growing numbers of unproductive aged.)

We could probably get by, but only at two certain and enormous costs—a greatly lowered standard of living, and an appalling increase in governmental controls over many phases of our lives.

Yet we, in the United States, doubling in about 40 years, possess far more resources than almost any other people on earth. We have a stable, responsible, orderly and reasonably honest government. We have an educational system that, although like the Canadian system, brilliantly described by Dr. Neatby,[2] leaves much to be desired, is vastly superior to those of most countries of rapid population growth, We have, and shall continue to expand, a strong capital structure. We have a democratic system providing for the free and fruitful exchange of ideas that has resisted demagogic attempts to impose a sort of American inquisition. We have one of the most highly developed technologies in the world.

Few of the rapid-growth areas share even a small proportion of these advantages—either the resource base, or the socio-economic structure—to cope with the population explosion. It is hard to see how they can escape serious economic and social tensions.

For humanitarian reasons, and because the Communists lose no opportunity to take advantage of these tensions, the trouble spots—wherever they develop—become the problem of the West, not excluding the British Commonwealth. This means increases in both defense costs and economic aid—which means less funds for wildlife research, forestry, refuges, public hunting grounds, pure water supplies, national parks and many of the other things that help to make life in America worth while for us who are meeting here. Population growth in other parts of the world will be increasingly at the cost of our living standard.

As the demand goes up, and as most of the people of the world are demanding as their "right" an improved standard of living, what of resources to meet it? Obviously, more renewable and unrenewable resources will be required. (In this connection, it might be well to note that in the real world of people the distinction between "renewable" and "unrenewable" is largely unreal. It exists only in a verbal, philosophical, conservation-book sense, since out of the 2.5 billion people in the world well under 20 per cent—perhaps even less than

[2]Neatby, Hilda, "So Little for The Mind," Toronto, 1954.

12 per cent—are using their soil, water, forests and grasslands on a sustained yield basis. Because of human behavior, what we call "renewable resources," or what could be "renewable resources," are in actuality transferred to the non-renewable category.)

Here, again, there is a situation that is complex and that cannot be considered in isolationist, nationalistic terms. In Haiti people now starve to death. In El Salvador the levee between the flood of people and starvation is a one-crop economy based on coffee. India, with the help of several hundred million American dollars and unusually favorable weather, has slightly improved her economic position; India's Director of the Census, meanwhile, gives his country 15 years in which to strike a balance between births and deaths—or suffer a drop in her already low living standard.[3] Merely to cope with India's population increase will require $1,100,000,000 of new capital a year—this in a country with a total income of 22 billion, or about $60 a year per capita. She has some minerals; more water is available, generally at great cost; her forests and soils have reached the bottom of the barrel. One of the most eroded countries in the world, only one of her 29 states has a soil conservation program; its director told me, in 1952, that at the current rate of progress it would require 125 years for completion.

The U.S.S.R., according to current folklore a country rich in resources, is finding that even resort to slave labor cannot cope with a population growing 4,000,000 a year. "The estimated per capita output of edible animal products (such as meat and milk) in the Soviet Union has declined by about 30 per cent in the last 25 years. The grain is now at about the per capita level of 1928."[4] The labor battalions are now being sent to the really marginal areas such as Siberia where a sustained increase in production will inevitably be slow and costly.

Each region, each country, presents a unique problem. In few areas are long-range prospects good. None of them can be ignored by Mexico, the United States, and Canada. Indeed, geographers, such as Dudley Stamp, and agriculturists like Sir John Russell, who have tended to decry as "alarmist" the warnings of conservationists, are now saying that large increases in food supplies must be looked for from such temperate regions as the United States and Canada.[5]

How the Indian *ryot*, the Mexican *campesino*, the Haitian peasant,

[3] Citedin Anderson, S.W. "Population Growth and Capital Requirements," World Population Conference, Rome, Sept. 1954.
[4] Harris, Chauncey D. "Growing Food by Decree in Soviet Russia," *Foreign Affairs*, OC,B, Jan. 1955, p. 269.
[5] Russell, Sir E. John. *World Population and World Food Supplies*, London, 1954, p. 472; Stamp, L. Dudley, *Land for Tomorrow*, Indiana University press, Bloomington, Ind., and N. Y., 1952, pp. 212,214.

the African native, are going to pay for this food, no one has explained —or I have missed it. The current recommendation seems to be that the American, reducing his soil fertility by about 1 per cent a year, should give away the stores that have been created through the medium of the printing presses of the American Government's mint. Other nations, that are producing more than they need, at varying costs to their own soil fertility, understandably resent any suggestion of American "dumping" of politico-economically produced "surpluses."

Meanwhile, what of the United States?

There seems little doubt that for some years to come we shall eat enough though to many of us the quality of our food seems worse each year.

Water resources tend to become scarcer and more expensive, and as more millions of Americans continue to occupy lands of low carrying capacity, such as the Southwest, the people of the rest of the country are penalized not only by having to pay for moving water from one place to another, but by losing the sanctuary they have so long had in their national parks. The truce between the Democratic Hetch-Hetchy and Republican Dinosaur covered a period with a population growth of more than 60 per cent. The people beat the Republicans, but today, it is not at all certain the Democrats won't again beat the people.

In the high plains region of southern Texas, ground water is being used 20 times as fast as it is being replaced; yet in the drought a year ago the American Government actually paid Texas ranchers to continue overgrazing their lands, through so-called "drought relief." Most of us south of the International Boundary now live in towns, and probably the vast majority of us urbanites drink treated sewage instead of pure water.

There has been so much discussion of the water problem by people more up-to-date with their information than I, that I shall only remind you it is a serious and all but nation-wide concern. There is little evidence, so far, of a rational attempt to solve the problem. Resources, like population, seem to be a function of politics!

The Paley Commission report of 1952, showing that in certain resources the United States is already a have-not nation and is rapidly becoming more of one, was well publicized. Whereas at the beginning of the century we produced some 15 per cent more raw materials than we consumed, by mid-century we were consuming 10 per cent more than we produced.

According to an eminent Jesuit scholar, the Reverend Stanislas deLestapis, at the World Population Conference in Rome last September, the United States consumes 50 per cent of the world's unrenewable resources and by the year 1980—if the present rate continues—we shall consume 83 per cent.

It was apparent from Father deLestapis' speech that he disapproved any such eventuality, and he is far from being alone in his attitude. Lord Simon of Wythenshawe, in a recent letter to *The Times* of London, pointed out that one consequence of the recent American baby boom would be a greater demand for raw materials already in short supply and suggested an ethical imperative for western nations to check their population increase. I must admit to considerable personal sympathy with both these gentlemen—provided other countries do everything in their power to help themselves.

Whether we of North America can continue to draw freely on the resources of the rest of the world will depend largely on the *failure* of our own international planners: if, through Point IV, technical assistance and other bootstrap lifting operations, we are successful in raising the consumption of the skyrocketing populations, we shall certainly have to give up some of today's 50 per cent of raw materials, not to mention tomorrow's 83 per cent. Living standards climb on a staircase of raw materials.

About certain other resources there can be even less doubt. We are told we need not worry, since technology will always find substitutes. The self-styled optimists give little consideration to either quality or costs. To them, treated sewage and spring water are equivalent.

Three generations ago Thoreau wrote of his "tight shingled and plastered house, ten feet wide by fifteen long . . . with a garret and a closet, a large window on each side, two trap-doors, one door at the end, and a brick fireplace opposite." The materials for this winter-proof house, about the size of a modern garage, cost $28.12½. John Muir, a few years later, built himself a "snug cabin of shakes" for $3. We can scarcely build a book-case for $28, today, for all the accomplishments of the technologists.

When we come to wildlife, the gap is even more conspicuous. The vast pigeon hordes, the heath hen, are no more. The bison that once roamed in millions is virtually a museum piece. So is the wild turkey over much of its former range. Only a remnant remains of the once great riches of waterfowl, salmon, antelope and, throughout much of the continent, many other forms that once delighted the epicure, provided magnificent sport, and gave our landscape the interest, beauty, variety and character to lift it above the dullness of "older" countries such as those of Europe. The hawks that once gave winged beauty to our fields and marshes seem to be following the bison, without either government or private organization lifting an effective hand in their defense. The technologists have not provided a substitute for the American ecology.

North America is not alone in finding it impossible to maintain wildlife. From Africa, the "Dark Continent," one of the last resorts of the explorer, come reports of bloody massacres of big game—to make way for expanding human populations. Dr. L. S. B. Leakey, in his new book "Defeating the Mau Mau," writes: population increase ". . . is not only a Kikuyu or a Kenya problem, it is one which faces all Africa, India and China to an equal degree.

"No solutions to the other problems dealt with in this book will by themselves suffice, if [birth control] which is so closely connected with population increase and pressure on the land, is not boldly tackled."

An ever scarcer resource, and one that is peculiarly valued by the group meeting here, is space. This, like water, is not something for which we can find a substitute. We cannot import it from India or Africa is exchange for our "surplus" soil fertility. We cannot have solitude without it. We cannot have good hunting and fishing without it. We cannot have the quiet nor the interest nor the beauty of the wilderness without it. Yet, every year it becomes rarer.

There are reasons for this. One is that pressure groups of one kind or another, be they power promoters, flood-control people, irrigation interests or the automobile associations, are making continued assaults on such wilderness as we have left. They find much of their justification in the swelling demands of a growing population, and by and large they are succeeding in their attacks. Canada, giving very practical recognition to what is happening in the States, is trying to lure tourists northward by advertising her wide open spaces!

Whether or not my associates on this panel will agree, I do not know, but it seems to me, after watching the resource-population ratio nearly 30 years, that we are worse off both quantitatively and qualitatively, except for a few bright and very minor spots such as timber production by a few companies that, with decades of devastation behind them, have at last got religion. In the face of our population trends, from the impact of which not even the Arctic is exempt, and almost deified technology, I think we must accept the end of the wild America that has contributed so much to our culture from the Rio Grande to Point Barrow.

A great deal, though in some ways second rate, may yet be saved.

If it isn't, it will be solely because the organizations represented in this room have been unwilling or unable to work together.

It is even possible that something will be done to dam the rising flood of humanity.

It is said that in one of the mental hospitals in one of our smaller states, where funds for psychiatrists are scarce, new inmates are given a simple test—they are put in a large concrete room and each is handed a mop. There are three or four large water faucets in the wall, and the attendant turns these on before he goes out and locks the door.

The lunatics use the mops.

The sane people turn off the taps.

DISCUSSION

VICE-CHAIRMAN PLEVA: Dr. Vogt's paper is open for discussion. While you are collecting your questions, I would like to ask one question.

In the estimates given for the United States population growth by the Bureau of Census, and even more optimistic estimates, do you believe that this population increase will be a spreading out of people or a thickening of people in the already well established areas, and whichever one you answer, would you tell us what the direct conservational implications may be?

DR. VOGT: Do I have another twenty minutes? [Laughter]

There are so many unknowns in these equations that I think everybody is pretty leery of making forecasts. I was surprised at Dr. Shryock when he said that there was no probability that our rate of population increase in the states would level off. I think most of the demographers disagree with him, and I, who am not a demographer, think that it is very unlikely that we shall continue to increase at the rate of 1.8 per cent a year indefinitely; that it should not be long before there is a leveling off.

However, we must look forward to adding a great many million people to our population. At our current rate of increase it would double in forty years. I don't think it is likely to do that, but I think it is likely to go over 200 million—perhaps 225 million.

Now, as to the pattern that that will assume, it depends on a great many things. Certainly at the present time, cities are growing rather rapidly. There are some fantastic rates of growth in the Southwest and in California.

Some of the cities, however, are beginning to learn that growth is not, perhaps, an unmixed blessing.

To turn for a moment to the Old World, the London County Council is doing one of the really intelligent things I have heard of in the past few years. It's trying to move a million people, with industries to support them, out of London.

New York is going broke. I commend to you an article in the current *U. S. News.*

Certainly many cities are going to build up for a good many years, but when we couple population growth with the internal combustion engine, it doesn't make a great deal of difference where they live, because even if they were dispersed over the countryside, they would still be in the cities, and when they are in the cities, they would get out anyway.

Maybe they will get discouraged about trying to get out of Boston and New York on Friday nights and trying to get back in at the end of the week end, but there is no indication of it when we continue building superhighways. That is going to spread. There are going to be more city people let loose on the countryside who do not value the country.

I would be inclined to up Mr. Osborn's estimate to 87½ per cent for a few years, to see if we couldn't catch up with some of the destructive influences that are arising from the failure of people to understand these problems.

MR. PREVOST: Dr. Vogt, what is your suggestion, then? Reducing the population? At what rate?

DR. VOGT: It's a good question, because at the moment we don't have any answer. One thing that I would exempt from the cut in research funds is money to develop new and simple ways of making it possible for people to decrease their own fertility. There is an abundance of evidence that people want to do it.

For example, one of our representatives who is working in southern India went to a small village. She had to walk 15 miles to get in. Most of the people were illiterate. Two men walked 80 miles to see her, because they had heard that she knew how to limit the number of births of children and space them.

Now, an 80-mile walk for an Indian villager is probably not noteworthy, but the fact that the information had traveled 80 miles through the Indian villages, I think, is highly significant.

Nothing very effective can be done until there is a biological means of fertility control. My organization is supporting a small amount of research, with all the money we can scrape up. Some more work is being done; but it's still pretty insignificant, and until we get something of that kind, population growth is not going to slacken significantly—until people begin to starve.

VICE-CHAIRMAN PLEVA: I wonder if we could get a comment from Dr. Vogt regarding Canadian population. The figures given for the first of the year were, the total population, 15.4 million. The birth rate was the highest on record—approximately 28 per 1000. The death rate was the lowest on record—approximately 8 per 1000. The increase was 2.7 per cent, the balance between the net increase and the actual increase being the excess of immigration over emigration.

On the basis of Canadian resources, is this a good picture?

DR. VOGT: Well, unfortunately, I don't know Canada well enough to make a very intelligent noise about that. Certainly, much of Canada's land is highly unproductive, and when her minerals are exhausted, and her means of buying what she needs from overseas, she may find it necessary to pull in her belt.

I was much interested in something Dr. Brock Chisholm wrote. He certainly is one of the great Canadians. He pointed out more or less in the spirit of Lord Simon of Wythenshawe that perhaps instead of having such large numbers of children ourselves, we should have smaller families and adopt some of the needy children from other countries. That may be hopelessly idealistic; Dr. Chisholm is an idealist.

On the other hand, it may be a very practical measure, because the point of view expressed by Father deLestapis and Lord Simon of Withenshawe is not at all rare. There is a growing resentment around the world that the Western countries, particularly the United States, should have such a high living standard, and their living standards are rising so slowly. They don't see that it makes it almost impossible to raise their living standards when their population is rising so rapidly, but that may come in time, and that's almost certain to have a considerable influence on our foreign affairs. Such generosity as Dr. Chisholm has suggested for Canada—and I'm sure he'd include the United States—may be a very practical step.

DO RESOURCE PROGRAMS MEET PEOPLE'S NEEDS?

STEPHEN RAUSHENBUSH[1]
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Conservationists are caught up in a series of peculiarly baffling circumstances at this moment in history. They feel sure that if man can invent the atomic bomb and atomic energy family, he can do almost everything. At the same time, if he cannot invent some way to prevent that family from being used destructively, he can do almost nothing at all.

Further, the conservationists have been elaborately informed about the great shortages of raw materials that will confront the United States during the coming quarter- and half-century. Yet they find themselves as citizens and taxpayers plagued with surpluses of wheat, cotton, dairy products, coal, lead and zinc and so many manufactured products that they are tempted to fence themselves in against the rest of the world. Again, they find that man is able to disturb old ways of operation, markets and whole economies by such achievements as synthetic rubber, condensed coffee and now algae and wood cellulose for cattle-feed. But that same man seems unable to solve the distribution problems in order to bring food to the world's hungry people. He is also unable or unwilling, or considers it *infra dig* for science to produce some efficient low-cost chemical that will help the human family to adjust its size to its opportunities for growth and individual dignity. Problems of international juvenile delinquency (better known as subversion) among crowded peoples in nations ambitious to enter the adolescence of industrialization are affected both by our achievements of surpluses and our own conspicuous lack of achievement in human bio-chemistry.

Both shortages and surpluses work against conservation of natural resources. When there are shortages there is apt to be over-use of the resource. When there are surpluses, there are apt to be inadequate sums available for conservation, as well as some lack of concern with the necessity for it.

Your own field of wildlife conservation is clearly one where there is going to be a great shortage in relation to probable demand, and one where no technological tricks can produce adequate substitutes.

We can be helped somewhat in seeing the magnitude of the future demand by looking at some correlations between the growth of past

[1]Mr. Raushenbush is the author of *Our Conservation Job* and edited *The Future of Our Natural Resources* (May, 1952, issue of the Annals of the American Academy of Political and Social Science).

demand for recreational opportunities and the growth of disposable personal income in the United States. From 1947-1953 each 10 per cent increase in real per capita income led to a 4 per cent increase in the proportion of our population making visits to the national parks and forests. This is simply a rough indication of the demand for outdoor recreation. The present administration estimates conservatively that total national disposable income in 1960 will be 28 per cent higher than 1953, and in 1965 will be 52 per cent higher. Our growing population comes in. The per capita increases will consequently be about half that much, percentagewise. The result of a simple correlation (Table 1) indicates that the low potential demand for

TABLE 1. ESTIMATE OF DEMAND FOR RECREATIONAL OPPORTUNITY

	1947	1953	1960	1965
U. S. Population	144.0 million	159.6 million	177.5 million	190 million
Visits to National Parks and Forests[1]	15.6 million	27 3 million		
Estimated Visitors (70% of Visits)[2]	10.9 million	19.1 million		
Visitors as Percent of Population	7.6 percent	12.0 percent		
Increase in Percent		4.4		
Per Capita Disposable Income (1953 dollars)[3]	$1405	$1567	$1794	$2000
Percent increase over '47		11.1%		
Percent increase over '53			14%	28%
Increase in Percent of Population as Visitors over 1953[4]			5.6%	11.0%
Increase in Number of Visitors			9.9 million	20.9 million
Visitors in 1953			19.1	19.1
Estimated Total (High Estimate)			29.0 million	40.0 million
High Index of Recreational Demand	57	100	150	200
Increase in Number of Visits (single visits)			9.9 million	20.9 million
Visits in 1953			27.3 million	27.3 million
Estimatel Total Visits (Low Estimate)			37.2 million	48.2 million
Low Index of Recreational Demand	57	100	136	176

[1] Combined prolonged visits to United States national forests and parks.
[2] 1.43 areas covered by each visitor [3]Estimate of total disposable income of $380 billion in 1965, Joint Committee on Economic Report, November, 1954. [4]For every 1% change in disposable income per capita recreational demand moves 4/10 of 1% of population.

outdoor recreation can be expected to be 36 per cent above 1953 by 1960 and 76 per cent above it by 1965.

These figures seem to be high and to indicate a huge new load and burden on our recreational resources. But our human arrangements may throw a burden on them far greater than this. The huge accessions to our labor force plus the increased productivity of our workers indicates that a 37½-hour week, or its equivalent in annual working time can be expected by 1960, and a 35-hour week can confidently be expected by 1965. (Table 2). Any such major change in time-use can give a very peculiar lift to the potential demand for outdoor recreation and for the development of wildlife resources and locations. If

TABLE 2. ESTIMATE OF PROBABLE WORKING TIME, 1960, 1965

	Employed Civilian Labor Force (000)[1]	Gross Production Per Man-Year[2]	Resulting Gross National Product (billions of 1953 dollars)	Expected Gross National Product (billions of 1953 dollars)[4]	Shortfall in G N P (billions of 1953 dollars)	Surplus Man-Years (000)	Required Work-Week
1953 (actual)	62,184	$5905	$367.2				
			Projections				
1960	67,355	$7262[3]	$489	$458[5]	$31	4,268	37.5 hours
1965	73,100	$8419[3]	$615	$535	$80	9,502	34.8 hours

[1]U S Census estimates (Series P-57 No. 143 June 1954) less 3,550,000 in armed forces and less 2% frictional unemployment.
[2]Based on the 40 hour work-week.
[3]With productivity per man-year increasing at the rate of 3% annually.
[4]Estimate of Joint Congressional Committee on the Economic Report, *Potential Growth of the United States*, Table B-4, November, 1954.
[5]Prorated between 1953 actual and 1965 estimate.

the work-week is altered to a 4-day week of 9 hours, for example, the burden of the new demand will fall on the areas within 100 or 200 miles of the big population centers. However, it is far from certain that the work-week will be altered in this way. The situation may be arranged by providing longer vacations. A reduction of 5 hours per week to 35 hours in 1965 means a reduction of over 7 weeks a year in working time, and a potential increase in vacation-time by exactly that amount. That throws the burden on areas which can be thousands of miles away from the Eastern population centers.

This gets to be an entirely different magnitude of burden, either a large recurrent weekly load on nearby areas or a very large and very prolonged load on distant areas. You already know the pressure on wildlife resources of the present demand. Certainly the prospects suggest a challenge of major proportions to our ecologists and experts as well as to our outdoor organizations. And it is one that cannot be met overnight, or by technological tricks.

Estimates of this order indicate a huge spill-over from limited recreation resources into Canada (if they still welcome us) of a size and also a character that Canada has never experienced so far. It means large demand for family-type accommodations among other things, and perhaps a new attempt on the part of the tourist industry to encourage a mass market, if such a horrible word is permissible in this connection.

Instead of watching the head of our National Park Service wrestling with his conscience about whether or not to announce the closing this summer of some of our major parks to save for future generations what is still left after the trample of the past years, the conservationists should clearly be developing a new outdoor recreation program, with new parks and resorts, scaled for a population of 190 million in 1965 and 220 million at the close of the century. They are not going to conserve wildlife by allowing more millions to rush in where too many hundreds of thousands are already destroying it. They must be provided with more and other places to go. This is one of the simplest and most obvious challenges to all lovers of wildlife today. It is peculiarly a place where the wildlife conservationists can and should give a lead to others.

The high probability of a very much increased demand for outdoor recreation will also have an effect on the disputed question of grazing privileges in the national forests of the United States. These forests were started for the main purposes of forest and watershed protection. In many of them grazing seems to have become a major purpose and local interest.

We know that the grazing privileges are now sold by permittees at an incremental value of $100 for cattle and horses and $25 for sheep or goat. Taking 1,100,000 of the former and 3,000,000 of the latter, we find that the annual value of the grazing privileges ranges between $20 million and $40 million, depending on whether the annual value is capitalized at 5 per cent or 10 per cent in the sales transactions. Clearly an influx of new recreationists of only 400,000 a year, each spending $100 during their stay in the states having national forests will be as important to the people of the states as the total value of the grazing privileges is to the stockmen. Yet we are not considering an influx of only 400,000 more, we are considering the possibility of 10 or 20 times that many. It seems reasonable to conclude that if the range in the national forests is not fenced off in the next 10 years the watershed and recreation values will soon overwhelm the grazing values, even if they do not already do so.

The water conservation problem now goes far beyond the areas that have sometimes suffered from overgrazing of private or public range, or from overcutting of forests. It now extends into a whole series of southeastern and southwestern areas where climate changes produce occasional drouth, as well as in those areas where we ploughed up the plains to get wheat and are now (March, 1955) getting the blow, the dust and the punishment. Is it not time for conservationists to consider the necessity for attaching to any bail-out funds some simple conditions about land-use? How often are we going to repeat this process of ploughing up pasture and getting dust storms? It is not as if either Canada or the United States were exactly starving for wheat. Neither human life nor wildlife is helped by this process.

The only new water conservation program of any potential importance is the provision of last summer for guarantees of bank loans up to a national total of $25 million to individual farmers or groups of farmers who want to tap local streams or underground aquifer for water to protect them against occasional drouths. If the climate changes persist, this program may be of some value and deserve expansion.

Another program that is somewhat short of meeting human needs is the reclamation program in the Western States. Here the development has slowed down to less than a walk in recent years, and there is some mistaken crediting the conservationists for it. There are other causes for the slow-down. Surpluses of farm products have raised some question about the value of developing new land. Why use Government funds to grow more sugar beets when those now grown have to be subsized? Perhaps speculation about whether atomic energy costs may get below the 5-mill power that is now available on the Missouri and Colorado at present construction costs may also slow down the reclamation programs attached to those rivers in the immediate future. Hydro-electric energy has been used to subsidize irrigation, and if the source of funds drops out, water will never get to the farms in many of the Western areas. (The Columbia River power should be below the cost of atomic energy for many years to come). A new look at this whole situation is in order.

At the same time I think there is more basic agreement between conservationists and Western developers than sometimes appears on the surface. Any development is bound to tamper a little with the existing ecology of an area. The developers can be properly asked not only to hold the tampering to a minimum, but to make good the interference by the establishment of some equivalent resource for wildlife or recreation in some adjacent areas. For that they should pay. At the moment the conservationists are powerful enough to say "No" to many developments. Once our national economy begins to boom again in the United States they may not be in exactly that position. The conservationists are so right in reminding the developers that man does not live by bread and automobiles alone. But the developers are also right in saying that he can hardly live without some of the bread or some of the internal combustion engineering. Here it would seem possible to achieve more harmony than has existed, say at Echo Park.

The conservationists could also use some help from others when they act positively on their own, instead of simply vetoing the plans of others. For example, in the matter of the pollution of our Eastern streams. Just how much progress has been made in recent years under that program?

Over-all, it is difficult to discuss the adequacy of resource programs —whether or not they meet the people's needs—without going into all phases of the economy. Conservation is based on material things, although the communion of man with nature, in all its beauty and wonder, is a spiritual matter.

A few things can be said, without undertaking any over-ambitious effort to answer the question posed for this paper in every detail, or by reference to every resource program devised by the ingenuity of the Canadian or United States conservationists.

The first thing that needs to be said is that neither the people of Canada nor those of the United States are using their natural or human resources adequately at this moment. Neither economy represents anything like a full use of either type of resource. While the unemployed, and particularly the young people wishing to enter the labor force and to take on their civic responsibilities, are the most immediate sufferers from this failure to achieve full resource use, they are not the only ones. The whole community and all its industries suffer in varying degrees sooner or later. In the the United States we have for the moment shut our eyes to the situation by comparison of our present progress with 1953. This results in official statements that we are having our second best or third best year, and that certainly cannot be very bad. But the fact of the matter is that par for the course changes every year.

Par for 1955 should be about $385 billions of gross national product. It may reach $365 billions in 1955, which is a little lower than the achievement of 1953. And par for the course in 1956 will again be somewhat higher. Reverting from golf to physics, and specifically to the bicycle illustration, there has to be a little momentum to keep from falling over. As non-employment of new population puts on brakes, the momentum has to be a little greater to offset the backward pressure. We can only stay upright by moving forward.

Conservation is involved in the movement of the economy because failures to move forward adequately pile up surpluses. If the free market prevails, low prices result in less attention to conservation. If stockpiling supercedes the free market, as it has done for wheat in both Canada and the United States, then so much of our tax-money gets tied up in stockpiling that we cut down on normal conservation efforts, and put aside such major problems as that involved in developing a better landlord-tenant relationship—one which might lead to a great deal of fundamental conservation improvement.

Certainly conservationists must sooner or later ask themselves whether as a group they do not have even more interest in some of our surplus problems than they have so far manifested. We sometimes raise our hands in more or less holy horror at the East Indian practice of letting cows eat more than they are worth. A look at our $6 billion stockpile of farm surpluses would, I think, find some very foolish but very sacred cows grazing and trampling around on that luxurious range. One of them is our belief that it is perfectly proper for our taxpayers to interfere with the free market through the government by stockpile purchasing, thereby helping the wheat farmers, but that on the other hand it is sinful for our taxpayers to refrain from interfering with the free market and for them to get the benefits of low prices if, at one and the same time, they help the farmers directly.

Certainly this is not a superior human achievement in the process of trying to make supply and demand match up without injury to human welfare. We can expect better of ourselves than this.

This problem of matching the huge supply which our farms and factories can produce up against human needs goes beyond the people of the United States and Canada. We have been growing as if we expected the rest of the world to grow along with us. Our own growth, once it gets started again, can use up a great deal of our capacity, but not all. The rest of the world has not been able to grow as rapidly as North America has done. It has not been in a position to pay for North American surpluses. Large parts of it, with a billion people, have made only very small moves in the direction of achieving enough income to pay even for our manufactured surpluses, let alone any farm surpluses. A few tricks such as the algae and a really effective conversion of wood pulp to cattle fodder which I mentioned earlier could enlarge our capacity for producing feed quite disastrously, at the same time that they would cut any potential foreign demand for it. What price conservation then? A new look at our distribution problem abroad is almost as important as a new look at it at home.

Another resource program which obviously does not meet people's needs quite adequately is the one by which the United States and Canada are providing only one kind of protective support for the free areas of the world. These areas, with some billion people in them, in Asia, the Middle East, Africa and Latin America, are struggling to get out of feudal and semi-feudal ways of life. In some times and areas these ways may lead to excellent soil and water conservation. In other areas, where buffalo dung has to be used for fuel, for example, because the trees have all been used up, the soil and water arrangement is going down hill. The studies by William Vogt of individual Latin American nations are tragic reading in any language. Certain human arrangements, such as farm credit at 60 per cent a year, are part of this slide-off in some of the Asian areas.

This attempt on the part of a large share of the billion people in the independent nations of the world to move toward a higher degree of resource use and enjoyment is not unaccompanied by troubles. My earlier reference to those troubles as international juvenile delinquency was not entirely flippant. The growing pains and maladjustments and sudden urges to achieve manhood and status and progress altogether in space and time give the opportunities for Communist advance that we have watched in recent years. Our efforts to help these nations to protect themselves in this process are a resource program. As such they are a proper subject of comment in this paper. We take our own natural resources, manufacture them into arms and offer military defense.

However, the problem in these areas is not primarily military defense. It is a defense in the process of resource development. Can the people in Indonesia, for example, get enough from both the land and the institutions surrounding land use to make them feel that they have an achievement worth defending? If they do not obtain that feeling of achievement, our resource use in the form of arms will not prevent them from falling victims to Communist subversion. Our resource program is not achieving what it should because it is not directed at the main source of danger. In comparison with our military aid, our aid to them on the economic front is very small, and our aid to them in the way of obtaining freedom by moving out and way from some of their most luxurious feudal institutions is nil.

Here we are probably all suffering from a hangover of force-first thinking which grew up during the last world war. Both in the field of atomic weapons and in this field of frustrated people unable to achieve an adequate livelihood from their resources, the force-first type of thinking has some clear limitations. An atomic-hydrogen-uranium bomb stockpile twice as large as the present one does not give twice the amount of deterrent power as the present one. At some point in the process our resources could be applied more usefully in other ways. Our resource program here stops meeting human needs as clearly as it did at first. Similarly in the field of the less developed areas, our military support may rapidly become a far less effective resource program than other types of resource programs.

The weight given in this discussion to problems of surpluses is not intended to carry far beyond the time when the economy again begins moving forward at the rate of $15 billion and more a year. Once that happens, the old conservation problems in their customary form will again appear to us as the most important ones. Particularly the matter of adequate wildlife and outdoor recreation facilities, which is your own immediate province and interest, will appear as one deserving of far more attention than the people of the United States or of Canada have given to it. I hope that my excursion into related problems does not distract your attention from the great challenge facing you in this field. It is one that has to be met long before people start ruining the wildlife and recreational resources for the simple reason that not enough of them have been provided.

DISCUSSION

VICE-CHAIRMAN PLEVA: Is there any comment on Mr. Raushenbush's address?

DR. VOGT: Mr. Chairman, there are a few points in the paper with which I will disagree, but it seems to me that it's one of the best papers on conservation and the relationship between probable trends on this continent and conservation problems that I have ever heard, in a good many years of conferences and a good many years of reading. I will go out on a limb and suggest that perhaps we have just heard a classic.

It does, however, point up one matter that I mentioned in my paper, which is that if we lose this conservation battle, it will be our own fault. We have enough interests represented in this gathering, and we are representing either directly or indirectly enough citizens in the countries on both sides of the international border to make ourselves effective.

It is a great pity that over the years we have not been able to get together. You all remember Jay Darling's dream of a federation of all groups interested in the outdoors and conservation. If you don't have nightmares over this picture of the developments in the states, you are different from me, and facing a situation of that sort, it just seems to me that we cannot let the possibility of saving some of the things that we love and value go by default through a failure to work together.

VICE-CHAIRMAN PLEVA: Does anyone else wish to comment? [No one responded.]

I too have the feeling, as I heard this paper, that I would see this paper quoted many, many times in the future. I think we have had an outstanding paper here.

CHAIRMAN CAMPBELL: This brings to a conclusion an afternoon devoted to the presentation of thoughts and experiences of our very able speakers. I'm sure that you would like me to thank them for what they have done. I myself express my own appreciation to them all for what I have learned.

The meeting is adjourned.

GENERAL SESSIONS

Wednesday Afternoon—March 16

Chairman: ANGUS GAVIN
General Manager, Ducks Unlimited (Canada), Winnipeg, Manitoba

Vice-Chairman: JOHN M. ANDERSON
Superintendent, Winous Point Shooting Club, Port Clinton, Ohio

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WATERFOWL FLYWAY MANAGEMENT PROBLEMS

INTRODUCTORY REMARKS

ANGUS GAVIN

Ladies and gentlemen, we have for you this afternoon some very interesting papers. Therefore, my remarks are going to be brief. I would just like to say that down through the years these sessions have become the gathering place for the conservation army of the continent. Represented here today are most of the state and provincial conservation agencies of Canada and the United States, and some of our friends from farther South.

It is well that we have so many people and agencies interested in this field. With the steadily increasing gunning pressure and a growing population, especially here in Canada, it is imperative that all agencies work together toward a common goal if we are to hold what we have at the present time. Over the years, and especially so during the last ten or so, much valuable information has been gathered in the field of wildlife management. However, to be effective, this must be applied to actual production to be of any use. There is little use in just knowing that certain methods will increase production or help the administration to regulate the harvest, unless we are prepared to apply these methods on a sound, practical basis.

In Western Canada, where a large percentage of the total North American waterfowl are raised, I am very happy to say that the coordinated efforts of all the agencies working together have done much to safeguard and perpetuate this vital breeding area. Too much praise cannot be given to the provincial and federal governments of Canada, for the foresight and broad over-all policies in this field, despite the continued pressure placed upon them by an ever-increasing population.

True, we are still at the crossroads, and no one can say whether the waterfowl populations will go up, down, or stay on the level. We are sure, however, that should any downward trend take place, it will not be the fault of lack of cooperation between the various agencies here in Canada.

Ladies and gentlemen, naturally at a session of this type, time does not permit all phases of waterfowl management to be discussed. Today the field is limited to flyway management problems and we have with us as the panel a number of gentlemen fully qualified to express to you their views.

Before introducing the first speaker, however, I would like to introduce my co-chairman, Mr. John M. Anderson, who is superintendent of the Winous Point Shooting Club, Port Clinton, Ohio. Mr. Anderson will lead the discussion periods which will follow the remarks of each of the speakers.

WHEREFORE FLYWAY COUNCILS!

T. A. McAMIS

Chairman, National Waterfowl Council; Executive Secretary, Arkansas Game and Fish Commission, Little Rock, Arkansas

Prior to 1947 the waterfowl of the North American Continent were managed and regulated on a zone plan, both in our Canadian Provinces and in the United States. The adoption of the Flyway Management Plan marked a new era in waterfowl management, and as it developed from 1947 to 1951, it became more apparent that the Fish and Wildlife Service was not equipped to assume the full responsibility of the waterfowl program, and it also became more apparent to the states that if they were to be charged with the principal work of enforcing the regulations in the respective states, that the states should have a strong voice in formulating these regulations and an active part in making the final decisions.

The turning point came in September, 1951, at the International Association of Game, Fish and Conservation Commissioners meeting in Rochester, New York. It was at this meeting that the National Flyway Council plan was adopted by this organization, and I might add that there was some difference of opinion as to its practical application and even if such an organization were wanted by the federal service and would be used by them.

The objective of the National Flyway Council is simply the improvement of waterfowl management to the end that the largest possible waterfowl crop will be available to annual harvest by our people. The attainment of this goal requires the full utilization of the qualified manpower and the vast resources of the 48 states, the Canadian Provinces, our friends in Mexico, and the U. S. Fish and Wildlife Service in a coordinated effort.

The Flyway Councils were established to implement the work of the National Waterfowl Council. The reasons for setting up these Flyway Councils can be briefly stated as:

1. The adoption of the flyway plan of management by the Fish and Wildlife Service.
2. The need for more active state participation in annual surveys, both on nesting and wintering grounds, to determine populations, movements, kill data, and other pertinent factual information.
3. The fact that state agencies within the Flyways bear the major share of enforcement of migratory bird hunting regulations—

regulations in the formulating of which they had practically no voice.

4. Conditions which prevail in each Flyway which present difficult problems for both the State and Federal Government.
5. The need for better understanding and cooperation between the states and the Federal Government in the formulation of annual migratory bird hunting regulations.

The primary objective of these Councils is to establish coordinated management plans, embracing research, law enforcement, and habitat improvement, which will insure protection to and restoration of waterfowl, providing for sustained annual harvest of shootable species.

A brief account of the Mississippi Flyway Council's activities may serve to illustrate the accomplishments of these organizations. One of the undertakings has been the preparation of a report documenting all pertinent actions which were involved in the establishment of the Mississippi Flyway Council and a Waterfowl Management Plan for this organization. The management plan was prepared in part by having the various states rate their primary needs and interests under a group of jobs associated with production, distribution, harvest and extension.

Jobs are assigned by the Council and by the Technical Section of the Council. Many of these are in the nature of annual inventories and are common to all of the Flyways. Many other jobs, such as habitat inventories, breeding ground surveys, management of particular species, and those dealing with depredation, may be limited only to one Flyway or to a group of states within a particular Flyway.

In the Mississippi Flyway, organized use of aircraft by all of the states for regularly scheduled inventories has provided valuable information not only on numbers of waterfowl but on time of migration of various species, dates of peak populations, habitat conditions, and to work out special problems in the management of particular species.

Standardization and improvement of this type of work, kill data, and other tasks common to all of the states in the Flyway, and to the Fish and Wildlife Service, is one of the chief aims of Flyway organizations, and we feel that notable progress has been made.

A cooperative banding program was carried out on the Canadian breeding grounds in the summer of 1954. States furnished personnel to assist the U. S. Fish and Wildlife Service and the Canadian Wildlife Service in this important undertaking. Banding of juvenile waterfowl to determine the contribution of various segments of the breeding grounds to the populations of the four Flyways was the chief objective.

In the effort to preserve and manage our wetland habitat, the work of the River Basins Division of the Fish and Wildlife Service has been tied in with that of the states represented. This coordinated, cooperative approach to this most essential part of waterfowl management is advantageous to all concerned.

In developing the Flyway Council and Waterfowl Council idea, certain problems have arisen. There have been conflicts between the states and federal service and some misunderstandings. These were brought out at the last meeting of the International Association at Seattle.

On Friday, January 14 of this year, at the invitation of Director Farley of the U. S. Fish and Wildlife Service, the National Waterfowl Council met with him and other representatives in Washington for the purpose of clarifying these problems:

1. Just exactly what is the Fish and Wildlife Service's attitude toward the National Waterfowl Council and the various Flyway Councils? Where do they stand in being accepted by the Fish and Wildlife Service?
2. Should the Service favor these organizations; then how does the Director believe they can be of the most help to the Fish and Wildlife Service in managing waterfowl?
3. A resolution was adopted by the National Council as follows:

   Whereas the National Waterfowl Council desires to suggest a procedure for the establishment of waterfowl regulations which will be acceptable to the U. S. Fish and Wildlife Service and the respective states concerned, and

   Whereas it is the opinion of the National Waterfowl Council that the procedure used in the past can be improved upon,

   Now, therefore, be it resolved that the National Waterfowl Council believes that the season and bag limit regulations which are the most important factor in controlling the total kill and which are most closely related to information from winter inventories and breeding ground surveys should be given principal consideration at the final annual regulatory joint meeting of the U. S. Fish and Wildlife Service and the National Waterfowl Council, and

   Be it further resolved that the Council believes that all *other* waterfowl regulations are of such a nature that they relate only in a secondary way to the over-all annual kill of waterfowl; that they do not depend on information received from the winter inventories and breeding ground surveys and that they should be worked out materially in advance of the final annual regulatory joint meeting of the U. S. Fish and Wildlife Service and National Waterfowl Council, and

Be is further resolved that the U. S. Fish and Wildlife Service send proposals for such secondary regulations to the respective Flyway Council officials for consideration and comment not less than ninety days in advance of their final regulatory meeting.

4. Presentation to the Service of recommendations of individual members of the Council.
5. Clarification of the status of the Flyway Coordinators and the Flyway Biologists. In their dealings with the Flyway Councils, these individuals should not be restricted by Service Regional considerations, especially when more than one Region is involved.
6. A request that the Service include the National Waterfowl Council in the final deliberations of the staff waterfowl advisory group.

These were prepared the day previous to the meeting with Director Farley on January 14.

Mr. Farley opened the meeting and in his introductory remarks answered most of the questions on the Council's agenda even before they were read. He asked for closer, real cooperation with the National Waterfowl Council and the various Flyway Councils. He stated the Service needed their help. He pledged every effort on the part of the Service to make all phases of waterfowl management a real partnership. He presented a proposal almost identical with Item 3 resolution.

A general discussion followed during which it was decided that the Fish and Wildlife Service would try to keep the various Councils better informed on staff thinking relative to waterfowl management and regulations. Specifically it was decided that the following procedure would be followed in formulating regulations for 1955:

1. The various states and Flyway Councils will forward to the Service any specific regulations (other than seasons and bag limits) which they feel are needed.
2. At the North American Wildlife Conference in Montreal the Flyway Councils will meet with representatives of the Fish and Wildlife Service and set dates for each Flyway Council to meet with representatives of the Service and discuss regulations other than seasons and bags. (This has been covered by meetings on Sunday of this week).

3. Between the Montreal meeting and the first Flyway Council meeting the Service will prepare and send to the states tentative regulations (other than seasons and bags).
4. Flyway Council meetings to discuss with Service personnel these regulations and arrive at tentative decisions.
5. Report results of Flyway meetings to all agencies.
6. Two-day regulations session as late as possible in August.

   1st day—Service and the National Waterfowl Council discuss the proposals developed at previous Flyway meetings, decide on their final form, and discuss latest breeding ground information as it affects Service and Flyway proposals for seasons and bags.

   2nd day—Settle final regulations including seasons and bag limits.

Following this meeting the National Council adopted two additional resolutions:

1. Urging the Migratory Bird Conservation Commission to reappraise current concepts of land values for the purpose of drastically increasing the amount per acre which can be paid for waterfowl management areas before suitable sites are forever lost, and it was further resolved that the cost of each area considered be weighed against its importance to the preservation of the resource rather than in inflexible ceiling.
2. An additional resolution was adopted that the National Waterfowl Council respectfully urge the Congress of the United States to appropriate a sum equal to the annual duck stamp revenue to be used each year for the same purpose as the duck stamp money until the planned land acquisition program is completed.

As this meeting was concluded in Washington, and as borne out, I think, by preliminary meetings in connection with this North American Wildlife Conference, there was a very real feeling that the Canadian Provinces, the States, the Flyway Councils, and the U. S. Fish and Wildlife Service were on firm ground, that there was mutual cooperation, that all were integral parts of our Waterfowl Management Program, that gratifying progress had been made toward carrying out expressed objectives, new techniques, and procedures, that the organization of management efforts and a better understanding of these common problems had been reached, and it is felt that continued improvement can be expected in the Waterfowl Management Program.

DISCUSSION

VICE-CHAIRMAN ANDERSON: As a fellow technician working in the Mississippi Flyway I can vouch for the fact that many things have been accomplished that would have been impossible prior to the formation of the Flyway Council. The cooperative waterfowl banding project is a good case in point, whereby several of the individual state game departments provided men, materials and money and cooperative effort to the U. S. Fish and Wildlife Service and the Canadian Fish and Wildlife Service.

However, we cannot rest on our laurels, so if someone has an idea or a question to ask Mr. McAmis in regard to the functioning of the Flyway Council, let us have them at this time.

MR. I. T. BODE [Missouri Conservation Commission]: I have one or two comments to make, one of which may not be too closely allied in connection with the Flyway Council idea, and yet I believe, in thinking over the planning work of the Council, it does relate to it.

Certainly I hope that because of the shortcomings of the Flyway Council movement to date we will not give up the Flyway Council idea.

I think fundamentally it's one of the soundest things that has ever happened. I hope there is no consideration given to the fact that they may not be functioning and may not be useful.

On the part of the flyways I think we should not forget that they can very easily become pressure groups instead of helpful groups, and in so far as we out in the States can avoid their becoming that, they will become permanent and successful.

Now, from the other standpoint I want to question a problem that we find in our state, and I think some of the other states, with regard to functioning in the best way and making the best contribution to the Flyway Council movement, and that is that matter of getting information back in time so that we can study it and really pass intelligently upon that information. I find a few shortcomings.

First, very many times the information we get and the reports we get back are rather vague. We don't know just exactly what the Fish and Wildlife Service means to tell us, and there, if I may digress a little bit from the flyway idea proper, I think that applies as much or more to the river basin planning work as it does to the flyway work.

The idea of the river basin planning work was that we could get into the original stages, and so that the report that was supposed to be made to Congress with regard to river basin projects would fully express the viewpoints of the coordinated efforts of the Federal Government and the states.

Now, we find that too frequently the report has already gotten into the hands of Congress before the states are aware of what the report was, and I wonder if something can't be done to bridge that gap in our flyway planning, in our river basin planning work, so that we wouldn't be at such a disadvantage and could make a better contribution.

MR. I. T. QUINN [Virginia Game & Inland Fisheries Commission]: I thought for a long time that in order to better develop a cooperative plan and undertaking on the part of the states with the Fish and Wildlife Service, that, as a river basin study was concluded, the state would get copies of those reports in which they would be interested, and I'm glad that Mr. Bode of Missouri touched on that, because I have been thinking of that for quite a while, and I believe that it is highly important that we do that, so that we will more intelligently be able to cooperate constructively in the public development of waterfowl feeding grounds and shooting grounds.

MR. MCAMIS: Now that we have heard from both of the I.T.'s, the I.T. of Missouri and the I.T. of Virginia, I'd like to get back to Mr. Bode's remarks and his question.

I certainly agree with Mr. Bode that this river basin work is most important, and we had a situation arise just last week where it was necessary for the Game and Fish Commission to wire the secretaries of the agencies in Washington, informing them that it was the desire of our Department that mitigating losses for wildlife and fisheries be written into the cost of projects.

At Tulsa, just previously, the Governor's representative from our state with the governors' representatives from several states, had voted to disallow these costs in the project. They are important to us, and I know that they are to you.

When we questioned the Governor's representative, it developed in his reply that the river basin group had not given them a report containing cost recommendations, so he had no alternative but to cast his vote as he did.

Therefore, we know they are important, and in my opinion the best way to bring it to the attention of the Service would be to ask for that action in your respective flyway councils.

MR. WADE CREEKMORE [Mississippi]: On this subject which you have just mentioned I want to say that Mississippi is vitally interested in this whole matter of flood control projects being carried on in the Mississippi Valley.

One other matter that I think I might mention is the studies that are being made and will be made by the representatives of the Fish and Wildlife Service on the losses of fish and wildlife values, and what can be done in mitigation of those losses.

The problem that I can foresee is that perhaps there will not be enough time under the present system and the routine that must be followed by those representatives in getting their reports and studies approved by the regional office, in allowance for our region and in Washington, and get those reports back to the states in time to do us any good.

I will give you one example. We have now a project in contemplation for the immediate construction and initiation of work on the lower auxiliary channel of the Agnew River.

The hearing has been called on that project for April 5th, and Mr. Roberts at Vicksburg has been making that study. I don't believe he has completed it, but when he does complete it, I think he will have to submit it to Atlanta and then go to Washington for approval, and I'm afraid that study report will come to us after the public hearing has been held.

What I am saying is that if there is any way for the Fish and Wildlife Service to expedite the approval of these studies, so as to get them back to us in time to be of use in these public hearings and for our own purposes in making plans, it would be a very fine thing.

MR. MCAMIS: Mr. Creekmore, that, I think, points up one of the prefaces of the formation of the Flyway Councils. At the time that this organization was set up, the need for the flyway representative from the Service to work with the councils was most apparent, and that was one of the strong points that was made at that time: that those flyway representatives not be bogged down in the regional offices; that they work directly with the councils and be cleared by Washington, because as you know, in most of the flyways at least two regions are involved, and in some of them, three, except for the Pacific flyway.

Therefore, it is an important point, and I want to thank you, sir, for bringing it to our attention.

SOCIAL SECURITY FOR MR. DUCK

ERNEST SWIFT
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Today's growing concept of dangerous living seems to be various and sundry schemes for regimented security which take over with the tying of the umbilical cord and write finis with the giving of the last rites. However, this paper is not a discussion of human welfare programs, but, rather, assuming there is merit in the welfare philosophy, that it be extended and made more inclusive. To be explicit, our assignment is to see what can be done about getting Donald Duck a social security number, together with all his numerous relatives who fly up and down the continent. The motive is strictly selfish, and I, for one, blush as a proponent. Our collective interest in the preservation of migratory waterfowl is for further and continued exploitation. In other words, our objective is to keep Mr. Duck alive and healthy so we can kill him after he has grown fat enough to eat.

Several million people wish to keep Mr. Duck as a component of our economic edifice. Human beings grow very solicitous about Mr. and Mrs. Duck around October first, hoping they have reared a big family and that drought, floods, predation, and pestilence were kind during the domestic interlude. The balance of the year, the greatest predator of all, directly or indirectly, has little or no interest in the welfare of the duck family; complains about restrictive regulations, and turns a deaf ear to the need of fundamental resource management to save waterfowl habitat. To put it another way, the duck hunting gentry can be just as cold-bloodedly commercial as many another group that desires to crawl under conservation's "Big Top" to capitalize on America's resources. If the hunter thinks that way, so thinks the administrator—and don't kid me that he doesn't.

Acknowledging that the above is just the good old American approach, let us examine the record as to the kind of a job we are doing.

For one thing, we religiously meet year after year and preach the gospel to each other. I would say that, although our evangelism has had some impact, it has not been an overwhelming success—if the daily bag limit can be used as a criterion. At best, it has been a rear-guard action. Possibly there have not been a sufficient number of Billy Grahams in the business; at least he preaches to others besides members of the Cloth.

To make any pronounced headway, we should—through proper channels—project our evangelistic efforts to the halls of Congress, to the state legislatures, to those draining the potholes, sloughs, and marshlands, to the unabashed polluters of waters, and to those who claim depredations.

We are confronted with a super-lobbying job for Donald Duck if we expect to lift the mortgage from his abode and keep the sheriff from hanging him for a thief. Much as it grieves me, Mr. Duck Hunter is not going to boil over with emotionalism unless he can see something in it for himself; in other words, a duck in the oven. Our American culture has not progressed beyond that point. For that reason we might as well be realists and understand the kind of characters we are dealing with.

I have a strong suspicion that the average duck hunter becomes frustrated when reminded that the waterfowl problem is international in scope. His generally localized knowledge gives him an inferiority complex, and to be self-important he strikes out at research, at regulations, at law enforcement, at administration, or at anything or anybody who intimates he lacks understanding. When viewing the continental problem from behind one duck blind, the idea that humans and wildlife live in a community of interrelation makes little impression. The vista is too broad, the horizons too far away.

However, the people expect, and rightfully so, a high type of evangelistic leadership from all of us. All of us as a team. It will not work any other way. In spite of his gripes, Mr. Public secretly yearns for someone to fight his battles, someone to lead the way, and —I secretly suspect—someone to pin the blame upon when things don't suit him. Therefore, our leadership must capture his imagination or he won't follow along, and will be disillusioned.

The Fish and Wildlife Service, in conjunction with the States through their Councils, the national and private conservation organizations, the great Dominion of Canada and the Provincial agencies, can collectively supply that leadership. They are working toward it already.

Time marches on, and the states now make their entrance onto the stage and declare their intent for greater responsibility. They have already made their curtsies before the footlights by creating four Flyway Councils and a National Waterfowl Council. Further proof of their intent and purpose is exemplified in the aggressive way the states have furthered the waterfowl program.

Let me advert to a few statistics. Prior to 1938, only two states, California and Utah, had given serious attention to buying lands and improving them for waterfowl. But the new money coming to the states through the Pittman-Robertson Act provided a stimulant to activities beneficial to ducks and geese. In the 16-year span from July 1, 1938, to June 30, 1954, the states acquired nearly 500,000 acres of land costing more than eleven millions. Developmental work for waterfowl on nearly 1½ million acres absorbed close to fourteen millions. More than 26 millions went into the buying and developing of lands and in maintaining the developments. With expenditures in excess of 4 millions for research thrown in, total waterfowl outlays by the 48 States were more than 30 million dollars (7½ millions state and 22½ millions federal). These cash outlays of Pittman-Robertson funds for waterfowl represented 25 per cent of the total federal apportionments to the states.

There is no point at this time to my detailing the states' waterfowl restoration achievements under the Pittman-Robertson Act. Those contributions have been set forth in reports and publications that have been brought to the attention of all of us. It will suffice to say that nothing has been too big to tackle, as evidenced by the acquisition and development of the 54,000-acre Rosseau Waterfowl Management Unit in northern Minnesota, nor too small to consider, as demonstrated by New York's creation of a pattern of more than 600 small production marshes ranging from 2 to 10 acres each in size.

One thing is abundantly clear. Without the massive state contribution to the national program to benefit ducks and geese, the outlook would be many shades darker than it is at the present time. And I think that the degree of cooperation in the partnership concept of resource preservation and development could have no finer illustration than the help the States have been giving the Fish and Wildlife Service and Dominion wildlife authorities on the Canadian marshes. Last summer, for example, 11 states detailed 23 of their wildlife workers to cooperate on waterfowl banding work so that more management data could be gleaned about the vagaries of migrations and the extent of harvest of birds whose flights to the South begin in the Canadian marshes.

On the Fish and Wildlife side of the picture, I do not intend at this time to go into the distribution of the Duck Stamp funds. That question has had a full-dress rehearsal before the International. However, here are a few figures and a little background on the philosophy behind the refuge program.

As of to date, there are 275 national wildlife refuges totaling about 17½ million acres, of which over 7 million are in Alaska. In the United States there are 204 national wildlife refuges, primarily for waterfowl, totaling about 3½ million acres. This group of refuges is maintained by the Duck Stamp Fund and the others from the annual appropriation.

The primary objective of the national wildlife refuge program has been, and continues to be, the protection of a basic breeding population of waterfowl as required to perpetuate the resource.

A related objective is the preservation of wildlife habitat representative of the major ecological areas of the country. Wildlife of the grasslands constitutes one of the greatest conservation problems before the public today. The future of many species of wildlife rests upon our ability to restore and manage grass and range lands still under public control. Over the years, strong public interest in a series of wildlife crises resulted in key areas of uplands being acquired through Congressional action or by national conservation organizations. It is because of these special problems that big game areas such as the Charles Sheldon Refuge, Nevada, and the Elk Refuge, Wyoming, were acquired and placed under the administration of the Fish and Wildlife Service. This class of refuge is maintained by a specific appropriation from Congress. There should be a continuity of purpose in administering these and other wildlife lands.

The scope and character of management have of necessity broadened greatly in the past decade. The Fish and Wildlife Service has moral, as well as legal, obligations to two groups of conservationists: (1) Those who are primarily interested in complete protection of all forms of wildlife, and (2) those who, through the purchase of duck stamps, contribute funds for the perpetuation of the resource to provide reasonable opportunities for hunting. In attempting to meet these joint responsibilities, a considerable acreage of national wildlife refuge land is, and always will be, closed to the hunting of waterfowl. Some properties designated as wildlife management areas have been acquired and developed under state enabling legislation that specified a portion of the land be available for public hunting. Other properties in this class represent a joint endeavor between the Fish and Wildlife Service and a state conservation department, with federally owned lands being managed primarily for the protection of basic breeding stock and the state-controlled property being used for public hunting ground. This latter type of management project has become a matter of necessity in many parts of the country as the sole means of providing essential habitat for many forms of wildlife, since the cost of land purchase and development exceeds the ability of either individual agency, whereas, jointly, provisions can be made for meeting the dual responsibility to both groups of conservationists.

Cooperation between the United States and Canada has been most gratifying. This common approach to the problem is vital if waterfowl are to be saved. Private organizations have done yeoman service in shoring up the foundation of cooperation, research, protection of habitat, intelligent understanding. In a sense, as the loyal opposition, although not so in reality, they have cheered us, criticized us, prodded us, and defended us. They are the kind of hair-shirt the public agencies need to keep them inspired.

But when Monsieur and Mademoiselle Pintail wing their way through the Klamath basin and on down the Sacramento, the San Joaquin, the San Bernardino, and the Imperial valleys, the kaleidoscope of human activities they fly over is a far cry from the days of Jed Smith, the 49'ers, or even two decades ago. The vast tule marshes, whose uncounted inhabitants of noisy waterfowl awed the most hardened mountain men, have become rice paddies and alfalfa fields. For what? I suspect it adds up to two Cadillacs parked in the driveway where one formerly sufficed.

Then there is old man Greenhead coming out of the Louisiana swamps, bound for the North country, and finally reaching the Dakota prairies where he vainly circles looking for the pothole which was the scene of last year's romance. His efforts are rewarded by being engulfed in a cloud of dust rising from the cutting blades of a double disk harrow. His pothole is no more.

When the petite pair of Bluewings who summered on Kokogan sloughs bordering Lake Superior, start proudly south with their brood of youngsters, what do they face while flying down the Mississippi valley? Guns! Guns behind every bush and cattail clear to the Delta. When Mr. and Mrs. Canvasback and the Scaups leave Canada for the Atlantic seaboard and points south, their journey is just as discouraging and fraught with danger.

What we finally have on our hands are frustrated ducks and frustrated hunters. The hunters are a part of that human element which has brought about the whole ugly situation. In many ways, defeat is inevitable so long as the pagan god, best known as "The Fast Buck," is worshipped in the American household in place of the Trinity.

In this confusion which has been given the rather dubious name of conservation, I see many esthetic and ethical concepts dragged about in the mud. It all proves to me that the culture we fancy has been established through the ages is not even skin deep; it is hardly a rash. If we can't eat a resource or sell it, our interest ceases and we pronounce the resource of no value.

Even lowly Mr. Merganser cannot escape the wrath of that defender of the faith—the present-day conservationist. The poor little guy is pictured in the role of a villain for eating a few trout fingerlings, so he, also, is condemned to die. Shades of the Spanish Inquisition!

Maybe the time has come when the professional conservationists should look the public right in the eye and state the issues, even though unpalatable. In addition, it might be well for all of us to take a good long view of ourselves in the looking-glass. Where do our responsibilities lie? With the resource? Or with the pressure groups who clamor for preferential treatment and local advantages regardless of what the survey figures may dictate. With the public as an entity, we are dealing with a dual personality. Regardless of what the long-range effect may be, those who demand optimum hunting loudly protest any interference when it comes to the exploitation of resources as a general proposition, so long as that exploitation helps gratify their economic status.

I am becoming as confused as the ducks. I saw the waterfowl migration in western Minnesota and the Dakotas 50 years ago. It is something I will always remember, even though I was pretty young at the time. Long files of ducks, geese, and sandhills from horizon to horizon, north in the spring, south in the fall, day after day. They were uncounted and without number. There were no laws. Spring shooting was nearly as common as fall hunting. Market hunting was a recognized occupation. This was before the day of drainage. There was still much unplowed prairie, but small-grain farming was an added blessing for waterfowl.

I doubt if anyone will seriosly argue with me as to the number of ducks at the turn of the century as compared with today. Yet I hear the term "under-harvest" used with increasing frequency at a time when there is a four-a-day bag limit. I am beginning to suspect that somewhere along the way my education in these matters has been sadly neglected. All in my lifetime, gentlemen: from no bag limit, no law enforcement, spring and fall shooting, and no biologists, to the narrow margin of four ducks a day. And we are still "under-harvesting"?! I hope and pray we do not *under-harvest* the duck population out of existence!

Democracy and free enterprise have been recording the artifacts of their particular idealism in this country for 175 years. The recording is far from complete. Being conceived in liberty and dedicated to the proposition that all men are created equal will not suffice. Freedom and liberty will survive only in proportion to the restraint with which they are exercised. Even a mediocre people will prosper when not required to manage and control the use of their resources. It is when those resources are in need of husbandry and rationing that democracy must meet the test.

We are approaching the crisis in the preservation of waterfowl. Regardless of where the fault lies, all conservancies with a ghost of a responsibility may ultimately feel the fury of their people. That is why teamwork is so necessary now. We certainly have great potential forces upon which to draw if we marshall them to a common objective. Did you ever read of the discouragements of men like Benjamin Franklin when selling the new Federal Constitution to the weary and distrustful colonies? *That* took a job of salesmanship. Today we have far greater advantages on our side if we will but grasp them and make them work toward our ends.

I would hate to feel that our future offspring, in viewing their favorite gamebirds behind glass, stuffed, will ponder on just what type of improvident barbarians sired them. But, on the other hand, even if you are not at all interested in posterity, there is a job to do for ourselves before we pass out of the picture.

Just what does this cooperative job consist of? Let us keep in mind, in capital letters, that no one organization can accomplish the job alone. I would say that Number One in the United States is a land purchase job by the Federal Government and the States which will give elbow room and feeding opportunities to waterfowl. No doubt Canada will soon have this problem, if it is not already with them to a lesser degree than with the states. So, again, saving wetlands and a purchase program are mighty high on the totem pole.

In addition, we should tell our story just as often and as effectively as our competitors in land use.

We need coordination of surveys and research of all government units and private organizations on a continental basis, and a realistic and firm stand in developing regulations, bearing in mind that all official bodies are going to be subjected to pressures from groups and communities, who in light of their own limited knowledge are very sincere. As I said before, we are working on a thin margin when we are down to four ducks a day.

The fundamental issue in this regard is the adequate protection of the resources at all times. Therefore, when recommendations must be turned down, it should be done with intelligent explanation and sympathetic forbearance.

In closing, I wish to points out that it is far better to be condemned for a purpose than for the lack of it. Let us hope our purposes are consistently sound and—more important—honest, even though they may be at odds with some of the current advocates of expediency and materialism.

Again, let me reiterate my appreciation of the work of private organizations—The Izaak Walton League which pioneered many years ago on behalf of waterfowl; Ducks Unlimited for their masterful money-raising ability and continual efforts in Canada; The Wildlife Management Institute whose work at Delta, with the Cooperative Wildlife Research Units, and in making possible meetings of this kind, have at long last given dignity to wildlife research; and in connection with research we do not forget The Wildlife Society; and the National Wildlife Federation whose close work with the states has guided the emotional enthusiasm of many local organizations toward productive accomplishments. Neither do we forget the untiring work of the Audubon Society. When conservation ethics are in danger of being sold down the river for greed and expediency, the Audubon Society has reminded many an administrator that certain fundamental precepts should be considered as well as license fees.

But teamwork is needed now! *Now!* Gentlemen! Five years hence may be too late!

As a parting statement, I cannot refrain from quoting Edmund Burke: "The only thing necessary for the triumph of evil is that good men do nothing."

DISCUSSION

VICE-CHAIRMAN ANDERSON: Well, well, well! We seem to have the unique situation here of a man in a responsible position in the Department of the Interior—rather high up in that Department—who has guts enough to say that a man who is in a position of leadership should lead. [Laughter]

His quotation from Edmund Burke reminds me of one by Dante, which is to this effect: The hottest places in hell are reserved for those who in times of moral crisis remain neutral. [Laughter]

I bring this up because it seems to me after sitting through the Mississippi Flyway Council meeting last Sunday that Mr. Farley in his very sincere attempts to listen to all sides of the questions of bag limits, and so on, is in danger of remaining neutral just a little bit too long.

Now, having stuck my neck out, is there anyone else who would make some comments or raise some questions for Mr. Swift at this time? [No one responded.]

Mr. Swift said that we need a superlobbying job for the duck. I hope you will all take that to heart, because there can be no question about the fact that there is superlobbying going on in Washington right now, and as far as I can see, it is all against the duck.

MR. I. T. QUINN [Virginia]: I can't let this opportunity pass without paying tribute to a man who has devoted the greatest part of his useful life to the field of conservation. I have reference to the last speaker, Ernest Swift.

I think that is one of the finest papers I have ever heard presented to this or to any other conference, and I wish we had more like him in the country. I wish we had men who could make up their minds and make decisions and lead, and we are in dire need of them. We want to pay tribute to this man at this time because of the fine leadership that he is developing in the Fish and Wildlife Service in Washington, and his desire and his spirit to cooperate with every agency, private or otherwise, in the development of a program for wildlife in this country and on this continent.

There is one thing that I hate to see develop here or anywhere else. The ducks are becoming relatively fewer in proportion to the number of guns, and the guns are just getting a little bit more powerful, and you see more magnums creeping up out of the blinds that pull the one the farthest away, and we're getting more cripples because of that condition than we otherwise would. When you think that 25 per cent of all the ducks that are shot at and hit are crippled and not recovered, there must be some program that will cut down on these crippling losses.

DR. CLARENCE COTTAM [Utah]: I too would like to extend commendation to my good friend Ernest for the splendid address he has given.

It seems to me we are reaching a time of crisis where we need both our good works and our good faith, and need to do something about it. If the report that reached me last night is correct, here is a good time to put this to the test: Do we mean "cooperation" or don't we?

I am informed that the Wichita National Wildlife Refuge is scheduled to be eliminated as a federal wildlife refuge, and turned over to the military as a point from which big cannons can be shot. The area contains a goodly number of ducks, but it's even more important as an area where vanishing species are living. It's an area where, if I remember my figures, 730,000 people went last year to enjoy a bit of God's creation out of doors.

Now, for that area to be turned over to the military as a shooting point to me is a disgrace of major proportions. I think the Administration should be skinned if it is permitted to get away with a thing like that.

Here is a time for action. Here is a time for cooperative work, to do something about it. God knows we need help! [Applause]

CHAIRMAN GAVIN: Thanks very much.

I think all of us should congratulate Ernest Swift for a very, very fine paper.

CANADA'S PLACE IN FLYWAY MANAGEMENT

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In January all but a minute proportion of North American waterfowl may be found south of the Canadian boundary; in July about 70 per cent of the population is north of that line. Generalities based on these facts have been frequently stated and are widely known. Partly through an appreciation of them the Migratory Birds Treaty was entered into in 1916 by the United States and Great Britain. Because of that international commitment the Federal Governments of Canada and the United States are presently engaged in waterfowl research and management.

However, while ducks and geese are within any province or state they are the property of that province or state; and all those governments must assume a measure of responsibility for waterfowl welfare and must also, to satisfy the requirements of their citizens, make provisions for a sustained harvest of reasonable proportions, if at all possible.

As human populations have increased and prospered and have made growing demands upon the waterfowl resource, the governments of the individual provinces and states have most rightfully become more and more concerned with the management of waterfowl.

The states of the Union have realized that independently they cannot properly deal with the many problems of waterfowl management. They have formed flyway councils which have served as forums and planning agencies with respect to their own activities, and in union as an advisory board to the Government of the United States. The next step in their development is to take full cognizance of conditions beyond the United States. Indeed, they are moving in this direction, and the recent participation of state agencies in banding on the Canadian breeding grounds is a fine token of this growing awareness. It is the object of this paper, however, to suggest that an even broader understanding is required.

Let us return to the theme which introduced this paper—the interdependency of Canada and the United States in waterfowl management.

From data obtained by aerial surveys in Canada during 1952, 1953, and 1954, we have calculated an average index figure of approximately 5.5 million breeding pairs of sport-ducks. The data we used have been published in the series of U.S. Fish and Wildlife Service and Canadian Wildlife Service Special Scientific Reports, "Waterfowl Breeding Ground Conditions." The figure just given is an index. It does not represent the total breeding sport-duck population of the areas surveyed. It does not refer to all of Canada, excluding the ducks from more than 1.5 million square miles of the Northwest Territories and northern prairie provinces, southern Ontario and Quebec, the Maritime Provinces and Newfoundland. A comparable aerial index figure for the United States and Alaska cannot be calculated from present data, but indications are that it would be in the order of 2.5 million pairs per year, averaged for the same period.

Very little of the total area of either country is prime waterfowl habitat. That section corresponding to the area of intensive spring wheat production is the core of the continent's duck breeding habitat. It has an area of almost 400,000 square miles, including southern Alberta, southern Saskatchewan, southwestern Manitoba, northeastern Montana, North and South Dakota and western Minnesota. All of the Canadian and part of the U.S. section has become known as the Big Duck Factory. More than 3.5 million breeding pairs of sport-ducks have been indicated by aerial surveys in the Canadian part and almost 1.3 million in the United States section.

Thus it can be said that as current breeding success in Canada goes, so goes the current continental duck crop. It is doubtful that the carrying capacity of the breeding habitat itself has been reached—even in this period of waterfowl abundance. However, with an eye to the future—to drought period and duck scarcities—it would seem that one phase of sound management would call for the bulk of habitat improvement efforts to be carried out in Canada, and particularly in the Canadian wheat belt, where populations in excess of 100 breeding pairs per square mile have been recorded. The U.S. supporters of Ducks Unlimited have long recognized this point.

The autumn migration period means duck hunting to approximately half a million Canadian and two million U.S. hunters. The Canadian kill can be estimated at present only on a partial return of hunting licenses to the provinces. It is usually considered small compared to a U.S. harvest in the order of 14,000,000 ducks. From this aspect, it is apparent that if the duck population is to be increased or reduced through control of hunting, or if it is to be adequately harvested, the effective part of such management must be carried out in the United States.

The winter picture, of course, shows practically all of the continent's waterfowl south of the 49th parallel. Therefore, any management required on the wintering grounds must also be practised in the United States.

It is quite apparent that management of waterfowl in the United States will have its effects not only in that country but also in Canada. Any policy which would either increase the carrying capacity for wintering waterfowl in the United States or reduce the harvest there, could, unless other changes take place, be expected to increase the pressure of ducks in the Canadian breeding grounds. Here it is of interest to note that throughout a waterfowl management plan prepared by one of the flyway councils great stress is laid on methods of increasing duck populations so that the hunter will have more ducks to kill. Other interests than those of the hunter are given practically no consideration. It would seem that such a philosophy can result only from a refusal to be realistic about our waterfowl and the habitat they use—because the most important part of this habitat is private land where the production of waterfowl is not the sole aim of the land-use practice and where in many cases the presence of ducks is considered by the landowner to be a liability rather than an asset. Another flyway management plan indicates a clear appreciation of this situation.

The preceding words have been written considering a period of waterfowl abundance, when, as we will point out later on, heavy pressure of waterfowl in the Canadian breeding grounds has its undesirable aspects.

Were duck populations to decline, and the most optimistic of us would likely say that they will sometime, the effect of U. S. policies on the continental resource would be no less profound. Under such conditions a policy of drastically reduced harvest would be in order, although it might be sufficient to hold the line as far as wintering habitat is concerned.

We hope that by this stage we have established sufficiently the necessity for continental management, without being too tedious about it. We think it follows quite clearly that some problems which may superficially appear to be only Canadian are an international concern.

Sportsmen everywhere are interested in harvestable surpluses. We feel that to consider the question of harvestable surpluses is somewhat academic until they can be related to a desirable population level. This level is normally the maximum population that can be supported year after year in the habitat during the most critical period. In most big-game species the limiting factor is usually food on the winter range in late winter or early spring. We submit that with the principal sport ducks, mallards and pintails, the limiting factor at present is crop damage in the Canadian section of the Big Duck Factory during August and September. This is the period of grain harvest in Southern Alberta, Saskatchewan and Manitoba. It is while the wheat and barley are lying in swath that mallards and pintails too often turn to grain as a steady diet, so much so, that it is now considered their "natural" food.

True, only two of our 14 species of ducks participate in these depredations in Canada. However, these two made up about 70 per cent of the total breeding population of the southern prairie provinces for the last three years. Pintails are not so bad as mallards perhaps, because the majority of them usually leave the country early. By this time, however, other species also have left the depredation areas and mallards alone then outnumber all other species. For this reason the welfare of all species is jeopardized because of the misbehaviour of one or two.

Let us look again at breeding pair indices in the Big Duck Factory —this time for mallards only. There are approximately 189,000 square miles of this prime duck and farming habitat found in Canada. In an average spring for the last three years, approximately 50,000,000 bushels of wheat and barley have been seeded there, and over 1,400,000 pairs of mallards (that is an aerial index) have arrived there to breed. This latter figure, is seven times as large as the corresponding one for those sections of Montana, Minnesota, North and South Dakota referred to above.

How widespread is duck damage to cereal crops in Canada? How many farmers are affected annually? We do not know. In what we might consider a normal year there are probably ten districts in the southern prairie provinces from which damage complaints will be heard. Such districts vary in size from a few hundred to several thousand square miles. In a bad year, like 1951 when damage continued through October and November, complaints may be expected from almost any point in the grain-growing area. Local and provincial farmer unions have taken the matter up, protest meetings have been held by farmers and the question has come up in political meetings. Earlier this month, a resolution requesting that waterfowl generally be shot the year round was presented at the annual meeting of Saskatchewan Rural Municipalities. Western Canada sportsmen are concerned about the farmers' attitude. An indication of the deterioration of conservation ideas built up over a long period is the farmers' deliberate destruction of eggs in areas where a very few years ago nests were carefully moved to one side in cultivation operations.

In any one year a minority of the farmers are affected. Why then is the problem of any importance on an international level? Simply because farmers over large areas, whether presently suffering damage or not, suspect that next week or next season they may be in the same position as their neighbors. They also feel that the damage suffered is a real injustice. They have not found nor been shown any method of control effective under serious conditions. When, as a last resort, they take to shooting the birds, a measure which by then may do little to alleviate the overall damage, they resent the necessary administrative procedures they are required to comply with.

In Saskatchewan, an insurance scheme, partly sportsmen-financed, is available to farmers. A farmer may insure an acre of crop for $5, $10 or $15 against damage by all forms of wildlife for premiums of 25, 50, or 75 cents respectively. Only 49 policies have been sold in the first two years. Possibly the fact that it costs about $14.50 on an average to bring an acre of wheat to the swathed stage has something to do with the lack of public enthusiasm. This figure includes normal charges for cultivating, seeding, spraying, harvesting, labor and depreciation.

What is the magnitude of the losses? We do not know the complete picture. We do have several farmer estimates. A loss of $432,000, was reported for a 360-square-mile section in the Vulcan, Alberta, district in 1951. At The Pas, Manitoba, in 1949, the loss to ducks was estimated at $20,000 to $30,000 on 15,000 acres under cultivation. For Kindersley Municipality, Saskatchewan, in 1951, losses totalling $290,000 were calculated by the Saskatchewan Game Branch after interviewing every farmer in the 324-square-mile district. The average of the 16 cash payments made to date by the Saskatchewan Government Insurance Office for claims of waterfowl damage, has been $540. Every farmer who claimed damage under insurance schemes received some payment. This has exceeded $2,000 in some cases.

Loss of the farmer's sympathy and cooperation may not be too important in years of waterfowl abundance, but what of the drought years? Americans and Canadians are uncomfortably familiar with the growing intolerance of landowners to free access to their privately owned lands for the purpose of harvesting a publicly owned resource. They may expect to become familiar with an intolerance on the part of landowners to the use of their land for the production of a publicly owned resource.

Having thus touched on the major land-use problem on the breeding grounds it would seem in order to review in a somewhat critical manner, pertinent aspects of waterfowl management.

For whom are we seeking to manage waterfowl? Those having a stake in their future are far more numerous than the holders of hunting licences. They also include the present and future generations of men who wish to enjoy nature in the whole fabric, the persons who offer goods and services needed in observing and harvesting the resource, and those for whom waterfowl create economic problems of greater or lesser degree. In theory, and as far as possible in practice, our management policies should reflect the requirements of all, not just one, of the classes just mentioned.

Probably no agency could deny the charge that undue attention has been paid to the demands of sportsmen even though they are often justifiable. The future welfare of the resource requires that our terms of reference be broadened.

We must realize that the farmer is one of several partners in management and that the magnitude of the production effort must be related to his requirements.

We must then recognize in planning and in action that there is an optimum upper limit to waterfowl numbers related to the carrying capacity of the lands concerned; and the carrying capacity must be set at a level so that serious damage to grain crops is unlikely.

To define the optimum will require a continuation of the annual assessment of waterfowl numbers so that a long-term picture of population fluctuations becomes available. Detailed information on the movements of various populations of ducks, now being sought by the intensified banding program, will also be useful in establishing our definition. We must institute a comprehensive annual assessment of waterfowl damage in Canada, at least. While we cannot define an optimum level now, we know that with the present methods of protecting crops from duck damage, populations such as those of the early 1950's are above the optimum.

It is questionable whether any of the conventional techniques of management applied on breeding grounds can put an upper limit on populations when they have reached an optimum level. The major importance of breeding ground management is for times of duck scarcity as previously mentioned. Thus if our present problem is ducks above the optimum level we must look for amelioration by harvest management—unless the situation becomes so out of hand that it is necessary to consider ducks as pests and treat them accordingly. We have no wish to see this happen for it would mean the end of wildfowling throughout a large part of America.

We seem to know very little about the efficiency of the harvest as presently conducted by sportsmen. There is some indication that it has a limit. For instance, if it were deemed necessary, could current mallard and pintail breeding populations be reduced through hunting without having drastic effects on certain other species? Such a reduction should not be considered out of the question. Aldo Leopold visualized such a procedure more than 25 years ago when he wrote "After game shortage has been corrected by management, the purpose may extend beyond mere limitation. It may become necessary actually to enlarge the kill in order to bring the game into a desirable relationship to farm or forest crops, or to regulate its kind and distribution so as to bring about a better or more uniformly distributed stock."

Certainly weather plays a large part in determining the magnitude of the harvest. There is a definite need to ascertain the relative effects of seasons of different lengths, bag limits of different sizes and differential species regulations. We should ask: How significant is the "burnt-out" breeding area concept in times of duck abundance? A long series of results from the U.S. Fish and Wildlife Service post office survey may help to answer these questions in part. Establishment of a comparable system in Canada is needed but will be somewhat difficult in the absence of any federal requirement of license or fee. The possibilities for a broad kill survey in Canada will be explored.

We have already indicated that for effective management of the continental waterfowl resource on a suitably broad basis a number of questions require to be answered. Some will only become clear with the passage of time and only then if the necessary investigations are continued.

There is this point to be made: Our continuing investigations must be subject to critical scrutiny at all times; no routine or policy should become so much a sacred cow that it is exempt from critical examination in the light of changing conditions and advanced understanding. There is a tendency to become so busy maintaining the organization of existing practices that little time is left for careful consideration of objectives and techniques. If in order to do so, it is necessary to slow the pace for a year or so, to set up a period for re-examination, we should not hesitate to do so.

Increased contact of waterfowl biologists at the working level will do much to provide for a continuing reassessment. While a lot can be accomplished by correspondence, exchange of reports and so forth, there can be no substitute for the direct meeting of minds. In the whole sphere of science many of the most promising advances have arisen from universities or other institutions where the prolonged contact of a number of intellects has been a characteristic feature. We should consider means of recreating the university atmosphere on all possible occasions. Technical sessions of the various flyway councils would seem to provide appropriate opportunities.

The Canadian Wildlife Service considers that desirable objectives of waterfowl management best organized on an international flyway basis should be: 1) to determine optimum populations by species and by areas considering the carrying capacity of the habitats utilized during the different seasons; the positive and negative values of the resource—economic, recreational and aesthetic; and the magnitude of depressive factors such as harvest, disease, predation, etc.; 2) to maintain such optimum populations; 3) to seek effective methods of ameliorating crop depredations by waterfowl; 4) to inform the public regarding waterfowl management. None of these objectives can be attained without research oriented to attain them.

DISCUSSION

VICE-CHAIRMAN ANDERSON: That we have necessity for a realistic approach should now be very apparent to all of us. Are there any questions or comments to be directed to Dave Munro on this or any other subject at this time? [There were none.]

Dave, I would like to raise this question. How many mallards do you think it would be necessary for us to kill to reduce effectively the duck depredation say, in the Province of Saskatchewan?

MR. MUNRO: I would hesitate to stick my neck out at all on that. My point in bringing it up was that we should consider that such a thought is one that may be useful to us, and we should continue our reassessments of population and institute an assessment of damage, and design such studies and interpret them in a way so that in a few years we could answer that question.

VICE-CHAIRMAN ANDERSON: Are there any other questions or comments?

DR. CLARENCE COTTAM [Utah]: Regarding this idea that has just been introduced of eliminating thousands of ducks in order to reduce these depredations, if there are 1000 left, they will all be in the middle of that area, and the only way you are going to eliminate those depredations is to wipe out the massed thousands of ducks.

MR. MUNRO: I should say that we don't anticipate that depredation will ever be eliminated entirely. We do think that the likelihood of widespread and serious damage can be reduced.

The essential point in the whole consideration is the maintenance of the good will of these people who own the land that produce our ducks. If we have to give in some—if we have to show them that we are on their side—those are the changes in our thinking that must take place.

VICE-CHAIRMAN ANDERSON: Any other comments or questions? We certainly can't afford to stick our heads in the sand on this matter.

I think it's just as Dave says, however: What we need is to get together and thresh this thing out and answer this question as to just how serious the depredations really are, and how often we have these so-called bad depredation years, and what are the experiences of hunters who have gone into these areas that Dave has described, and who thought they would be excellent places to shoot.

IS THERE SCIENTIFIC BASIS FOR FLYWAY MANAGEMENT?

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In attempting to discuss the scientific basis of waterfowl management by flyways, I have written this paper as a review of the biological principles we are now using or hope to use. I would particularly like to appraise the practical difficulties that we face in placing waterfowl harvests on a scientific basis.

Here at the beginning, I would like to restrict my use of the word *science* to accumulated and accepted knowledge which has been systematized so as to permit the discovery of general truths. An immature science often involves the slow accumulation of facts which will ultimately be fitted into a mosaic permitting one to perceive the operation of general laws. In a mature science, these laws are sufficiently perceived so as to permit one to make predictions about the future. Waterfowl biology has, for instance, attained its greatest degree of maturity in its grasp of the migratory behavior of ducks and geese, in its awareness of many of the migration routes that are used, and in its understanding of the species composition of birds on the breeding and wintering grounds and at migratory concentration points. It is scarcely necessary to point out that this body of knowledge formed an important scientific foundation for the initial acquisition of our great national wildlife refuges. It is used by each state in the selection of dates for the annual hunting season. It governs the distribution of law-enforcement officers. In short, it permits planning for the future.

Parallel to this science is an *art* which consists of a collection of skills acquired by experience and observation. The art of waterfowl management extends to the management of man, as when we continue to plug loopholes in our laws so as to simplify our control of poaching. It extends to the management of habitat, as when we choose a quiet night in which to burn *Phragmites*, or elect to gamble by plowing on a given day, or knock down a field of corn in order to hold some newly arrived geese.

A primary aim of modern game management today lies in its hope of changing as many of its techniques as possible from an art to a science of game management (McCabe 1954). A few years ago, Clarence Cottam (1949) and Harlow B. Mills (1951) each reviewed the factual basis of waterfowl management and emphasized the extremely wide variety of information that still needs to be discovered before the management of these populations can be said to have attained the level of a science. Are we practicing the art of waterfowl management today? Or are we well on the road to making it into a science? Are there practical reasons why some aspects of waterfowl management will always remain an art to which science can make only a minor contribution? These are the questions I would like to face in this paper.

HABITAT MANAGEMENT

Management of waterfowl habitat today is partly an art and partly a science. The scientific basis is clearest in the dependence on hydrological principles for the availability of water and on the engineering sciences for the permanency of dikes, spillways and the like. Where habitat manipulation has had to face biological problems, waterfowl managers have borrowed from agriculture, range management, plant and animal ecology, and animal psychology when they could. In many cases, however, there are no ready-made formulas, and our whole habitat-management program has, in a sense, been a continuous research project in which practical operations have constantly served as experiments. One firm foundation for this program was the early and long-continued work of the Bureau of Biological Survey in ascertaining the food habits of waterfowl and in exploring methods of propagating aquatic plants (McAtee 1911 *et seq.;* Cottam 1939; Martin and Uhler 1939).

The complexity of aquatic-habitat management is well illustrated in the varied uses of drawdowns and grazing by waterfowl managers. A drawdown may destroy food plants on a TVA lake (Steenis 1950b), or serve as the initial step in the creation of a dense stand of valuable smartweed in Michigan (Pirnie 1935:213), or make beds of submerged aquatics available to puddle ducks in the South (Griffith 1948). Summer grazing by livestock may be successful in readying a Wisconsin field for autumnal geese (Hughlett, personal communication); its elimination on a unit of short-grass country may increase the number of duck nests as much as 30 per cent (Griffith 1948). Used judiciously, grazing may create edge effects that will be helpful as brood cover; used carelessly, it can completely eliminate softstem bulrush and reduce millet and smartweed by 80 per cent (*ibid.*). There are important ecological principles which govern such changes in the production or availability of food; but when much of the operation depends for its success upon the experience of the manager and his intimate familiarity with his marsh, the management process must at least be considered as much of an art as it is a science.

The use of upland farming to supplement natural supplies of waterfowl food has become an increasingly important principle in our adding to the carrying capacity of many waterfowl refuges (Griffith 1948, Horn 1949, Steenis 1950b, Atkeson and Givens 1952). I have heard agriculture characterized as half art and half science. While this division undoubtedly varies with the type of farming, it clearly implies that the relatively recent practice of waterfowl-habitat management will require a lot more research before it stands upon its feet as a series of scientific procedures.

At the present time, research is serving to broaden our management of waterfowl habitat by fundamental studies of the ecology of the aquatic plants we require (Chamberlain 1948, Crail 1951, Cottam and Bourne 1952) and of the ability of these plants to carry given numbers of ducks or geese (Lynch, O'Neil and Lay 1947; Griffith 1948; Horn 1949).

It is likewise succeeding in giving us new tools, as in the use of herbicides to control aquatic weeds (Eicher 1947, Gerking 1948, Steenis 1950a). It is evaluating older tools, like burning, for their effects on waterfowl foods (Singleton 1951). And it is testing new techniques, like the transplantation of ducks and geese to new or vacated breeding grounds (Johnson 1947, McCabe 1947, Bellrose 1953a), the artificial creation of small-water areas (Bue, Blankenship and Marshall 1952, Brumsted and Hewitt 1952), extensive systems of nesting boxes (Frank 1948, McLaughlin and Grice 1952, Bellrose 1953a), and the use of dynamite in the creation of edge (Mendall 1949).

Although this sums up to an increasingly impressive record of technological achievement, it is true, as Griffith (1948) says, that "there is no hard and fast rule that can be recommended towards the treatment of a particular tract of wildlife habitat to increase its productivity." In effect, the art of marsh management will always be wedded to the science of marsh ecology. Certainly, one's net impression of aquatic-habitat manipulation today is that it is proceeding on a rapidly expanding scientific basis. A major problem today lies not in how aquatic habitats shall be managed, but where and with what dollars. It is in this administrative strategy that flyway management can make an important contribution.

CONTROL OF NATURAL MORTALITY

Techniques of reducing nonhunting mortality in waterfowl are evolving more slowly. Just to put the picture at least into a partial focus, I would estimate that less than one million waterfowl simply vanish each year from this continent.

Where natural mortality has been locally concentrated, research workers have a chance to meet the problem head on. Mortality from a botulism outbreak can now be reduced by about 95 per cent with techniques such as the herding of birds, removal of the dead, and changing water levels (Rosen and Bischoff 1953). The success of these practices has a firm scientific basis contributed by many investigators and stands as a proud achievement. Aside from nest predation and the occasional outbreak of lead-poisoning, fowl cholera, or algal poisoning, the evidence for most nonhunting mortality in waterfowl disappears so rapidly in the field as to make the progress of research tenuous and extremely difficult to plan.

The report of Ducks Unlimited (Canada) that over 5,000,000 crows and blackbirds have been destroyed since 1937 is an interesting example of an attempt to reduce mortality at the nest stage. This program has its basis anchored in research that Kalmbach (1937) carefully labelled as "preliminary studies." Between the original finding and the present control program, two steps are missing. One of these lies in an evaluation of the net effect of the predation in reducing the total number of young raised; the second lies in controlled experiments that test the actual effect of a crow- and blackbird-control program upon the production of waterfowl. I am here neither disparaging nor advocating crow control. I am observing that the destruction of so many birds does not rest upon a sound published scientific basis.

Other techniques to reduce nesting losses include the creation of islands for geese (Johnson 1947), elimination of grazing by livestock (Griffith 1948), control of water-level when this is possible (Mendall 1949), the artificial creation of flood-proof nesting platforms for geese (Yocum 1952), and devolopment of a predator-proof nesting box for wood ducks (*Aix sponsa*) (Bellrose 1953a). While these have a somewhat local application, the wood duck box also has state-wide possibilities in creating niches for this species in stands of immature timber, as McLaughlin and Grice (1952) have shown. The new box deserves a wide-scale tryout in the Atlantic and Mississippi Flyways, with boxes concentrated on watersheds which can be checked for brood production, and comparisons made with other unmanaged watersheds left as controls.

Control of natural mortality is characterized by the regional importance of the factors to be faced: pollution in the industrial East, raccoons (*Procyon lotor*) in the Mississippi Valley, crows (*Corvus* *brachyrhynchos*) on the prairies, and botulism in the Far West. There is a clear need to integrate our management of the wood ducks-raccoon problem in the Atlantic and Mississippi Flyways, and to develop a sound approach to the crow in the Central Flyway. The coordination of ideas and techniques on botulism I take as an accepted fact. Progress in the control of natural mortality will undoubtedly be accelerated by flyway-management plans. The progress will be slow, but it will, I think, be concrete.

Two other phases of waterfowl management involve control of the harvest taken by hunters and control of depredations. The latter is a relatively new and increasingly important problem on which Horn (1949) has reported progress on the wintering grounds of California and on which Hochbaum, Dillon and Howard (1954) have reported some progress on the breeding grounds in Manitoba. Because the harvest is of paramount interest to so many hunters, and because its biological basis is so little understood, I would like to devote the rest of my paper to a discussion of this subject.

WHAT BIOLOGICAL PRINCIPLES UNDERLIE WATERFOWL HARVESTS?

In attempting to answer this question, I am deliberately omitting any mention of harvests which are largely determined by economic factors such as real or potential crop depredations by waterfowl on their breeding grounds or their wintering grounds. This phenomenon is regional in character, and the harvest is, of course, designed to prevent the birds from attaining a given population level. The more normal waterfowl regulations are aimed in principle not at merely providing interest on a capital investment but at yielding the maximum interest without damage to the capital stock. And by capital stock, I mean the maximum breeding population consistent with the greatest number of young birds that can be brought to the opening of the hunting season.

Twenty-five years ago, the biological basis of hunting was thought to be a matter of simple arithmetic. "If satisfactory sport and a safeguarded breeding stock are desired on the same ground year after year," Stoddard (1931:226) wrote of bobwhite (*Colinus virginianus*), "the number of birds harvested by man must be offset by control of natural enemies, improvement of coverts, or restocking."

This thinking began to change in the 1930's with the intensive studies of predation by Errington (1935, 1937) in Wisconsin and Iowa. The theory emerging held that predators, parasites, disease, and/or food shortages act in a complementary fashion upon most wildlife populations and that one or more of these, acting in combination, level off the fall and winter surpluses produced each summer. Within this system, various kinds of predators likewise act in combination. In effect, then, well-regulated hunting should be able to remove a part of the fall population without affecting the population next spring (Errington 1935, 1936). The critical test of this hypothesis rests upon controlled experiments. Three of these have been carried out on bobwhites with generally confirmatory results (Errington and Hamerstrom 1935, Baumgartner 1944, and Mosby and Overton 1950).

Similar experiments by Glading and Saarni (1944) revealed that hunting does affect spring population levels of California quail (*Lophortyx californica*). The shot population, however, gained 97 and 109 per cent in two reproductive seasons in contrast to gains of 38 and 58 per cent on the control area. The principle governing this recovery phenomenon was Errington's (1945) rule that the percentage of summer gain tends to be inversely proportional to the density in spring. Harvests of 24 and 36 per cent did not affect density the following fall; a 38 per cent harvest did.

One should notice that the reliability of these tests of two biological principles underlying hunting harvests importantly rests on the amount of unrecorded ingress and egress of birds on the study areas. Errington and Hamerstrom (1935) felt sure that this occurred on some of the plots that they censused, and for final and positive proof, Glading and Saarni (1944) suggest the use of much larger acreages than the ones they used. It is, I believe, understood that these two principles (hunting as a complementary component of predation; hunting-accelerated rates of reproductive gain) can and probably do operate together on game bird populations.

As to how these two ecological principles underlie our present system of harvesting waterfowl, I am afraid we usually do not know. This is basically due to the practical problems our field men face in censusing these highly mobile species and determining their productivity. The answers may be different for ducks and for geese, and in view of the relatively unstable water conditions on our prairies, they may not hold for any long period of years. The recent development of Flyway Councils clearly focuses the search for these answers on a broad regional basis, where it should be.

If moderate hunting pressure does not affect the population in spring, then the biotic factors limiting the population operate chiefly in late fall or winter. If the effect of hunting is not counterbalanced until the following summer or fall, then the biotic limiting factors operate on the breeding grounds. In the first case, a conservative hunting system takes a winter surplus; in the second it essentially takes a spring surplus that is, in effect, reproductively inefficient. The distinction between the two loses much of its importance when economic forces clearly or potentially set a limit to the number of waterfowl which a given breeding or wintering area can carry. It takes on practical importance when management can offset the operation of a limiting factor by habitat development or curtailing the effect of some biotic factor.

At the present time, it seems likely that verification of the biological principle (or principles) underlying our harvest of waterfowl will occasionally come from straight deductive thinking and our experience with ecological phenomena that are taking place. The spectacular upward surge of the Canada goose (*Branta canadensis*) population in the Mississippi Valley in recent years is, in the opinion of many waterfowl workers, almost certainly a response to the increased carrying capacity of this range, largely brought about by the wide-scale use of mechanical corn pickers in that region. This change in population level must mean that conservative harvesting of this species, when it did take place in the past, operated on the principle of complementary predation.

The picture for Mississippi Valley mallards (*Anas platyrhynchos*) is complicated by the increase of carrying capacity accompanying the expanded culture of domestic rice (Hawkins, Bellrose and Smith 1946) and by great losses of winter range brought on by the construction of levee systems by the Corps of Engineers (Anderson 1955). While these two forces may not cancel each other completely, the northward march of agricultural drainage and clearing strongly implies (to some of us at least) that the mallard population in this great region is in general limited by its breeding range (Gavin 1953; Mair 1953, 1954).

If one turns to the Atlantic seaboard, one can find Wright (1948) concluding that there is no need for habitat improvement or predator control on the waterfowl nesting grounds in eastern Canada; while, on the other hand, Williams (1950) believes that management must concentrate on a small-marsh-development program in the northeastern states and southeastern Canada. If Wright's conclusion is valid, conservative harvesting in this flyway is today based on hunting that is a complementary component of winter predation, winter diseases and the like. If Williams' belief is correct, conservative hunting in this region serves to increase rates of summer gain.

Still another school holds that local populations in the interior are being wiped out by hunting pressure (Hochbaum 1947). It is against this background that the need of management for more facts on waterfowl has been repeatedly stressed by writers and speakers on waterfowl: by Cottam (1949), Bellrose (1950), Mills (1951), and Olds and Swift (1953). As the latter emphasized two years ago, the states are willing to assist in waterfowl management with all their resources, but they need to know first whether the present breeding and wintering grounds in each flyway are being fully used by the birds.

Having thus found an academic question closely interwoven with a practical management problem, I think we can answer our original question about the biological principles underlying waterfowl harvests as follows: Scattered and rather preliminary research on upland game birds suggests the existence of two principles: (1) that conservative hunting is offset by decreases in natural mortality in winter and (2) that such hunting can also be offset by automatically increased rates of reproductive gain. These quite possibly have different degrees of application for ducks and geese. The differences may also extend to individual species as well as to regions or flyways. At the present time they are not clearly identifiable with the harvest of many waterfowl populations.

WHAT FRACTION OF THE POPULATION CAN BE HARVESTED?

As far as I know, this question was first raised at a North American Wildlife Conference by Harlow Mills in 1951. The question is a challenging one, especially when we realize that modern research has repeatedly disclosed how many species of fish and game were being underharvested under modern conditions.

Efforts to turn the art of harvesting game-bird populations into more of a science have necessarily awaited development of a reliable system of forecasting population levels available for hunting. Such a science developed rapidly around the ring-necked pheasant (*Phasianus colchicus*), but harvest principles based on a sexually dimorphic polygamous species (D. L. Allen 1947) are for the most part different from those that can be set up for monogamous species like waterfowl.

At our present stage of research and development, we are moving into a position where we can determine just what fractions of certain populations are taken by hunters each year. The trend of inquiry is proceeding along two lines. On the one hand, there are new and increasingly precise measurements being made of the harvest. At least for some species in which the population can be accurately inventoried in January, these kill estimates can be converted into fractions of the population. Thus for Canada geese, Hanson and Smith (1950:192) were able to estimate that a hunting kill of 37 per cent and natural mortality of 18 per cent dropped the Horseshoe Lake population 28 per cent from 1944 to 1945. One year's production could make up for a 27 per cent total mortality; it could not make up for a 55 per cent drain.

The second line of inquiry utilizes raw banding data in which hunters furnish all or most of the reports of recoveries. Attempts to convert these recovery rates into percentages of the birds that were bagged in each flyway have been held up by (1) the failure of some hunters to report bands (Leopold 1933:156), (2) by the early emphasis on banding during the hunting season, and (3) by the probability that trapped samples are not randomly selected components of large regional populations (Hickey 1951, Crissey 1955). I would like to review here briefly the published progress that has been made in this connection on mallards in the Mississippi Valley.

Bellrose (1945, 1955, and Bellrose and Chase 1950) has been particularly aggressive in clarifying the first of these variables for mallards banded in Illinois. It is now apparent that, for every 10 bands reported for Illinois-trapped mallards, at least 21 banded birds are actually bagged; and, as Bellrose (1955) points out, the actual figure bagged must be higher. As a working approximation of the correction factor for this variable, I use 2.3 times the reported recovery rate. This may be regarded as a minimal estimate, and Bellrose (1955) has suggested that correction factors of even 2.5 or 3 are possible.

Over an 8-year period, 9.9 per cent of DU's summer-banded adult mallards were shot and reported by hunters in the hunting season that immediately followed (Hickey 1952:127). If we multiply this figure by 2.3 for unreported bands, we have 22.8 per cent of this sample bagged by hunters. This represents, of course, a minimal estimate and stands in contrast to $11 \times 2.9 = 32$ per cent calculated for Illinois mallards by Bellrose and Chase (1950).

Now this 22.8 per cent is simply an estimate of an 8-year average of mallards bagged. The magnitude of annual variations in some samples of the mallard population may run as high as 35 per cent for a North Dakota refuge in 1939 and as low as 13 per cent for some of DU's birds in 1941 and 1943 (Table 1).

This banding technique and other sampling methods for determining the hunting kill still require a further estimate of unretrieved cripples that ultimately die from gunfire. Our past reliance on hunters' reports of birds knocked down has permitted us to compare crippling losses from different types of shooting and from different places. While Bellrose's (1953b) tabulations of the kinds of wounds encountered in bagged, crippled and trapped birds possibly open the

TABLE 1. MINIMAL ESTIMATES OF PERCENTAGES OF BANDED MALLARDS BAGGED IN THE SAME SEASON IN WHICH THEY WERE BANDED[1]

Where Banded	Canada	N. Dakota	NE. Ill.	Ill. River
1939	....	35	....	7
1940	21	33	....	16
1941	13	20	....	6
1942	15	....	18	13
1943	13[2]	....	12	....
1944	25[2]	....	22	....
1945	24[2]	....	20	....
1946	22[2]	....	16	....
Banders	DU Canada	Hammond Henry	Jedlicka	Bellrose

[1]These birds inclule both adults and juveniles; correction factor of 2.3 used for unreported bands; band recovery rates taken from Bellrose (1944) and Hickey (1952:127).
[2]Summer-banded only; all others include birds trapped during the hunting season.

door to a statistical evaluation of this phenomenon, it more and more looks to me as if we will eventually have to compare the bag at checking stations with the numbers of cripples that our dogs can pick up immediately after the opening weekend of a season.

For the present, I am willing to take hunters' reports of cripples only as a crude and wholly preliminary estimate of this added drain on the population. Bellrose (1953b) has tabulated reported cripples from six states and shown that these average 22.5 per cent of the birds found in hunters' bags at checking stations. According to my reckoning (22.8 per cent bagged times 22.5 per cent), this represents 5.1 per cent of the population being studied. This figure is so small that any further guesswork about which cripples recover is academic in our review. I assume the cripples to be lost. The total average drain on these banded segments of the population is about 28 per cent.

With these reservations in mind, I think it is instructive to tabulate what little we know statistically about current rates of harvesting monogamous game-bird populations in North America (Table 2). Because the extent of nonhunting mortality is virtually unknown in game birds, I have substracted known or recommended hunting-mortality rates from average adult over-all mortality rates to see what non-hunting rates are like. The subtraction is defensible only if one subtracts averages from averages. Lines 1 and 4 refer to hunting mortality on experimental areas and should be regarded as exploratory only. Line 7 compares an approximate adult mortality for a supposedly stable population of Canada geese with hunting and nonhunting rates calculated by Hanson and Smith (1950:192) for a specific year when the population decreased 28 per cent. A second limitation to this comparison is imposed when (a) the hunting-mortality rates involve both adult and juvenile components of the

TABLE 2. HUNTING AND NONHUNTING MORTALITY RATES (REAL OR ESTIMATED) IN MONOGAMOUS GAMEBIRDS

Annual over-all mortality rates (a) here represent averages. Hunting mortality rates (b + c) tend to refer to study areas. The nonhunting mortality rates (d), obtained by subtracting (b + c) from (a), can only be regarded as rough estimates of the magnitude of this phenomenon. This subtraction was not carried out for the Canada goose.

	(a)	(b)	(c)	(b + c) = (d)	a — (b + c)	
		Annual Hunting Mortality				
Species	Average Over-all Adult Mortality Rate	Per Cent Bagged	Per Cent Crippled	Sub-total	Annual Nonhunting Mortality	Ref.
1. Bobwhite	83			38	45	1
2. Bobwhite	83			50	33	2
3. Blue-winged Teal	57	7	2	9	48	3
4. Calif. Quail	50	25	11	36	14+	4
5. Mallard	48	32	9	41	7	5
6. Mallard	48.7	23	5	28	20.7	6
7. Canada Goose	ca.40	28	9	37	18	7

References

1a, 2a, 4a	Estimated from age-ratio data by Hickey 1955
1 (b + c)	One-year experiment by Mosby and Overton 1950
2 (b + c)	Generally recommended figure (Davison 1949:134-135)
3	Bellrose and Chase 1950
4b, 4c	Glading and Saarni 1944
4d	Obtained by subtraction; since the experimentally shot population dropped below that of the control in March, nonhunting losses were probably higher than 14 per cent.
5	Bellrose and Chase 1950; Illinois-banded birds
6a	Hickey 1952:159
6b, 6c	As explained in the text
7a	Round figure taken from Fig. 80 in Hanson and Smith 1950:186. This refers to birds banded in 1925-1932.
7 (b + c), d	Calculated for 1944-1945 for the Horseshoe Lake flock by Hanson and Smith 1950:192; population decreasing.

population and (b) the juveniles are killed at a higher rate than are the adults. This may limit the significance of the nonhunting rates derived here for California quail.

My own calculation of an average hunting-mortality rate for mallards differs quite importantly from an early one estimated by Bellrose and Chase (1950) and Bellrose (1953b). The difference in the two estimates principally arises from a choice in the correction factor to be used for unreported bands. I am not sure how much Bellrose would revise his first estimates in the light of his recent findings (1955) on the failure of hunters to report bands they recover.

On a previous occasion (Hickey 1952:79), I used age ratios and banding data in an attempt to determine the percentage of redheads (*Aythya americana*) shot by hunters. The results for both adult and juvenile components of the population are limited by the time period of the banded sample used (birds banded 1926 to 1935), and by my failure to represent the age ratio in hunters' bags in an accurate and realistic manner; I believe these results should be rejected.

As a general rule, I think we can observe that hunting mortality rates must bear some relationship to over-all mortality rates if hunting itself functions as complementary predation. In species that are heavily cropped, the hunting mortility rate is at least one-half of the over-all annual mortality rate. This can be noticed in Figure 1 in which I have emphasized our best available estimates of average conditions.

On the whole, it would be better to add two more points to this graph before we base any hard and fast conclusions on it. The biological evidence currently available suggests to me that we could probably harvest 32 per cent of our adult blue-winged teal (*Anas discors*) instead of 9 per cent (estimated as the current harvest for Illinois-banded birds by Bellrose and Chase 1950). It also seems possible that 20 per cent would prove to be a conservative harvest figure for adult Canada geese. While higher harvesting rates presum-

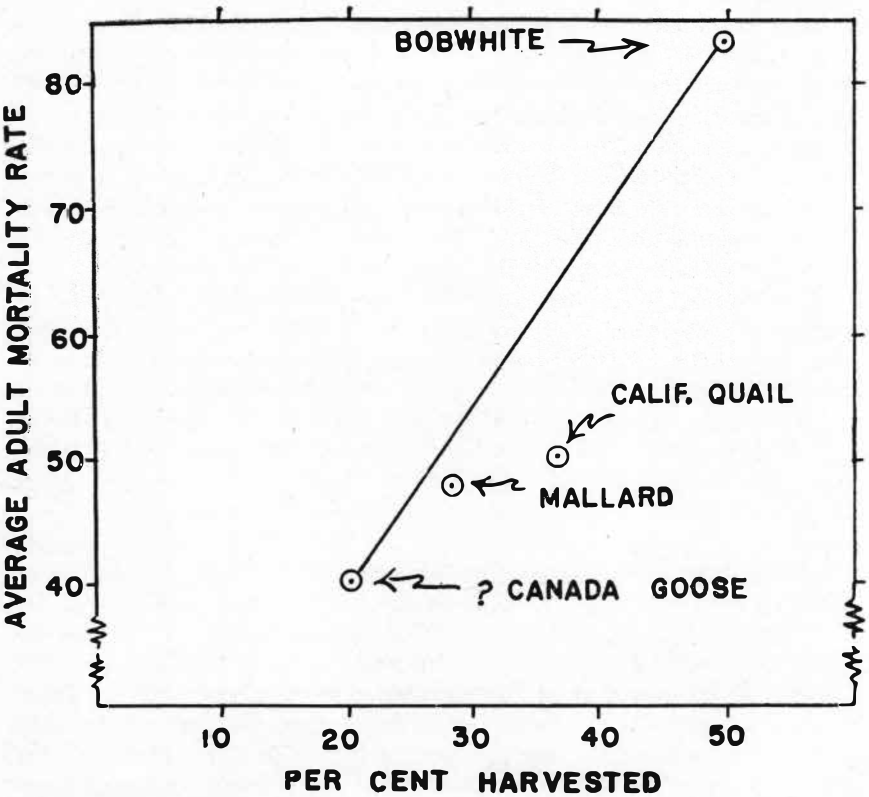

Fig. 1. Conservative hunting-mortality rates currently appear to be one-half to three-fifths of the average over-all adult mortality rate in monogamous gamebird populations. These hunting-mortality rates include crippling losses. A more instructive comparison would relate hunting-mortality to over-all mortality rates for both adult and juvenile segments of the population.

ably apply to juvenile birds in waterfowl populations, it is difficult at the present time to make any preliminary generalizations about them. This is particularly regrettable in the case of Canada geese (where juveniles are so vulnerable to the gun), but perhaps not critical for our purpose. From a table in Hanson and Smith's monograph (1950:192), I would judge that a 19 per cent harvest of the population would have kept it in balance in 1944-45.

I think the meager information assembled here suggests that DU-banded adult mallards, which formed the basis for my calculations of hunting mortality, were harvested in the early and mid-1940's in a conservative manner analagous to recommendations offered for bobwhite quail by Davison (1949) and for California quail by Glading and Saarni (1944). Mallards banded on the Pacific Coast in the 1920's sustained average over-all mortality rates of 64 to 67 per cent per year (Hickey 1952:145a). Since this occurred at a time when bag limits averaged 26 birds and the season lasted three months, there is a strong implication that the increase in over-all mortality over modern rates of 45-49 per cent per year was due to hunting. In the absence of population indices in the 1920's, it is now difficult to prove that high harvest rates of the order of 45 per cent mean a declining mallard population. Judging from what we know of quail, hunting mortality of this magnitude is out of line for the mallard.

Hochbaum (1947) has described how 1946 was the first year in which he could find perfectly suitable breeding habitat in the Lake Manitoba basin, without breeding waterfowl. Since the kill there in the following autumn was the heaviest on record, he believed that overshooting was locally responsible for the decline in breeding ducks. In 1939 and 1940, first-season band-recovery rates of 15 and 14.2 per cent for mallards trapped by Hammond and Henry at the Lower Souris National Wildlife Refuge in North Dakota far exceeded the average of 9.9 per cent obtained for the birds DU banded in Canada (Hickey 1952:127). If the familiar unreported band bias and crippling losses associated with such recovery rates are of the same order of magnitude as those reported for Mississippi Valley mallards by Bellrose (1953b, 1955), I believe we would have to conclude that this North Dakota segment of the population was being overshot. These two pieces of evidence lead one to infer that our modern hunting regulations are on the average permitting us in the interior of the continent to harvest close to the maximum percentage of the mallard population biologically possible. The margin is, in fact, so close that the maximum is occasionally and locally exceeded.

Band-recovery data (Table 3) demonstrate that we are harvesting

TABLE 3. SOME BAND-RECOVERY RATES FOR NORTH AMERICAN WATERFOWL

Species	Banded in North America[1] No. Banded 1923-46	Banded in North America[1] Per Cent Recovered to 1946	Banded by DU. 1939-50 Per Cent Recovered to 1950[2]	Banded in Illinois Per Cent Recovered First-season Only[2]
Gadwall (*Anas strepera*)	5,445	17.4	13.4	11
Canada Goose (*Branta canadensis*)	15,076	15.7	....	....
Mallard (*Anas platyrhynchos*)	259,986	15.6	15.6	6.4
Black Duck (*Anas rubripes*)	96,354	12.8	....	....
Redhead (*Aythya americana*)	12,987	12.8	18.6	8.0
Baldpate (*Mareca americana*)	16,163	12.7	12.6	6
Shoveller (*Spatula clypeata*)	4,841	10.4	9	8
Greater Scaup (*Aythya marila*)	2,851	10.3	....	....
Pintail (*Anas acuta*)	172,900	10.2	9.4	6.0
Ring-necked Duck (*Aythya collaris*)	22,064	(9.3)	....	5
Blue Goose (*Chen caerulescens*)	1,000	7.0	....	....
Canvasback (*Aythya valisineria*)	5,390	7.6	14	....
Green-winged Teal (*Anas carolinense*)	41,329	6.7	5.5	5
Lesser Scaup (*Aythya affinis*)	40,770	(5.7)	9.4	5
Wood Duck (*Aix sponsa*)	9,277	5.5	....	5.4
Cinnamon Teal (*Anas cyanoptera*)	2,678	4.8	....	....
Blue-winged Teal (*Anas discors*)	46,357	4.6	3.9	2.4
Ruddy Duck (*Oxyura jamaicensis*)	900	3	....	....
Source	U.S. Fish and Wildlife Service banding files, 1947		Cartwright and Law 1952	Bellrose

[1]An explanation of the derivation of these data as well as some reservations about their interpretation is given in Appendix I.

[2]Percentages not carried past the decimal point refer to samples of less than 1,000 birds banded.

other species of ducks at markedly lower rates. For the most part, there is no doubt that many of these other species could biologically sustain markedly higher rates of hunting mortality than the ones these recovery-rates imply. Whether or not such a change could be effected in practice remains to be seen. This leads us to our next question.

HOW FAR CAN WE REFINE OUR HARVEST SYSTEM?

If any further proof is needed that regulation of our waterfowl harvest is an art rather than a science, one need only point to the scarcity of published research work evaluating bag limits and other prohibitions for their effect upon the mortality (or survival) of waterfowl. As Mair (1954) points out, our knowledge of the effect of specific regulations is still generally qualitative and not quantitative.

Modern duck-hunting differs so much from goose hunting that we can consider the two as separate harvest systems in many parts of the continent. The duck kill is significantly characterized by the manner in which it is dispersed over great areas; the goose kill often is locally concentrated. Let us first consider the effect of regulations on ducks.

Ducks. The most thorough and objective of the published evaluations of regulations have been those carried out in Illinois by Bellrose (1944) and in Utah by Van den Akker and Wilson (1951). There are three significant conclusions brought out in each of these studies: (1) bag limits had little effect upon the kill when set above a certain point; (2) although short seasons are probably more effective in limiting the kill, the total bag does not seem to be proportional to length of season; (3) setting special bag limits for different species as we did in the 1930's and 1940's is a technique of doubtful effectiveness.

In flyways where biological rather than economic limits govern the rate at which we want to harvest our ducks, harvest regulations will apparently have to reflect the general failure of duck hunters to identify many of their birds before they are shot. This is, I think, a generally accepted condition which is backed up by the Bellrose's (1944) research on the wood duck, the experience Van den Akker and Wilson (1951) report on protected diving ducks, and Elder's (1955) conclusion that size of the target offered by each species in general determines the average rate at which each species of duck is now being harvested. There are, of course, specific behavioral or ecological factors that tend to abrogate the effect of Elder's rule: the pelagic feeding behavior of eiders (*Somateria* sp.) and old-squaws (*Clangula hyemalis*), the early movement south of the blue-winged teal, and the coincidence of the wood duck's breeding range with areas of intense hunting pressure.

With the mallard apparently bearing about the maximum rate of harvesting in the Mississippi Valley that it can sustain, and with local overshooting evident, a corrolary to Elder's rule would embrace the idea that any generalized attempt to increase the take on currently underharvested species in this flyway will jeopardize the main species at which the regulations are aimed.

I think the rate of harvest could be safely increased on an under-exploited species like the blue-winged teal by means of special hunting seasons which would have to be confined to areas (a) south of the breeding range of the mallard and perhaps of the black duck, and (b) away from waters frequented by the wood duck. This is a pretty large order. Perhaps the first step toward it would be an intensive management program in the South to raise the wood duck population to a point where it could sustain hunting pressure from regulations definitely designed to harvest more blue-winged teal.

The selective raising of harvest rates is a tough problem not because of its biological aspects but because of its human relationships. What waterfowl managers need today is a public acceptance of the professionals' freedom to experiment in this connection and a public willingness to allow such experiments to be carried out locally without reference to "penalty days" and the like.

I think it is well to ask at this point if a harvest system can ever be precisely designed for our principal game ducks like the black duck, mallard, and pintail. Can it ever be really "scientific"?

Nearly every duck hunter is aware of the effect of weather on his bag, and this is particularly true of hunting conditions where migrants are being shot. In the Lake States, conservation-department estimates of the state-wide kill show varying degrees of synchronization, as Figure 2 demonstrates. The year 1947 was a generally poor one for

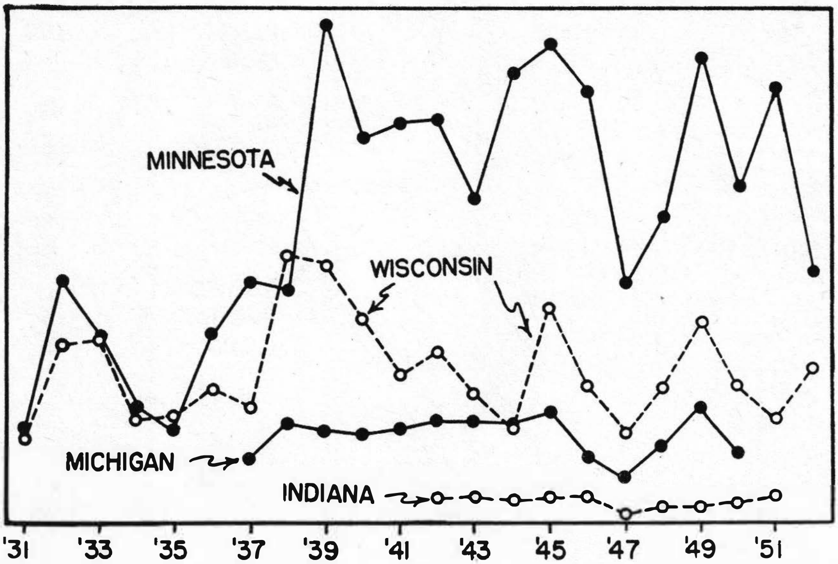

Figure 2. Estimated waterfowl kill in four states in the Middle West

wildfowlers in this region, but in the absence of good population indices, the effect of weather on the kill is not evident.

Weather effects are, I think, evident in the variations that one finds in first-season band-recovery rates (the number of birds reported shot in the same season in which they were banded divided by the number banded). This is clear in Figure 3 in which you will notice that the kill in 1941 and 1943 failed to bear any relationship to length of season as established in the previous year.

What waterfowl hunting regulations do is to hold for average conditions. Restriction of the length of season has, on the average, been associated with a 50 per cent reduction in band recovery rates (Bellrose 1944). It has also been shown to have coincided with a marked

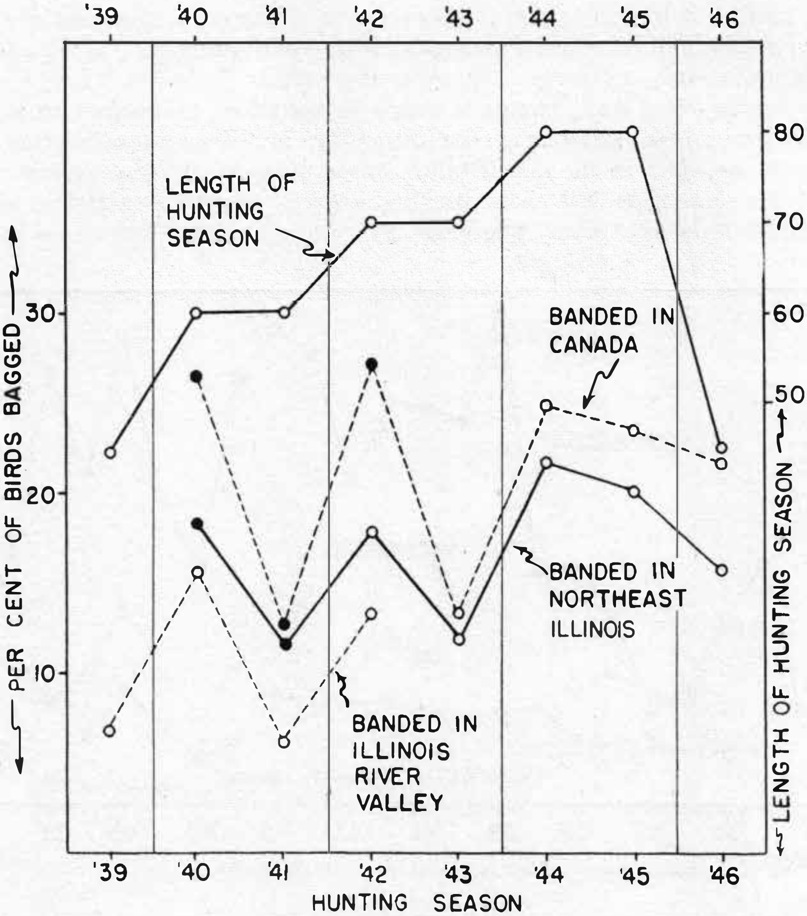

Fig. 3. Relation of length of hunting season to the percentage of unaged mallards bagged in the same season in which they were banded.

reduction in average over-all mortality rates of adult mallards on the Pacific Coast, from about 64 per cent per year to less than 50 per cent (Hickey 1952:144). The point is not that regulations are ineffective but rather that they hold for average conditions. They are an important adjunct to the art of managing the waterfowl crop. Under the present system, a regulations committee makes every attempt to utilize the best information available about the size of the duck population that is expected to start south. If you will recall the present vagueness of our 30-day weather forecasts, you will appreciate our current inability to fix or forecast the duck kill with real precision in many parts of the country 60 to 120 days later.

Does this mean that we should abandon a lot of our summer inventories and let an assumed average population level be the basis of averaged-out hunting regulations? One of the most remarkable waterfowl discoveries of the past decade centers in the very great fluctuations in the productivity of mallards encountered by Bellrose and Hawkins in Illinois and Arkansas (Hawkins, Bellrose and Smith 1946). In delaying the annual announcement of waterfowl regulations each summer, the Fish and Wildlife Service attempts to keep abreast of these fluctuations. In baseball terminology, it is playing the percentages. For the Service to abandon the inventory work so ably developed thus far would, it seems to me, be akin to a baseball manager failing to keep track of the number of balls and strikes.

Geese. Many goose populations in North America today exhibit three of the four requisite characteristics which make a scientific management of their harvest possible. They have relatively narrow migration routes and well-delineated breeding grounds that can often be associated with precisely defined wintering areas. The breeding population lends itself to an actual count (in this case on the wintering grounds). The kill, being largely concentrated at a few places, lends itself to weekly enumeration. What is absent is a good index of breeding success, or at least an index that can be taken into consideration by a regulations committee.

At the present time, goose populations are largely harvested according to the number we count in January and an educated guess as to their probable productivity. I think that we are definitely moving toward goose seasons in which the kill will be fixed as so many thousand birds, that the U.S. Fish and Wildlife Service may have to set the kill for each state or rather for the sum total of the principal harvesting places in each state (perhaps as a percentage of the duck stamps sold in that state during the preceding year), and that seasons will remain open until the set kill figure is reached and no longer.

The biological obstacle to such a system is, I think, surmountable, despite the prohibitive costs of running aerial transects for broods across the tundra. We can, I think, make our regulations in August, sample productivity at September stopping-off places, and modify the regulations well before geese hit the big refuges in the states in October or before the kill mounts in November. Such a system, to me, implies controlled goose shooting at all the major sites of the goose kill today. The margin of error in the final kill outside such places will not be large. Managed goose hunting as it is appearing on an experimental basis at places like Horicon, Wisconsin (Jahn, Gunther, and Bell 1954), certainly holds the promise that this segment of our waterfowl population will eventually be harvested on a scientific basis.

In attempting to answer here the qustion set for me by the program committee of this conference, I have of necessity avoided any discussion of the esthetics of hunting waterfowl. To many these are fully as important as the size of their bag, and they look with a jaundiced eye on controlled-hunting programs. It is true that such programs importantly break with traditional patterns of hunting that we like to think of as our heritage, but it is manifestly unrealistic to look for unrestricted wildfowling in the face of a rapidly mounting human population.

ACKNOWLEDGMENTS

The current exchange of ideas between waterfowl workers is so rapid and informal as to make acknowledgments difficult in a paper of this kind. I am certainly indebted to A. S. Hawkins of the U.S. Fish and Wildlife Service, for unpublished data and fresh ideas; and to F. C. Bellrose of the Illinois Natural History Survey, whose published papers have made a review like this one possible. C. S. Robbins at the Patuxent Wildlife Research Refuge was extremely helpful in tabulating band recoveries for me. To S. T. Dillon, Alex Dzubin and R. A. McCabe of the University of Wisconsin, I am also indebted for specific advice and criticism.

SUMMARY AND CONCLUSIONS

Waterfowl management in North America is in part an art, based on common sense and experience, and in part a science, based on a systematically derived set of principles. Management of waterfowl by means of refuges, by the curtailment of botulism, by the elimination of poachers and predators, by the creation of new habitat or the development of better habitat is essentially an art to which technology has been making increasing contributions. These components of the program vary in the amount of precise knowledge available to the waterfowl manager. Each originated as an art and is progressing toward refinement as a science.

Attempts to regulate the waterfowl harvest involve somewhat the same evolutionary development. There are sharply different problems as to how we can harvest ducks or geese; and in the degree to which each can be placed on a scientific basis. The biological basis for harvests by hunters appears to rest on the principle that conservative

hunting reduces winter mortality from natural causes or results in increased rates of reproductive gain in the following summer. These principles are not too well documented in upland game bird research, and they will be hard to verify experimentally with waterfowl. At the present time, it appears that conservative hunting can take a fraction equal to about one-half of the annual over-all mortality rate.

Rates of hunting mortality are relatively high in the mallard and black duck, intermediate in the diving ducks and lowest in the teal. About 28 per cent of the adult mallard population in the Mississippi Valley area is estimated to die from hunting in an average year. This appears to be about as much as the population can stand, but under present conditions the hunting-mortality rate will importantly fluctuate from year to year and locally be excessive. The governing variable for a given year is weather which can so influence the migration of some ducks as to double the rate of harvest of one year over another or reduce it 50 per cent even with the same set of regulations. This is true of mallards, probably of pintails, and may or may not apply to relatively consistent migrants like bluebills and coots (*Fulica americana*). Modern bag limits and seasons are in this sense set up on the basis of average conditions. They are relatively crude management tools which are known to be effective in the long run. The dispersed character of duck hunting seems to preclude any immediate possibility of attaining a weekly autumnal index of the success of our hunters and our regulations; for our more erratic migrants, annual changes in season-length and bag limits remind one of a set of conference rules requiring football coaches to name their starting line-ups in August and to stick to these without substitution from October through December.

Possibilities for managing the harvest of geese on a scientific basis are more encouraging. In Mississippi Valley geese, we already know the relation between regional nesting grounds, migration routes, and winter areas. We lack a summer index of annual productivity, and we may have to settle for some moderately inexpensive one determined at September concentration points. To place the goose harvest on a scientific basis, administrators will thus need to enjoy greater flexibility in setting regulations; they will also need authority to designate the total goose kill in each state and to modify their regulations, if necessary, during the course of the hunting season. At the present time, the goose harvest is being regulated on the basis of winter inventories, experience with recent regulations, and with due regard to potential damage to cropland by the geese on their winter range. The conference rules permit the coaches to name their line-ups in

August on the basis of spring practice and the behavior of last year's performers. This is working reasonably well in goose management today, as the recent recovery of Canada geese in the Mississippi Valley testifies. Our present program does not, however, endeavor to harvest a precise percentage of the population.

One's over-all impression in this review is that the federal bureaus are doing a pretty good job in setting regulations. The real promise of scientific management lies, it would seem, not so much in refinement of the harvest as it does in the improvement of waterfowl habitat.

APPENDIX I

The numbers of banded ducks and geese have been routinely catalogued each year by Fish and Wildlife Service personnel since 1923. Table 3 contains the number of birds reported banded from 1923 to June 30, 1946, inclusive. A small error occurs here due to birds which were banded in say 1945-46 and not reported as banded until later. Because they probably were not typical of the behavior of wild-trapped waterfowl, I eliminated from this series 21,762 hand-reared birds and a small number of individuals involved in homing experiments.

The totals for the numbers recovered were obtained by discarding noticeable runs of reports of trapping accidents or shooting reports sent in by a bander. This standard served to eliminate a rather large series of birds banded by E. A. McIlhenny at Avery Island, Louisiana, and reported by him as shot in the vicinity. (McIlhenny sent in 45 per cent of all the reports of banded ring-necked ducks shot in North America.) In addition to these 4,225 records, I also separated out 24,139 reports of birds retrapped by the banders. The number of waterfowl recoveries left in the banding files of the Fish and Wildlife Service from 1923 to June 30, 1946 was then obtained and approximate recovery rates were computed as of the end of this period (Table 3). The result approximates the relative rates at which these species were harvested. A better index would of course involve only the percentage of birds shot in the first hunting season after they were summer-banded.

When the recoveries were further extended to include the five fiscal years ending on June 30, 1951, the mean recovery rate on 16,643 geese (4 species) was raised from 15.1 to 18.6 per cent. This change presumably reflected the active Horseshoe Lake, Illinois, banding of H. C. Hanson near the close of the 1923-46 period. For 17 species of ducks involving 212,507 birds the mean recovery rate similarly changed from 7.5 to 7.8 per cent. I am indebted to Chandler S. Robbins of the U.S. Fish and Wildlife Service for ascertaining these changes.

The rates reported here are subject to certain real or potential errors that force one to regard the percentages as a crude index only. A time barrier prevented me from rechecking the annual Service tabulations for clerical errors in the numbers of birds banded. The summation of recoveries included a number of duplicate recovery reports for all species except the mallard; in a sample of 801 waterfowl cards that I handled in this period, 3.1 per cent were duplicates. These as well as non-hunting records included in the tabulation can be regarded as minor and fairly constant sources of bias between each species. (Many reports of birds "found dead" or "injured" must of course reflect hunting phenomena.) Variability between species is due not only to sampling size, as these figures imply, but also to the season of banding. Recovery rates are presumably lowered when adult birds are banded on their wintering grounds, and increased when juveniles are marked just before they start their first flight south.

The North American banding system is defective in representing the hunting reports of Indians in Canada and persons south of the Mexican-United States boundary. Any index based on banding reports thus holds principally for literate white hunters; fluoroscopic tests of the frequency of birds carrying body shot presumably are necessary to disclose the degree of shooting pressure directed upon waterfowl by non-English-speaking peoples.

It should be obvious that these recovery rates may vary not only regionally but from year to year as well. In general, most of the birds cited here were banded in the United States during the late summer or in the autumn. It should also be obvious that the numbers of banded waterfowl reported here are not necessarily randomized samples of the continental population. The banding work on mallards, pintails and black ducks has been fairly well distributed over those parts of the United States in which these species normally range. For a few species, however, the banding operations have been geographically restricted. Thus, the more important banders of green-winged teal have been in the west (A. J. Butler in British Columbia, Malheur National Refuge in Oregon, and the Bear River Migratory Bird Refuge in Utah). Over one-quarter of the blue-winged teal recoveries reported here come from McIlhenny's banding work in Louisiana. The ring-necked duck and lesser scaup data likewise reflect McIlhenny's trapping success with these species as well as my deliberate elimination of so many of his recovery reports.

I have no explanation to offer for the surprisingly high recovery rate reported here for gadwall. The number of recovery cards for this species was checked twice. The result should, however, be regarded with some reserve. The high recovery rates for some geese could be biased by the greater cooperativeness of hunters in reporting trophy game. This point perhaps can be settled by fluoroscopic comparisons of the frequency of geese and ducks carrying body shot.

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DISCUSSION

VICE-CHAIRMAN ANDERSON: Although this paper is rather scientific in its nature, it has a very practical application. For example, at the Mississippi Flyway Council meeting last Sunday the statement was made by one of the state representatives that we have no evidence at all that we could not safely harvest far more mallards than we do now. I hardly see how that gentleman could make that statement in the light of the data which have been presented by Dr. Hickey.

Are there any other comments or questions of Dr. Hickey on this subject?

DR. H. F. LEWIS [Nova Scotia]: I think it is very encouraging to hear Dr. Hickey say that he sees no hope for the scientific harvest of ducks. [Laughter]

On the other hand I'm afraid I can't agree with him that the art of wildlife management is becoming a science. That appears to be an idea that they hold in Madison. However, I myself differ with it. [Laughter]

All the dictionaries that I have been able to get hold of say that when you apply a science that application is an art. Therefore, if the art were to become a science, it would no longer be applied, and would not be useful.

There is an abundant application for wildlife science, but its application, unless we are to rewrite the English language, must remain an art.

I must say that I think it's encouraging that he sees no hope for the scientific harvest of ducks, because after all I don't think we should forget, nor should we allow these keen, lean greyhounds of science to lead us to forget [laughter], that we are all serving a sport. The harvest of waterfowl and other game birds should be a sport, and it should depend to a very large degree on chance and individual skill.

DR. COTTAM: I should like to ask my friend Joe to what extent these principles that he has applied so splendidly in this paper to waterfowl apply also to upland game?

DR. HICKEY: Dr. Cottam, the principles that I meant are essentially brain children of Paul Errington. They were derived from a series of papers beginning in 1934 and 1935. In giving a presentation of this type I did not mention bibliographical titles, but the principle that hunting is a compensatory form of predation appeared in 1935 for bobwhite quail.

The other principle is found in Errington's paper of 1945. This is sometimes called the inversity principle—that the rate of reproduction gain in a population is inversely proportional to its density in the spring. It just happens that we have just a very few sets of experiments which show that these things actually apply to upland game birds. It's amazing that we don't have more.

VICE-CHAIRMAN ANDERSON: Did I understand you correctly that 28 per cent of the adult mallard population is removed annually by hunting, and that that is in your opinion about all that the population can stand?

DR. HICKEY: I'd like to answer that question this way, that based on what has been found out from some rather preliminary studies of upland game birds, a harvest of 28 per cent of the adult population of mallards is conservative. This does not take into consideration juvenile birds. We are certainly harvesting higher fractions of the whole mallard population than at the rate of 28 per cent, and I'm pretty sure that that is what has taken place in the samples that I studied.

Now, I want to emphasize also that this took place in the early and mid-1940's. What others are finding out now it is up to them to report. That is essentially an historical part in my paper.

DR. COTTAM: I'd like to ask one other question. It seems to me that our aim should be to take all the available harvest that the supply will admit, and I think we all will agree on that fundamental principle.

In the Pacific Flyway if conditions develop to the extent that we don't have carrying capacity for the winter; if, for example, Tule-Klamath Lakes is eliminated—which is the single most important waterfowl habitat on this continent in my opinion—we can harvest up to 75 per cent of the ducks and not hurt them a bit, because nature will eliminate them anyway.

While I'm mentioning that, may I say this, that such rumors are reaching me—and I'm hearing them from many sources. For obvious reasons they are not brought out in the open—they can't be. If the rumors are even half correct, some high powers are selling down the river Tule-Klamath Lake, which is the single most important area on this continent for ducks. If that area is drained and opened for homesteading, I think you can kiss good-bye to 75 per cent of the ducks in the Pacific Flyway, and it won't matter if you shoot them, because nature will eliminate them anyway.

CROP INSURANCE AGAINST WATERFOWL DEPREDATIONS

E. L. PAYNTER

Game Commissioner, Department of Natural Resources, Regina, Saskatchewan

During recent years some species of migratory birds have become a serious menace to the production of crops on the Canadian prairies. This was brought about by several factors:

(1) Agriculture has greatly encroached on wildlife habitat throughout America. Marshes that provided habitat for wildlife in the past were drained in an attempt to increase cultivated acreage. This was not always profitable and in some cases it was necessary to reflood these areas.

(2) We have changed our method of harvesting so that at the present time a very high percentage of grain is swathed before it is combined instead of being cut with a binder and stooked. This makes it much more vlunerable to wildlife depredations.

(3) High water levels in the prairie provinces since 1951 caused water impoundments to increase sharply in area and numbers and this encouraged the ducks to come back to our province and remain in the agricultural areas in large numbers. It is estimated that we have approximately 4,000,000 pothole water areas in the settled portion of Saskatchewan.

(4) As a result of continuous wet weather in 1951, many crops were not harvested. Consequently, the ducks stayed with us until well into the winter. This occurred again last fall; in fact, some of them did not migrate at all. Most of those that did migrate came right back in the spring and made their homes among the unharvested crops where water and feed conditions were ideal.

Each year our Department receives a large number of letters from farmers of which the following is typical:

"Last year I had about 40 acres of barley and wheat almost totally destroyed by ducks—a loss to me of over $1500; the undamaged portion of my crop averaged 40 bushels to the acre.

"Hail is an 'Act of God,' but duck damage can be prevented. If the crows in the district had not been practically all destroyed and a closed season for ducks put in force by the Saskatchewan Government, many of us farmers would not be as poorly off as we are now. I have been informed that Ducks Unlimited was the power behind the drive to increase the duck populations.

"In my seeded acreage this year, I had 28 acres of summerfallow in barley. In mid-August the crop of barley gave promise of a 60 bushel to the acre yield. The ducks have been eating and trampling the barley so much that it is not worth cutting—especially so as I have to pay $12.00 an hour for custom combining.

"On August 23rd, I notified your conservation officer, Bruce, at Tisdale that ducks in numbers were damaging my crop. He came out to my farm and gave me a permit to get shooters in. Despite almost daily shooting, the ducks still came, at dawn and after sunset being the worst time. I telephoned Mr. Bruce again last week and asked him to come out and view the damage; he said he would if he had time.

"I do now make application for compensation from your Department for these losses."

Farmers recommend everything from poisoning ducks to placing a bounty on them and to most of these people all ducks are potential predators. Few realize that only mallards, pintails, and sandhill cranes cause crop losses in our province.

The administration felt that the farmer was standing an undue share of responsibility for losses caused by wildlife and as a result it was decided that we should look into the possibility of establishing a form of crop insurance against destruction by wildlife. In the spring of 1953 a farm-to-farm survey was carried out in three municipalities in heavy crop damage areas. Each farmer was asked the following questions relative to losses during the crop years of 1950, 1951, and 1952: (1) the number of seeded acres of each crop, (2) the number of acres affected by ducks or sandhill cranes, (3) the loss in bushels per acre, and (4) the value per bushel of grain harvested.

We found that (1) the losses as reported were much heavier than had been anticipated; (2) farmers who had not suffered any damage in past years were not interested in any type of crop protection; (3) farmers who had suffered in previous years, in many cases were doing a good deal to keep the birds out of the crops either by straight combining when possible or by putting up scaring devices as soon as the crop was swathed; and (4) many farmers who would insure at a nominal premium rate indicated they would only insure the field that had been damaged in previous years.

Results of the survey were rather startling as shown in Table 1. In these three municipalities alone which average eleven townships each in size, reports indicated that an average of 32,652 acres a year had suffered damage to the amount of $274,632 or $8.41 per acre. There are 34 municipalities of a total of 300 in the province that fall into this class and many others where losses from waterfowl and big

TABLE 1. INFORMATION ON CROP LOSSES FROM WATERFOWL ON FARM-TO-FARM SURVEY TAKEN IN 1953

Year	Total Crop Acreage	Acreage Damaged	Loss to Farmers	Average Yearly Loss
	R.M. 103 (Sutton)			
1950	x	4,778	$ 33,900	
1951	99,267	10,875	$133,600	
1952	x	4,560	$ 42,400	
	Total average acreage damaged—	6,738	Three-year average—	$69,966
	R.M. 251 (Big Arm)			
1950	x	4,923	$ 21,500	
1951	87,330	5,857	$ 58,200	
1952	x	6,035	$ 29,900	
	Total average Acreage damaged—	5,605	Three-year average—	$36,533
	R.M. 290 (Kindersley)			
1950	x	11,661	$ 85,100	
1951	137,904	29,094	$289,500	
1952	x	20,173	$129,800	
	Total average acreage damaged—	20,309	Three-year average—	$168,133
	Total average acreage damaged—3 R.M.'s—	32,652	Total average yearly loss 3 R.M.'s—	$274,632

x Total acreage in crop not available for 1950 and 1952, but will be approximately the same as in 1951.

game are sporadic. There was a total of 23,705,575 acres of crop in the province in 1951. From this you can readily see that crop depredations from wildlife in Saskatchewan are not just a myth.

As a result of information obtained, it was evident that a premium of approximately 10 per cent would have to be charged to farmers in addition to a reasonable increase in the price of hunting licenses in order to provide insurance protection against duck depredations. It was apparent that few farmers would be interested in obtaining protection at such a high rate. There were indications, however, that the average farmer was inclined to exaggerate losses. This information was considered by the administration, which agreed that the premium rate should be set at 5 per cent and the impost on hunting licenses for this purpose be $1.00. The organized sportsmen encouraged this action. Insurance could be taken to the amount of $15.00 per acre and should heavy losses occur before a reserve was built up, the Provincial Government agreed to underwrite the same.

In the summer of 1953 a contract was made with the Saskatchewan Government Insurance Office to handle this type of insurance at a cost basis. Protection covers crop damage by ducks, geese, sandhill cranes, deer, elk, and antelope. Types of crops insured are wheat, oats, barley, flax, rye, speltz, field peas, buckwheat, grasses or clover grown seed or field corn or sunflowers grown for seed whether the same are standing or are cut and lying in swaths or in sheaves either on the ground or in stocks on the land.

It is the responsibility of the company to procure the business and administer the program including making adjustments and paying claims. The minimum premium accepted is $10.00. This minimum premium would give $10.00 protection per acre on 20 acres of crop. All of any one type of crop on a quarter section must be insured. That is, a man could have 120 acres of wheat and 40 acres of barley and, if he wished, he need only insure the barley as it is more vulnerable than the hard spring wheat which is commonly grown in Saskatchewan.

No indemnity is payable unless there is a loss of $10.00 or more; however, if 85 per cent of the crop is destroyed, 100 per cent indemnity is paid. The closing off date for obtaining protection is set at July 31st (Figure 1). As a rule no damage occurs prior to that date and it offers farmers an opportunity to assess the progress of their crop in relation to the potential duck population. Due to the very late harvest last year, however, the term was extended until August 14th. Policies expire on the 30th of November on the year in which they are made. However, once a crop has been threshed or stacked as the case may be, the protection ceases. Most of the other terms of the policy, as may be noted in Figure 2, are very similar to those in hail insurance and are adjusted on the same basis. Qualified hail insurance adjusters are employed by the company to inspect and adjust claims.

It is interesting to note that of indemnities paid over the two years the program has been in operation, two-thirds have been for duck damage and one-third for big game. However, payments for damage caused by migratory birds were much higher than those for big game. Last fall a farmer at Hearts Hill, Saskatchewan who insured 280 acres for $15.00 an acre paying a premium of $210.00 collected an indemnity of $2,482.50. Others received payments of $2,087.50 and $1,125.00 and payments ranged as low as $39.40 on 7 acres that were destroyed by deer. The claims paid for duck depredations amounted to $7,857.00 compared to $1,067.75 for damage by big game.

To date 49 policies have been taken out covering a total risk of $55,860 and claims totalling $11,059.75 have been paid. It is interesting to note that of every $6.00 paid in indemnities, field inspections, and commissions the premium paid by the farmers would cover about

NOTE:

ORIGINAL AND DUPLICATE APPLICATIONS MUST BE SENT TO HEAD OFFICE.
TRIPLICATE REMAINS IN AGENT'S RECORDS.

APPLICATION FOR WILDLIFE INSURANCE TO
THE SASKATCHEWAN GOVERNMENT INSURANCE OFFICE
REGINA, SASKATCHEWAN

No. ______

I ______ of ______ P.O. in the Province of Saskatchewan, hereby make application to the Saskatchewan Government Insurance Office, hereinafter called the Insurer, for insurance upon crops as detailed below and for the amount specified against loss resulting from damage caused by ducks, geese, sandhill cranes, deer, elk and/or antelope (hereinafter called wildlife), from 12 o'clock noon, standard time of the day this application is accepted by the Insurer but not before July 2nd and not later than July 31st, both 12 o'clock noon, standard time, until 12 o'clock noon, standard time, November 30th next or when prior to this date the crop is stacked or thrashed.

APPLICANT MUST INSURE ALL OF ANY ONE KIND OF INSURABLE CROP OR CROPS IN ANY DESCRIBED LEGAL QUARTER SECTION.

NO. OF ACRES	KIND OF CROP	DESCRIPTION OF LAND					AMOUNT OF INSURANCE PER ACRE	TOTAL AMOUNT OF INSURANCE
		PART	SEC.	TWP.	RANGE	MER.		

HEAD OFFICE USE ONLY

PREM. ______
COMM. ______
NET ______
CHECKED

RATE	PREMIUM

SEC.___ TP.___ R.___ W.___
NORTH
WEST
SOUTH

SEC.___ TP.___ R.___ W.___
NORTH
WEST
SOUTH

Each large square represents one section. Each small square ¼ Section.

MINIMUM PREMIUM FOR ONE POLICY IS $10.00

Is the above all the crop on the land ______ (yes or no) If no, location of crops to be insured must be shown on the diagram above.

My interest in the above crop is ______ (owner, tenant or what?) I reside on ______ Sec. ______ Twp. ______ W. ______

Has crop described above been damaged by wildlife or by any other cause(s) prior to the date of this application? Answer ______ (Yes or no)

If the answer is yes, by what? ______ (state cause or causes)

Give approximate acreage and percentage of loss ______ (acres) ______ (percentage)

Mail policy to ______

I hereby declare all statements in this application true, that the Insurer will not be bound by any act or statement made to or by its agents or representatives restricting its rights or waiving its written or printed contract.

Dated this ______ day of ______ 19___

NOTE—THE PREMIUM MUST BE PAID TO THE AGENT ON APPLICATION. ANY INDEMNITIES PAYABLE UNDER THIS INSURANCE WILL BE PAID TO THE APPLICANT NAMED HEREIN.

______ applicant

______ agent

ORIGINAL

800-10M-8-54 11

Figure 1

$1.00 leaving the other $5.00 to come from the fund collected from the sportsmen in license fees. During the first year of operation 19 policies were taken out and 9 claims were paid. Last year 30 farmers applied for protection, 13 of them receiving indemnities. So you can see that the instance of loss among those insuring is very high.

SUMMARY

Field surveys indicate that farmers in Saskatchewan are suffering substantial crop losses from wildlife, chiefly mallard ducks. This has been definitely proven by actual field inspections by qualified adjusters when settling wildlife insurance claims.

There is no doubt that sportsmen are willing to contribute their share towards a system of relief for farmers who are suffering depredations caused by wildlife as there has been practically no resistance

WILDLIFE POLICY

POLICY NUMBER

the SASKATCHEWAN GOVERNMENT INSURANCE OFFICE

HEAD OFFICE - - - REGINA, SASK.

AGENT AT

AMOUNT INSURED	PREMIUM
$	$

INSURED ADDRESS

EFFECTIVE FROM 12 O'CLOCK NOON (STANDARD TIME)

IN CONSIDERATION of the statements contained in the written application attached hereto upon which this policy is issued, and the payment in advance by cash of the premium set out in the said application and subject to the policy conditions set forth overleaf, the Saskatchewan Government Insurance Office does insure the person or persons named therein against loss resulting from damage to crops caused by ducks, geese, sandhill cranes, deer, elk and/or antelope up to a maximum of $15 per acre, and for the term stated in the application. In no case shall any sum be payable in respect of any crop or part thereof which has not been damaged to the extent of at least ten (10) percent of the crop insured.

ATTACH DUPLICATE OF APPLICATION HERE

SPECIMEN COPY

The written application upon which this policy is issued and of which the foregoing is a duplicate, is hereby incorporated in and made a part of this insurance contract.

IN WITNESS WHEREOF, The Insurer has executed these presents but this policy shall not be valid unless countersigned by a duly authorized representative of the Insurer.

DATED AT SASKATCHEWAN, THIS DAY OF

Authorized for the Purpose

H. L. Hammond
Manager

624-2M-5-54

Figure 2

from this group towards the increased charge on hunting licenses for this purpose.

In spite of advertising by posters, radio, and press, farmers are slow to avail themselves of the protection. It is believed that this will change as more of them become familiar with its possibilities.

The insurance reserve now amounts to approximately $200,000 and consideration will be given to reducing the premium rate to farmers next year. Indications are that a type of wildlife crop insurance on a contributory plan with farmers and sportsmen both paying a share, may be an answer to the depredation problem. Possibly a good deal of modification will be required over the years in order to make this program tick, but we are a young province and we are optimistic.

DISCUSSION

VICE-CHAIRMAN ANDERSON: I'd like to ask Ernie if the agricultural colleges have been contacted to see if they might carry on research for varieties of crops which might not require swathing because they would mature earlier; or whether they might carry on research on the question of spraying the wheat so that it will ripen evenly, and will eliminate the necessity for swathing?

MR. PAYNTER: Agriculture is looking into this feature, but I might say that it is hard to go to the farmer and tell him, "Look, you've got to spray your crop again late in the fall so the ducks won't eat it."

Also, it's pretty hard to tell the farmer that his land is there for the good of everybody. He's got the idea that he has title to it, and he thinks that gives him certain rights that don't belong to everyone else.

VICE-CHAIRMAN ANDERSON: Perhaps we should invest a sum of money to make a survey and ask the farmers if they have ever heard what the Good Book says about man being unable to live by bread alone. [Laughter]

PART II

TECHNICAL SESSIONS

TECHNICAL SESSIONS

Monday Afternoon—March 14

Chairman: J. Frederick Bell
Senior Surgeon, U. S. Public Health Service, Rocky Mountain Laboratory, Hamilton, Montana

Discussion Leader: V. W. Lehmann
Wildlife Manager, King Ranch, Kingsville, Texas

DISEASE, NUTRITION, AND CONTROLS

CAUSES OF WINTER LOSSES AMONG CANADA GEESE[1]

C. M. Herman
Patuxent Research Refuge, U. S. Fish and Wildlife Service, Laurel, Maryland

J. H. Steenis
Patuxent Research Refuge, U. S. Fish and Wildlife Service, Laurel, Maryland

and E. E. Wehr
Animal Disease and Parasite Research Branch, U. S. Agricultural Research Service, Beltsville, Maryland

Since at least the turn of the century, Canada geese wintering on the South Atlantic coast have been subject to unexplained mortalities. Such losses have been noted particularly on Pea Island, North Carolina. Brief investigations into the cause or causes of die-off at Pea Island have been made by state or federal personnel on several occasions. As a result, various theories or explanations for the losses have been brought forward—parasitism, food shortage, lead poisoning, severe weather effects, hunting injuries—but none of these has been proven.

Since losses have been noticed most on Pea Island, it may be desirable to indicate the chief purpose of a federal refuge here. The Pea Island National Wildlife Refuge was established in 1938 with the primary objective of protecting the dangerously reduced populations of the greater snow geese. It is continuing to serve this purpose. In addition to flocks of snow geese which winter on the island, the refuge serves as an important haven for Canada geese and other waterfowl. Conspicuous mortality has been evident only among the

Canada geese. Severity of losses has varied from year to year, one of the heaviest die-offs having occurred in the 1931-32 winter. Since establishment of the refuge in 1938, it has been possible to obtain reliable information on the approximate extent of losses. During the 16-year period (1938-1954), the mortality was comparatively light; the heaviest toll appears to have occurred during the winter of 1948-49. In the winter of 1950-51, when there was a peak population of Canada geese estimated at 10,000, 113 dead and 51 sick birds were found. In 1951-52, the losses were still lower (less than one per cent), only 16 dead and 36 sick birds being found in a peak population of 7,000 to 8,000. Still fewer sick or dead birds were observed in 1952-53 when only 21 such specimens were collected from a peak population of 7,000 to 8,000. In 1953-54 there was a slight increase when 81 sick or dead birds were observed (approximately one per cent) among a peak population of 7,500. While it is not possible to predict with accuracy the severity of future losses since the basic cause or causes are not clearly understood, a somewhat regular 7-9 year recurrence of peaks indicates a possible increase in losses within the next few years.

The loss of several hundred geese in the winter of 1948-49 resulted in a field and laboratory study being initiated in 1950 to try to find out the cause of the trouble. Since there has been indication that die-offs may be due to a combination of causes, rather than just one, various angles of the problem have received attention by a team of investigators,[1] from the U. S. Fish and Wildlife Service and the U. S. Agricultural Research Service. This is the first coordinated attack on the problem. Besides parasitologists and veterinarians, the project has the services of a field biologist who has spent most of the past four winter seasons on Pea Island. Particular emphasis is being given to the following:

(1) Vital Statistics—Obtaining information on extent of goose mortality and on weight, age, and sex of representative samples of the local population. Studying relationship of these factors to survival.

(2) Food—Appraisal of local food resources, their nutritive composition, extent of use by geese, possibilities of increasing or improving food supplies, and relation of nutrition to disease.

(3) Movements—Determining extent of local movements of family

[1]The authors acknowledge contributions and assistance from various individuals including L. B. Turner and E. G. Phillips of the Pea Island Refuge; W. G. Cahoon and L. Hovell of Mattamuskeet Refuge; C. W. Wallace and S. Webster, Blackwater Refuge; D. M. Hickok, Bombay Hook Refuge; and, other refuge personnel; S. Powell, Maine Department of Inland Fisheries and Game; personnel of the North Carolina Wildlife Resources Commission; and, M. Farr and J. L. Gardiner, U. S. Department of Agriculture, L. S. Diamond, J. V. Derby, and W. C. Good of the Patuxent Research Refuge.

groups to and from feeding and resting areas both on and off refuge and lines of migration of birds to and from northern breeding grounds. Obtaining this information has necessitated extensive trapping and banding work.

(4) Diseases—Identification and study of the role of parasites.

During the 1950-51 winter, attention was given to kinds, abundance and nutritional quality of goose foods. Widgeongrass, a common water plant growing in the sound on the west side of Pea Island from a half to five miles off shore, provides a substantial part of the winter food. It contains a medium amount of protein (8-10 per cent). Eelgrass and shoalgrass also are locally abundant in this area and contribute additional sources of valuable food. However, at times adverse weather conditions keep the birds away from these favorite feeding grounds and force them to obtain much of their food on the island itself. Most of the available plants growing on the island and utilized by the Canadas as food are extremely low in protein (2-5 per cent). Beach pea is the lone exception; its seeds contain 24 per cent protein on an average. Geese search intensively for this protein-rich food which usually is not available in large quantities. Many natural factors limit the growth and spread of this plant, and its availability as a food therefore tends to fluctuate sharply. The relation of protein level to intensity of parasitism and poor condition of the birds is recognized as a potential factor in the problem. Further studies are in progress on the goose foods at Pea Island and elsewhere, with emphasis on nutritional quality and availability.

By means of a cannon-projected net trap (Dill and Thornsberry, 1950) 1,339 Canada geese were trapped in the past four winters. Age, weight, sex and parasite loads of these birds were recorded. Banding them with leg or neck bands enabled identification of recaptured individuals and provided information about flocking and group movement. It became apparent that at Pea Island, even more than elsewhere, Canada geese maintain distinct flocks and only among certain of these flocks did sick birds occur. Data from banding have led to the conclusion that when the geese leave Pea Island for their northern breeding grounds, many of them proceed along the coast northward beyond Maine. This may have significance with regard to sources of parasitic infection. Another point indicated by studies on movements on the wintering grounds is the probability that the Canada geese of the Pea Island vicinity are inclined to restrict their feeding activities to the outer banks. This is particularly true in severe winters.

Age determinations on trapped geese have shown a low percentage of immature birds: 30 per cent in 1950-51, 22 per cent in 1951-52, 39 per cent in 1952-53, and 48 per cent in 1953-54. Of 250 sick or dead birds examined 90 per cent were immature or birds less than one year old. Practically all sick or dead specimens were seriously underweight. Also the average weight of Canada geese at Pea Island was found appreciably lower than in other Atlantic Coast areas.

The most severe disease damage noted was the result of gizzard worm infections. This infection is wide-spread in Canada geese (Herman and Wehr, 1954). Among 419 Canada geese examined from Pea Island during the past five years, gizzard worms were observed in over 90 per cent of healthy and 100 per cent of sick birds. In sick birds, the number of worms present was much greater than in well birds. Intensity of infection has been lower each succeeding year. The extent of erosion and sloughing off of the gizzard lining was noted to be in proportion to the number of worms present and the physical condition of the bird. In heavy infections, the gizard was almost completely denuded of its lining and, in such individuals, complete loss of normal function of this organ was evident. Such birds would be expected to exhibit effects of malnutrition as well as other symptoms, even though abundant food was available.

The life cycle of the gizzard worm is direct; no intermediate host is involved. Worms mature in the gizzard, and eggs from the female pass onto the ground in the bird's excrement; the time required for development of eggs to the stage that can infect birds is about a week. Gizzard worm eggs and larvae require certain conditions of moisture and temperature for optimum survival. They readily succumb to adverse conditions. The sandy soil, high salinity, and low winter temperatures at Pea Island are not ideal for these parasites to achieve their infective stage except perhaps in some few locations for limited periods. It has been demonstrated (Wehr and Herman, 1954) that geese become infected with gizzard worms during the first few weeks after hatching. Initial infections in young birds are likely to be much more harmful than later ones. This raises the question whether low percentage of young at Pea Island some years may be a result of losses among goslings on the breeding grounds.

In addition to gizzard worms, coccidia were of frequent occurrence in birds examined. These were found in the intestines and kidneys. The kidney form has been suggested as a factor in the losses (Critcher, 1950). However, in our studies, only 50 per cent of the geese examined were infected with the kidney form, and severe damage was evident only in the relatively few cases of heavy infections. These coccidia also have a direct life cycle with specific requirements for development to infective stages.

Of 29 species of internal parasites collected from Canada geese at Pea Island, gizzard worms and coccidia were much more prevalent than the others. Little is known of these less prevalent parasites. They are not so commonly associated with sickness and mortality, as gizzard worms and coccidia. Major emphasis has therefore been placed on the latter parasites. Experiments are now in progress to determine the natural requirements for transmission of gizzard worms and coccidia, their specific effect on geese, and whether transmission occurs at Pea Island.

It has been determined that, in general, as parasites increase in number within an individual goose, the bird loses weight and condition. Also, as a bird loses weight and condition, the parasites are able to develop with less resistance from the host. It is evident that loss of weight has an important bearing on the problem, especially since it has been observed that most of the parasites which occur in Pea Island geese are also present in Canada geese elsewhere, though in lesser numbers and without causing serious winter losses. Weight of birds that were considered well when collected averaged 3 to 5 pounds heavier than sick specimens. Some sick geese weighed only 3 pounds whereas healthy birds averaged about 7 pounds.

Other workers have shown that animals on low-protein diet are more susceptible to ravages of parasitic infection. Assuming that this might be important in the losses at Pea Island, an experiment was set up involving 100 penned wild Canada geese at Pea Island. Half were fed on a comparatively low-protein diet (7 per cent) and half on a higher-protein diet (14 per cent). Both groups gained weight. However, gizzard worm infections in these birds were too light to be significant. Domestic geese maintained at the laboratory on a diet of only 4 per cent protein also showed weight gains and little or no increase in the few gizzard worms present from experimental inoculations. All geese on the lower protein diets exhibited loss of muscle tissue and increase of internal fat.

Though final conclusion on the cause of goose die-offs at Pea Island has not been reached, it seems evident now that gizzard worms and nutritional factors are important elements in the problem.

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THE ROLE OF FOOD AND COVER IN POPULATION FLUCTUATIONS OF THE BROWN LEMMING AT POINT BARROW, ALASKA[1]

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In range ecology it is unusual to find a single species of herbivore as virtually the sole consumer of all vegetation. The writer enjoyed the good fortune of studying such a situation in the relative simplicity of the tundra community at Point Barrow, Alaska, where the brown lemming (*Lemmus trimucronatus*) clipped more than 99 per cent of all standing forage in attaining a recent population peak.

Lemmings are best known for the great emigrations which have been reported in considerable detail from Norway. These emigrations are associated with periodic fluctuations in numbers which tend to reach peaks every three to four years. While emigrations are not usual at Barrow, the three- to four-year population fluctuation seems well marked and was reported by Rausch (1950) who observed the decline of a peak population in 1949 and noted that a previous peak was reached in 1946.

Lemmings were in extremely low numbers when this study began in 1950; they passed into a phase of population proliferation in 1951 which caused a 200-fold increase by June of 1952. An additional two-fold increase was attained by June of 1953 at the peak of the current population cycle. By August of 1953, a phase of population decline was under way which returned the lemmings to low densities by the following year.

The relation of lemmings to their forage has been discussed in some detail in the literature. Elton (1942) has discussed the Lotka-Volterra oscillation in relation to rodent population fluctuations. More recently, Lack (1954) has refined these ideas and specifically postulated that the basic cause of rodent cycles is the dominant rodent interacting with its vegetable food to produce a predator-prey oscillation. A test of this hypothesis was included in the present study in which forage production and utilization were followed through a full cycle of population fluctuation.

TUNDRA HABITATS

To the casual observer, the coastal plain near Point Barrow is a seemingly endless tract of flat terrain interspersed with an amazing

[1]This study was supported by grants from the Office of Naval Research, the Arctic Institute of North America and the Edward K. Love Foundation.

number of lakes. The vegetational aspect is much the same as on the short grass plains; a dull brown expanse of grasses and sedges stretches out from the observer to the horizon in all directions.

While the first impression of the coastal plain tundra is one of monotonous sameness, closer study reveals some distinct habitat differences. Three types of habitat can be described: (a) beach ridge, (b) marsh, and (c) polygonal earth. Of these, the beach ridge is a relatively unimportant component of brown lemming range and can be dropped from further consideration in this paper. The marsh habitat occupies roughly one third of the land surface and forms a major component of the brown lemming's habitat. As winter range, the marshes are excellent; however, these areas frequently flood as the snow cover retreats. The polygonal earth habitat occupies the remaining land area. This is optimum year-round habitat for brown lemmings and is the type of habitat in which the present forage studies were centered.

Viewed from a low-flying plane, polygonal earth resembles a network of interconnecting cells of a fairly constant diameter (20-25 feet) over any given piece of terrain. Figure 1 is a cross-section of two common types of polygonal cells.

The point of interest in the present paper is that the brown lemming has distinct and rather inflexible microhabitat preferences with relation to the surface features of polygonal earth. During the snow-free season, lemmings in polygonal terrain rarely venture out from the protection of the troughs. As the snow level builds in winter, however, the lemmings are able to work upward onto the ridges and raised centers of the polygons under the protection of the snow mantle. For this reason the polygonal troughs are termed primary habitat in the present paper. They afford the lemming protection from wind and and to a lesser extent from avian predators. They also combine a lush growth of forage with the proximity of good burrow sites in the sides of polygonal ridges.

As indicated in Figure 1, the ridges and centers of the polygons make up the major portion of the polygonal unit. Lichens, mosses, herbs, grasses and sedges able to tolerate dry conditions typically grow on the tops of raised-center polygons. In low-center polygons, the same xerophytic species occur on the ridges, but aquatic or semi-aquatic plants grow in the shallow pools which often occupy the saucer-like depressed centers. (Wiggins, 1951)

In either type of polygon, the ridges and centers are considered secondary habitat since they support relatively inferior stands of

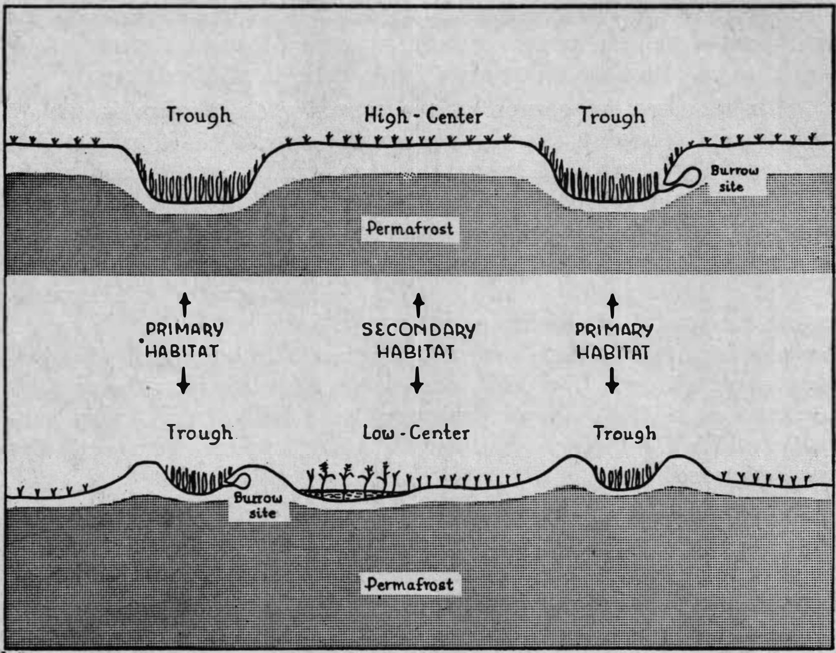

Figure 1. Profiles of High and Low Center Polygons.

forage which are available to the lemmings only under the protection of snow.

FORAGE PRODUCTION

To measure forage production and the effects of lemming grazing on the tundra vegetation, 20 exclosures were constructed in 1950. The exclosures were placed in polygonal troughs and other low wet sites where maximum utilization could be expected as indicated by old runways, cuttings, and winter nests remaining from the previous population peak. The rectangular exclosures, one square meter in area, were covered on top and sides with one-half inch mesh hardware cloth. Iron rods were driven into permafrost as corner stakes, and the wire sides were set down 4 to 6 inches beneath the soil surface, making a rodent-proof exclosure. The size, materials and fabrication of these exclosures proved to be satisfactory over the four-year period studied. One exclosure was accidentally destroyed during the winter night in 1952 by a crawler tractor; the rest remained intact.

Each exclosure was matched as closely as possible with an adjacent open plot of the same dimensions and vegetative composition. The exclosed and open plots were divided into one-quarter square meter quadrants. Annual forage production was determined by clipping the standing vegetation on corresponding one-quarter square meter quadrants at the end of each growing season.

Figure 2 summarizes forage production on exclosed and open plots from 1950 to 1954. Annual population peaks of lemmings are superimposed on the graph. Exigencies in the field program prevented measurement by clipping of the yield in 1951; however, all plots were inspected during the growing season and the height of growth was carefully measured at the end of the summer. From these data and observations, an estimate of the 1951 yield was obtained.

The inverse relationship between lemming numbers and the production of their staple food is at once apparent. The peak forage production year in the current cycle was 1951, two years removed from

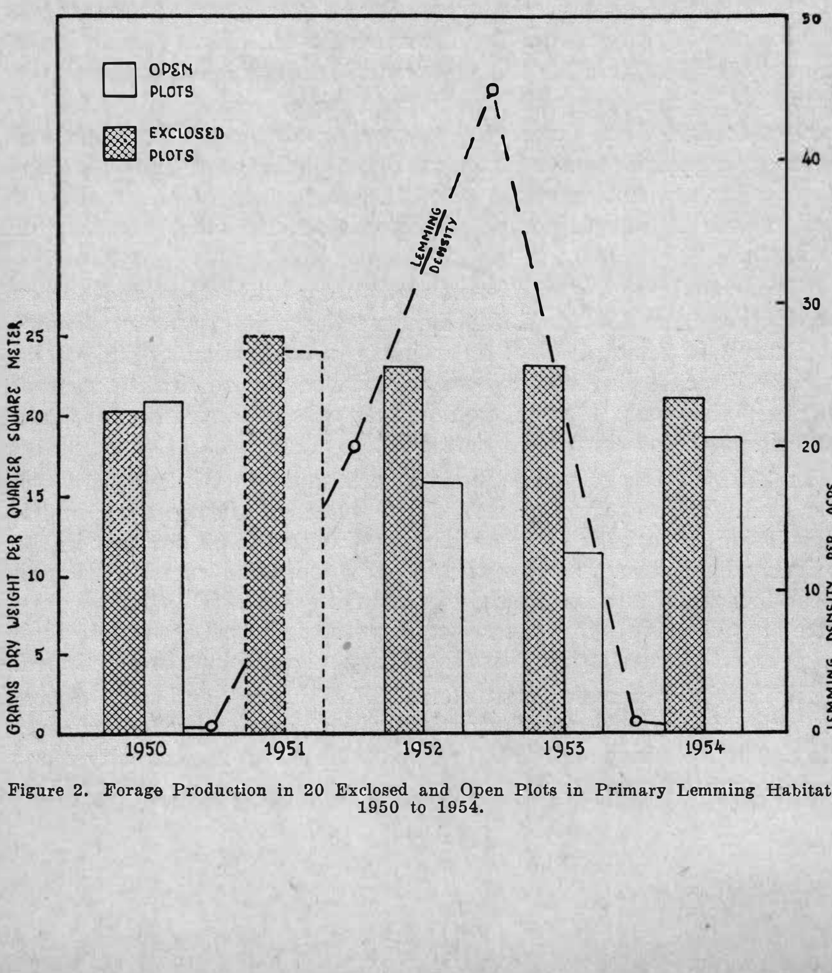

Figure 2. Forage Production in 20 Exclosed and Open Plots in Primary Lemming Habitat, 1950 to 1954.

the previous population high reported by Rausch (1950). A comparison of the exclosed and open plots in 1954 shows that the vegetation had not yet completely recovered from the heavy utilization of 1952 and 1953. From this comparison, complete recovery of the vegetation can be anticipated by 1955 if utilization by lemmings remains low this winter.

INTERACTIONS OF LEMMINGS AND FORAGE

Brown lemmings feed in a manner similar to common field voles (*Microtus*). Blades and stems of grasses and sedges are staple food. In summer when the plants are green and succulent, the lemmings eat almost the entire plant with the exception of the inflorescence and seeds. By far the greatest impact on the vegetation occurs during the fall, winter, and spring when the forage is dormant. Lemmings work along the surface of the frozen tundra under the protective surface of the snow and eat the basal portion of the grasses and sedges, leaving the dry tops as scattered surface litter. Plants thus clipped are easy to identify after the snow mantle melts in June; this is the best time of the year to obtain a measure of winter utilization of tundra forage. Each June the presence or absence of cuttings was noted on a quarter square meter quadrant of the open plots. If clippings were present, an estimate of the percentage of stems clipped was recorded. Sample counts were made to check the accuracy of estimate.

Figure 3 shows the relationship of forage utilization and forage production from 1950 to 1954. Because the writer was not present at Barrow in June of 1954, an estimate of utilization as judged by surface litter in August has been substituted for quadrant measurements in this year. Annual population peaks of lemmings are again superimposed for convenient reference.

In 1950-51, forage utilization was estimated to be less than one per cent. The next winter marked the onset of the upswing in the population and winter forage utilization jumped to 69 per cent in the primary habitat sampled by the forage production plots. Although local forage exhaustion caused the lemmings to shift grazing pressure onto the ridges and the raised centers of the polygons, less than 20 per cent of the stems in these secondary microhabitats were clipped.

In the winter of peak populations (1952-53), grass and sedge forage on both primary and secondary microhabitats was exhausted by early May. The lemmings shifted to less palatable forage, principally moss, and so averted starvation.

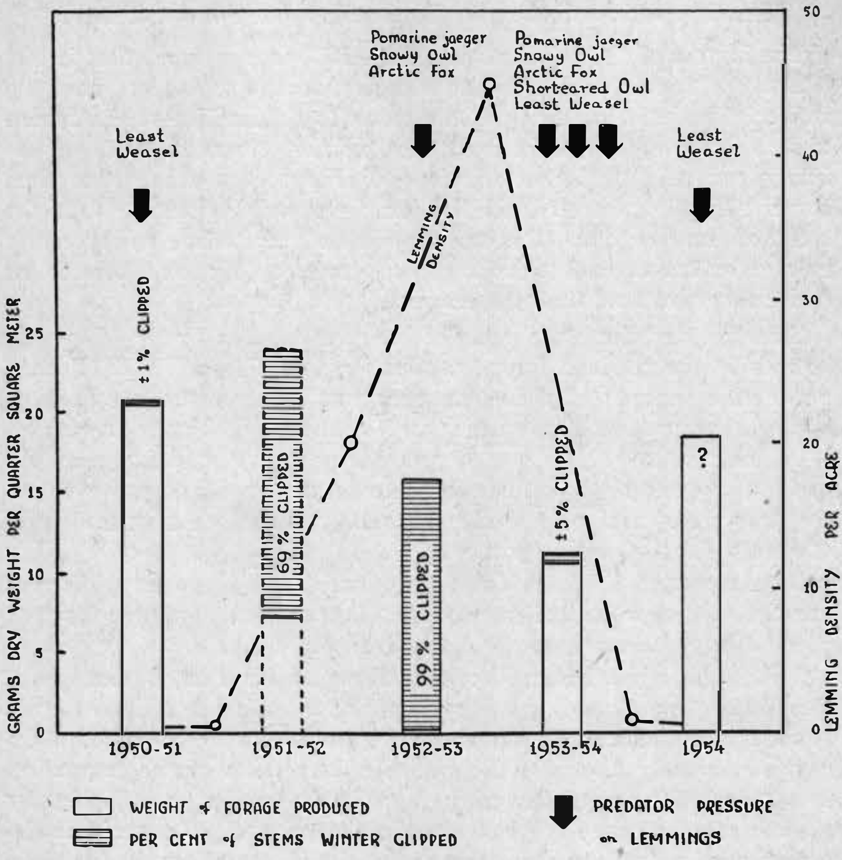

Figure 3. Forage Production and Forage Utilization in Primary Lemming Habitat, 1950 to 1954.

The principal interactions of lemming population dynamics and the vegetation can be summarized as: (a) The lemming population was prompt in responding to forage recovery; (b) The greatest rate of population increase (200-fold) occurred over the winter of 1951-52 and made heavy inroads of the forage resource in primary habitat; (c) The subsequent twofold increase of lemmings to the population peak over the winter of 1952-53 necessitated an extension of foraging activities into secondary habitat and climaxed in the complete exhaustion of standing forage; and (d) Forage production was depressed by one-half in the year of peak population; a phase of population decline followed.

THE EFFECTS OF LEMMING UTILIZATION ON FORAGE THRIFT

The impact of the population peak in 1953 was so great that it was difficult to find a single stem of standing forage which survived the winter. The lemmings clipped the stems to the surface of the frozen tundra but could not effectively dig out the crowns or roots of the hardy perennials. Serious damage to the forage stand was thus averted by the protected position of the growing points. With the onset of summer thaw, the crowns became vulnerable; however, the new green leaves and stems began to grow rapidly, and became an easier source of food than the crowns.

The effects of these two years of heavy utilization by lemmings can be seen in the reduced forage production in 1952 and 1953. Forage production in each of these summers was actually the net gain of total forage produced minus summer consumption by the lemmings. Whether forage growth was depressed by heavy utilization in the previous winter or by utilization during the growing season (both factors were operative), the result is nearly the same; a reduced level of forage for the coming winter.

By August of 1953 the forage production on the open plots was reduced to 50 per cent of the exclosed plots; it was apparent that the population peak could not be maintained on this reduced growth.

With such a pronounced cycle in forage utilization, it can be inferred that lemmings play a very important role in the nitrogen cycle in the tundra habitat. In these cold regions the rate of vegetation decay is extremely slow and the periodic conversion of the vegetation into lemming flesh and droppings is probably requisite to the healthy growth of the forage components. In like manner, the periodic destruction of vegetative cover must have significant effects in the thermal regime of the tundra.

THE EFFECTS OF FORAGE DEPLETION ON THE LEMMINGS

While the lemmings would have been faced with starvation if they had continued into the winter of 1954 in a phase of population proliferation, food shortage *per se* was not a controlling factor in reducing this peak population. A reduced reproductive rate was already evident in the summer of 1953 and this trend continued as a failure in reproductive gain over the winter of 1953-54 (Thompson, ms.). Scholander *et al.* (1950) have shown that the fur of the brown lemming does not provide good insulation against arctic temperatures and hence must be compensated for by increased metabolic activity at temperatures below 10 degrees C. (50 degrees F.). Since ground temperatures fall well below 0 degrees F. in the lemming's winter habitat, it is obvious that they must expend a considerable part of their metabolic energies in heat production during the winter. In a winter of abundant forage, the lemming is apparently able to meet the demands of heat production and also show a high reproductive gain. But, this becomes impossible on a depleted range when the lemming must extend his efforts to meet his daily food requirements.

DISCUSSION

The curtailment of reproductive success would not alone account for the spectacular plunge of the population from 1953 to 1954. A host of predators was primarily responsible for the mortality indicated in the steep downward slope of the curve shown in Figure 3. Pomarine jaegers (*Stercorarius pomarinus*) were the principal predators in the summer of 1952; snowy owls were also present. Arctic fox (*Alopex lagopus*) appeared during the fall and early winter of this year and further reduced lemming numbers until the accumulation of snow offered protection to the lemmings. The short-eared owl (*Asio flammeus*) appeared as an additional avian predator in the summer of 1953 while densities of pomarine jaegers increased more than four fold (Pitelka, *et al.*, 1955). By May of 1953, least weasels (*Mustela rixosa*) were evident again for the first time since 1950. It is significant that the weasel is the only predator at Barrow whose prey in all seasons is almost exclusively lemmings. While avian predators and the Arctic fox are responsible for the sharp initial decrease in lemmings in summer and fall, our field evidence strongly suggests that it is the continued pressure by the weasels through the winter which eventually reduced the lemmings to the extremely low numbers of 1950 and 1954. As lemmings declined in abundance, the owls, jaegers, and foxes emigrated and shifted to other food, but the weasels' only alternative was to extend their efforts in pursuing the remaining lemmings.

While this study did not attempt to measure the health of the lemmings in detail, our observations indicated that neither parasites nor disease played a major role in the population decline in 1953. Furthermore, the emigration which occurred in June of 1953 was of minor importance in relieving population pressure (Thompson, in press).

Thus in surveying the population mechanisms operative at Barrow in the recent lemming cycle, the interactions of food, cover and predators were considered to be the most important. With minor qualifications, this seems to fulfill Lack's postulations as to the cause of population fluctuations in rodents. It is not to be inferred, however, that all lemming cycles at Barrow follow the pattern of the recent one. A great number of forces interact to cause the rise and fall of the lemming population; in other cycles at Barrow these forces might occur in a different combination or alignment.

SUMMARY

In the recent cycle of lemming abundance at Barrow, Alaska, the upswing of population in 1951-52 coincided with the recovery of the forage from heavy utilization in the previous population peak.

Forage utilization fluctuated from one per cent in the years of low population to nearly 100 per cent at the population peak.

Forage production was depressed by one-half following two years of heavy utilization by lemmings.

With an increase in predators and a decrease in forage production, the lemmings failed to make a reproductive gain in the winter following the population peak and passed into a phase of population decline.

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DISCUSSION

CHAIRMAN BELL: Thank you, Mr. Thompson. That is a good job done in an ideal site.

As for the factors involved in these population changes, I don't think we should be too afraid to try to draw some conclusions from the lemming. Until we work out some of the basic principles in these situations which seem to be very remote, where you have the elements working against you, I don't think we'll ever get our feet on the ground.

Are there any questions?

DR. SHELDON [Massachusetts Cooperative Unit]: Mr. Thompson, I wonder if you would comment a little more fully on the sampling technique you used to measure the population of the lemmings, and particularly of the weasel.

MR. THOMPSON: I wish I could elaborate in great detail. It's been very difficult. We used snap traps. They are valuable for comparisons between species but are not particularly helpful for population densities.

We finally came to using King's grouse flushing method, except we used a cargo carrier; run over the tundra, it shakes the ground, and flushes the lemmings.

As soon as they were flushed, a man would chase madly after them with a stick, and if the lemming went down a hole, we dug it out. In other words, we tried to collect every animal we saw.

You can go in any direction for a while, unless you happen to hit a lake, and of course we took precautions for avoiding that. We made a report of our miles and our flushing distance, and got an idea of the lemmings flushed per area.

At the very peak you can do a strip census. Just walk along and delineate a plot with a stick, and just call out the lemmings that you see as you walk along it. You'll get some idea there of the minimum density, but the error in both of those methods, incidentally, is that we don't know for sure that we're flushing all the animals out of their burrows, and there are some indications that females with litters will tend to remain in their burrows, rather than flush, because you get a different census from flushing than you do by snap trapping.

DR. JOHN L. BUCKLEY [Alaska]: Was there any difference in reproductive rates between years in these different stages in the cycle?

MR. THOMPSON: Yes, there were, John. The problem, of course, in the low population is to get a sample. That's always the critical thing, we didn't get enough females to determine litter size, but from the tests in the low in 1951 and in 1954, the weight was depressed, compared to the males in 1953 at the peak. The number of young per litter declined in 1953.

DR. JOHN CRAIGHEAD [Montana Cooperative Wildlife Research Unit]: How do you measure the population pressure, and also measure the mortality on the lemmings from their predators?

MR. THOMPSON: That's a very good question. We marked a good number of nests and we located all of the jaeger territories in the area, studied and plotted them, and from that got the population density of the jaegers.

They averaged about four to five pairs per square mile in 1952. In 1953, at the peak, the jaegers built up to about 18 pairs per square mile. The size of their territory was about 40 acres.

As to mortality in 1953, we made a series of line transects of three different habitats, ten plots each. We cruised down these with two men, one checking the other, and picked up all the dead lemmings that we saw on the surface.

This was about two weeks after the snow breakup, when the population was starting to crash. We picked up an average of 11 to 12 dead lemmings per acre. The average population density was around 50 to begin with, so we had a tremendous mortality within a period of two weeks. Of these lemmings, 4 out of 5 at autopsy, showed crushed skulls or hemorrhages or torn skin or teeth marks, indicating that they were the victims of predators which were left on the surface—which surprised me very much, because I didn't think that the predators were killing undue numbers of lemmings and leaving them on the tundra, but apparently that was the case.

Of the 50 animals, we had 20 per cent mortality from sources which we couldn't identify.

DR. CRAIGHEAD: I gather that most of this mortality which occurred was during the summer months. Was there appreciable mortality in the winter months?

MR. THOMPSON: That I can't answer, except that mortality, as it is indicated in snap trapping is helpful. There was a decrease from August 1953 to June of 1954, and that, of course, means that there was no reproduction that winter, and also the reduction of level would give you some idea of winter mortality.

We picked all the lemming nests off of a given area of around seven acres, and that gave us some idea of the density of the winter nests, and of these nests, we examined them carefully and found that 15 per cent of them in 1950 showed evidence of weasel predation.

I was going to use this technique in 1954 to get some idea of this, but apparently the lemmings did not build any winter nests over that period.

DR. PETRIDES [Michigan State]: I wonder if you would say a few words on the Arctic fox and its relation to lemmings.

MR. THOMPSON: The Arctic fox is an enigma in many ways. He's a predator upon the lemming. The litter dens are back inland where we get some sandy ridges, where they can have satisfactory burrow sites. They do not have dens along the coast, because apparently there just isn't enough depth of drainage.

We didn't have one site within five miles of the Arctic research laboratory, but flying over the country back 50 or 100 miles inland it became very apparent to me in 1954 that you would see all these dens of foxes, and that's their breeding area; in the fall they move out to the coast. They prey on lemmings until the snow builds up, and after the snow reaches about 6 inches they are no longer effective predators; then they move out on the ice and become scavengers on the seals, or whatever they can find.

I say they are enigmatic, because they appear and disappear. I saw foxes at sundown, and they came right up to within about 10 feet, and squeaking a little, I made them come closer and closer. We hadn't seen any all summer, but this was about September, and clearly it was their autumnal migration.

DR. PETRIDES: Did you get any observations on fox cycles and lemming cycles from your observations there?

MR. THOMPSON: Yes, I tried to get some material from fur returns along the coast there but I can't pass on that. There is some indication that there is a causal relation there, but it's not quite clear.

In Greeland it has been shown that there are lemming foxes; and coastal populations which show no relation to lemmings. I suspect that there is some relation there with the foxes at Barrow.

DR. DUNBAR [McGill University]: I want to ask Dr. Thompson whether he has any ideas on what determines the periods of oscillation; if the plant food supply is so important, then what about the 9- or 10-year cycle in rabbits, where you have a food supply which presumably recovers more quickly than the Arctic vegetation?

MR. THOMPSON: That's a very interesting question. We have a few ideas here, but every time we answer one we raise two more questions, and I just don't know.

I should think with the lemming the rather rapid recovery with vegetation would be one of the factors that would affect the short period. However, it has been observed that the shortness of the cycle is an expression of the reproductive rate, and the higher the reproductive rate, the shorter the cycle, and the quicker the animals eat themselves out of house and home; the longer the reproductive rate, the longer the cycle. That holds fairly well.

REDUCTION OF ADRENAL WEIGHT IN RODENTS BY REDUCING POPULATION SIZE[1]

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INTRODUCTION

Increased mortality, decreased reproduction, and stunting of growth frequently have been associated with high populations of various mammals (O'Roke and Hamerstrom, 1948; Cheatum and Severinghaus, 1950; Latham, 1950; Rowan, 1950; Errington, 1954). These phenomena are compatible with the known responses to increased stress in a population (Christian, 1955b). The term stress is herein defined as the acting sum of those stimuli which tend to produce alterations in the homeostasis of an animal. It is generally believed that the adrenal cortical hormones are primarily involved in counteracting the effects of these stimuli, and that changes in the amount of adrenocortical tissue reflect changes in the amounts of cortical hormones produced. Consequently the amount of cortical tissue, as measured by adrenal weight, is an indirect measure of stress. The following experiments were done to determine the adrenal response in terms of weight to reducing populations to 50 per cent of the maximum size and maintaining them there on a sustained yield basis.

A direct relationship between population density and adrenal weight was demonstrated in laboratory populations of house mice (Christian, 1955a, b, c) for which food and water were provided in excess. The changes in adrenal weight were produced primarily by changes in the amount of cortical tissue, especially of the zona fasciculata. Reproductive function and growth were suppressed in proportion to increases in population size, while infant mortality increased at the same time. In rural and urban populations of Norway rats, adrenal weight was found to be related to population density (Christian, 1954), and there was a corresponding increase in adrenal weight when submaximal populations of rats were allowed to increase. Conversely, a reduction in adrenal weight attended a decrease in population size. However, these reductions were not main-

[1]The opinions and assertions contained herein are the private ones of the authors and are not to be construed as official or reflecting the views of the Navy Department or the naval service at large.

tained and the populations were allowed to increase immediately following reduction; so that the effect on the adrenal gland of maintaining populations at reduced levels was not determined. Preliminary studies indicate that the above relationships are true for other species of wild mammals.

These facts become important in game management if they suggest a means of reducing stress in a population. Maintaining a population at a reduced level should result in decreased stress with a consequent improvement in the general condition of the population.

MATERIAL AND METHODS

This study was conceived with the idea of comparing unhunted with consistently hunted populations of woodchucks in the belief that high unhunted populations would reflect a greater degree of stress than populations kept below their maximum levels by regular harvesting. Inadequate data compelled us to shift to a study of Norway rat (*Rattus norvegicus*) populations for which data could be collected more adequately. However, there is considerable information to indicate that the basic physiological responses to adverse circumstances are similar in various mammalian species (Selye, 1950; Christian and Ratcliffe, 1952).

The rat populations in three Baltimore City blocks, A, B and C, were chosen for study. These populations had been followed by repeated censuses for at least two years, during which time they had increased constantly and were at or near peak levels. The censuses were made by D. E. Davis using the method of Emlen, Stokes, and Davis (1949). This method has been found satisfactory during a decade of continuous use, and can detect differences of about 20 per cent in rat populations. These three populations were low in early 1953 and increased steadily until January 1954, when they ceased growing. Samples of six mature rats of one sex per block were collected in January and again in February 1954 (See Table 1).

Late in 1953, maximum (asymptotic) population values were estimated on the basis of available data for each of the three block populations with the following results: block A, 200 rats; block B, 140 rats; and block C, 115 rats. Estimates of the maximum populations were made by fitting logistic curves to the census data and determining the upper asymptotes of these curves. The details will be presented at another time. Trapping was begun to reduce each block population to one half of its estimated maximum value beginning the week after making a census in March. Seventy-three rats were removed from block A, 48 from B, and 57 from C. These rats

TABLE 1. SUMMARY OF DATA COLLECTED FOR BLOCK POPULATIONS OF RATS. ADRENAL VALUES ARE THE MEANS AND STANDARD ERRORS FOR EACH SEX OF EACH SAMPLE. SEE TEXT FOR EXPLANATION OF ADRENAL VALUE USED.

			Mean difference between logarithm adrenal weight of sample and reference values							
			Male Rats				Female Rats			
Block	Date	Estimated population	No.	Mean difference	Standard error	Percentage weight change from reference value	No.	Mean difference	Standard error	Percentage weight change from reference value
	May 1953	103								
	July 1953	110								
	Sept. 1953	118								
	Nov. 1953	133								
	Jan. 1954	150	6	—0.047	0.045	—10.1				
	Feb. 1954	167	6	—0.035	0.037	— 7.7				
	Mar. 1954	152	25	—0.002	0.019	— 0.5	35	—0.008	0.017	— 1.8
A	Apr. 1954	100	0							
	May 1954	120	6	—0.132	0.032	—26.2	6	—0.149	0.029	—29.0
	June 1954	120	5	—0.101	0.051	—20.8	5	—0.177	0.038	—33.5
	July 1954	125	5	—0.203	0.024	—37.4	7	—0.191	0.036	—35.5
	Aug. 1954	98	0							
	Sept. 1954	120	5	—0.062	0.045	—13.3	8	—0.104	0.048	—21.3
	Oct. 1954	130	5	—0.058	0.035	—12.5	3	—0.097	0.073	—20.0
	May 1953	62								
	Sept. 1953	88								
	Nov. 1953	115								
	Jan. 1954	140	6	—0.053	0.030	—11.1				
	Feb. 1954	130	6	—0.002	0.035	— 0.5				
	Mar. 1954	130	26	—0.055	0.020	—11.8	15	—0.073	0.022	—15.5
	Apr. 1954	70	3	—0.171	0.050	—32.5	1	—0.280		—47.5
B	May 1954	105	12	—0.155	0.038	—30.0	11	—0.189	0.017	—35.3
	June 1954	90	7	—0.098	0.031	—20.2	6	—0.168	0.031	—32.0
	July 1954	85	5	—0.083	0.040	—17.4	4	—0.249	0.019	—43.6
	Aug. 1954	100	7	—0.152	0.026	—29.5	6	—0.161	0.032	—31.0
	Sept. 1954	90	7	—0.139	0.030	—27.4	11	—0.103	0.018	—21.1
	Oct. 1954	66	1	—0.155		—30.0	4	—0.280	0.059	—47.5
	May 1953	65								
	June 1953	86								
	Aug. 1953	100								
	Oct. 1953	105								
	Dec. 1953	120								
	Jan. 1954	135					6	+0.010	0.037	+ 2.3
	Feb. 1954	130					6	—0.075	0.031	—15.9
C	Mar. 1954	130	23	—0.003	0.025	— 0.6	18	—0.049	0.021	—10.6
	Apr. 1954	80	9	—0.137	0.032	—27.0	5	—0.179	0.030	—33.8
	May 1954	100	17	—0.110	0.027	—22.4	12	—0.265	0.032	—45.6
	June 1954	80	7	—0.111	0.046	—22.5	5	—0.173	0.024	—32.9
	July 1954	75	4	—0.173	0.067	—33.8	4	—0.254	0.025	—44.3
	Aug. 1954	80	3	—0.099	0.035	—20.4	9	—0.082	0.039	—17.2
	Sept. 1954	80	5	—0.034	0.060	— 7.5	5	—0.089	0.028	—18.5
	Oct. 1954	75	1	—0.011		— 2.5	4	—0.171	0.014	—32.5

constituted the final pre-reduction high population samples for each block. Natural mortality and trap robbery by children and dogs accounted for additional reductions of unknown numbers.

Monthly censuses were made of the rat populations beginning two weeks after the reduction. One week after each census enough rats were removed by trapping to maintain the populations at the reduced levels. The age and sex composition of the monthly samples were kept comparable to those of the reduction trapping by removing only rats of the desired size and sex and releasing all others. Practical difficulties in trapping the desired sample were considerable; so that sample size and composition each month only approximated the ideal at best, and was frequently quite far removed. The monthly censuses and the number of male and female rats removed each month from each block are given in Table 1.

The length of the head and body to the nearest centimeter, body weight to the nearest 10 grams, and the weight to the nearest 0.1

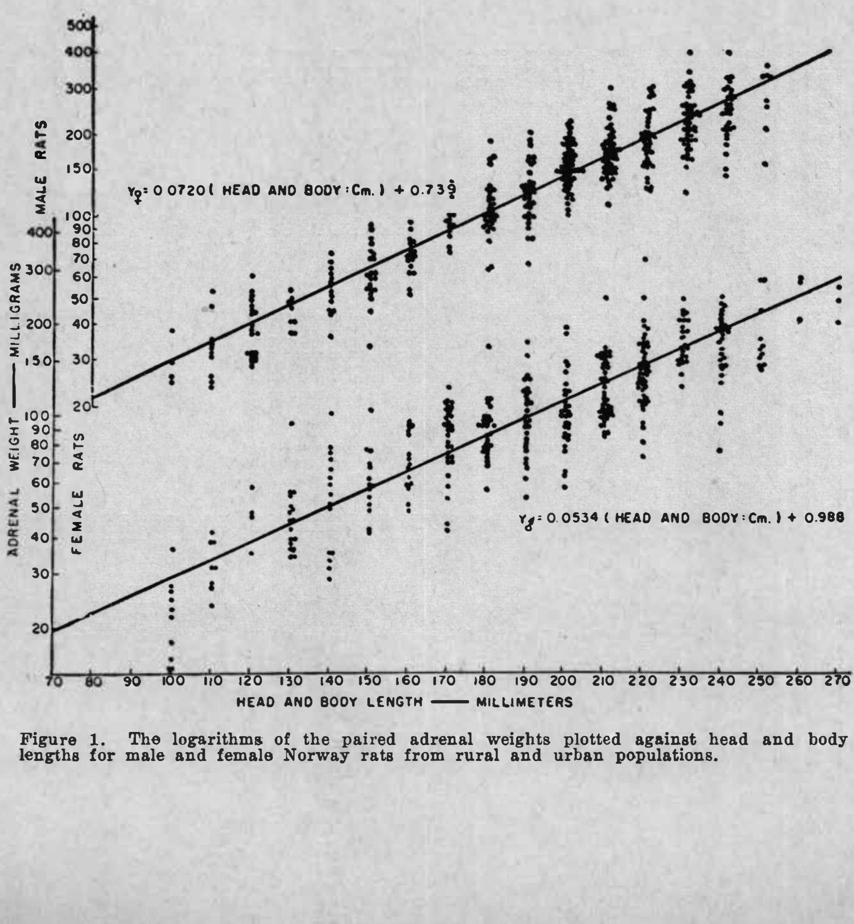

Figure 1. The logarithms of the paired adrenal weights plotted against head and body lengths for male and female Norway rats from rural and urban populations.

milligrams of the paired adrenal glands were obtained for each rat. It was determined previously that the logarithm of the adrenal weight in milligrams plotted against the length of the head and body in centimeters gave a better linear relation than any other standard measurement, such as body weight or the logarithm of the body weight. Regressions were determined for the logarithms of the adrenal weight against the head and body length using a large number of urban and rural rats of each sex from a variety of population densities, thereby eliminating the effects of population size (Christian, 1954; fig. 2). The equations for these regressions are:

female rats: log adrenal weight (milligrams) = 0.739 + 0.0720 (head & body length in centimeters).

male rats: log adrenal weight (milligrams) = 0.988 + 0.0534 (head & body length in centimeters).

The logarithm of the adrenal weight of each rat was compared with that of the reference population at the appropriate head and body length, and a mean difference and standard error were also calculated for each sex for each sample trapped (table 1). The mean per cent difference from the reference adrenal weight in milligrams was also determined for each sample by means of the above relationships (table 1).

RESULTS

Populations. The rat populations in the three blocks had increased steadily for almost two years prior to the beginning of the study. In February, block A had reached 80, block B 93 and block C 113 per cent of the previously estimated maximum value. We cannot state that the estimated population values are clearly different from the calculated asymptotic values on the basis of existing information. However, the fact that all three populations remained stationary for three months immediately preceding the reduction-trapping indicates that all were at or near their peak levels. The March populations were estimated to be 160 rats in block A, 130 in block B, and 130 in block C. The three populations were reduced by 46, 37 and 44 per cent of the March population respectively. During the month following reduction there was a rapid increase in population size in each block. Block A stayed at a level of about 125 rats for 3 months in spite of monthly trapping that attempted to maintain it at approximately 100 rats, and from August through October there was a definite increase in population size. Blocks B and C decreased more or less irregularly from the May estimate. These changes were in part due to trapping to maintain a constant population size, but from August on there was also a reduction in the capacity of these blocks. The environmental changes resulted from such clean-up procedures as removing fences, outbuildings, and other harborage for rats on a more or less random basis by the owners of the individual properties. By October it was apparent that the lowered environmental capacities had produced irreversible changes in the populations. The October samples were the last which could be used for this study, although changes started to become apparent in August.

The population data for each block is given in Table 1 and shown graphically in Figure 2.

Adrenal weights. The pre-reduction mean adrenal weights were close to the reference values for all samples (5 male and 7 female) (Fig. 2). The adrenal weights declined precipitously from the reference values following population reduction for both sexes in each

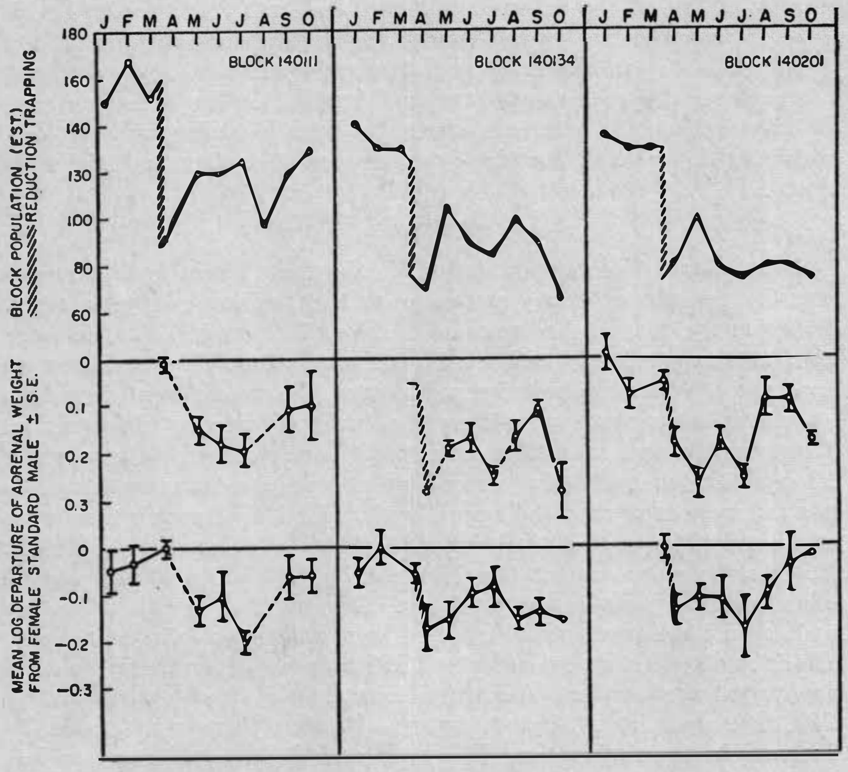

Figure 2. The adrenal weight and population histories for three populations of Norway rats. The adrenal weights are plotted as the sample means—their standard errors. Block 140111 is block A of the text, block 140134 is block B, and 140201 is block C.

of the three blocks (Fig. 2). The declines in mean adrenal value were 32.0 per cent and 23.2 per cent below the pre-reduction values for the male rats and 20.7 per cent and 26.4 per cent for the females from blocks B and C respectively (Table 1). No post-reduction sample could be obtained from block A; so that changes in adrenal weight immediately attendant upon population reduction are unknown, but comparing the reduction sample with the May sample a month later reveals a decline of 27.2 per cent in the male adrenal value and 25.7 per cent for the females. These figures are comparable to the reduction in the adrenal values for the other two blocks. The monthly figures for the per cent reduction in adrenal value in the three blocks have been combined in Figure 3. There was a 26 per cent reduction in adrenal value for both sexes from the three blocks which coincided with a mean reduction in population size of 42 per cent from the peak values.

The reduction in adrenal weight from the reference values was, with some variations, maintained for the seven months through October, although there was a slight tendency for the adrenal weight to increase from August through October. We have already seen than permanent changes in the environment began to take place between the July and August samples. The moderate increases in adrenal weight during the last three months of the study were entirely compatible with the relative increases in population resulting from lowered environmental capacities.

Several tests of significance were applied to these adrenal data. For purposes of detecting a relation between population density and adrenal weight one evidently cannot compare individual population samples, but must use a number of such samples, since population density is only one of several factors which affect adrenal weight in a given population. The mean value for each sample must be used as a unit of measurement. The three pre-reduction mean differences from the reference values for each sex for each block were compared by a t test to the post-reduction mean values for each block from April through October. The post-reduction female adrenal values from block A were significantly less ($P<0.05$) than those from the pre-reduction samples. The single pre-reduction value for the male rats from this block was 7.2 standard errors from the mean post-reduction value ($P<0.01$). The adrenal values from block B, when similarly treated, indicated that for both sexes the post-reduction adrenal values were significantly less than the pre-reduction values ($P<0.001$). Similarly, the post-reduction adrenal values from block C were significantly less than the pre-reduction values for both sexes

($P<0.001$). Thus, there was a significant difference in the adrenal values for both sexes in each block associated with a 42 per cent reduction in population size. If the pre-reduction mean adrenal values from the three blocks are combined for each sex and compared with the post-reduction values for the seven months similarly combined, the post-reduction values are significantly less than the pre-reduction values for each sex ($P<0.0001$). The female rats had a mean pre-reduction weight 5.9 per cent below and a mean post-reduction adrenal weight 22.8 per cent below reference values. The difference between the pre- and post-reduction adrenal values was 16.9 per cent for the female rats. Similarly, the adrenal glands of the male rats were 8.3 per cent less than reference values for the pre-reduction samples and 32.7 per cent less for the post-reduction samples. The difference between the pre- and post-reduction adrenal values was 24.4 per cent for the male rats. The mean difference in adrenal value was 20.7 per cent for the two sexes combined. The mean population reduction was 32 per cent for the seven months after reduction trapping. These data are summarized in Figure 3. It should be noted that the changes in adrenal weight closely parallel each other for the two sexes. There is a close correspondence between the maintenance of reduced populations and the maintenance of reduced adrenal weights.

The direct relationship between population size and adrenal weight was also explored. The mean departure from the reference values for each sex from all three blocks were plotted against their respective numerical population values and a regression was fitted for each sex by the method of least squares. The slopes of the regressions were significantly different than zero for both sexes ($P<0.05$ for the females, and <0.01 for the males), indicating a direct relationship between population size and the deviation of adrenal weight from reference values.

Finally, the mean differences from the reference adrenal weights are plotted for several other blocks with high increasing rat populations for the same months, but not the same year, as the pre- and post-reduction samples in this study (Fig. 3). These show that there is no reason to suspect a significant seasonal change in adrenal weight between December and May which might account for the sharp decline in adrenal weight following the population reductions in this study.

DISCUSSION

There was a direct relationship between population density and the weight of the adrenal glands in populations of mice in the labora-

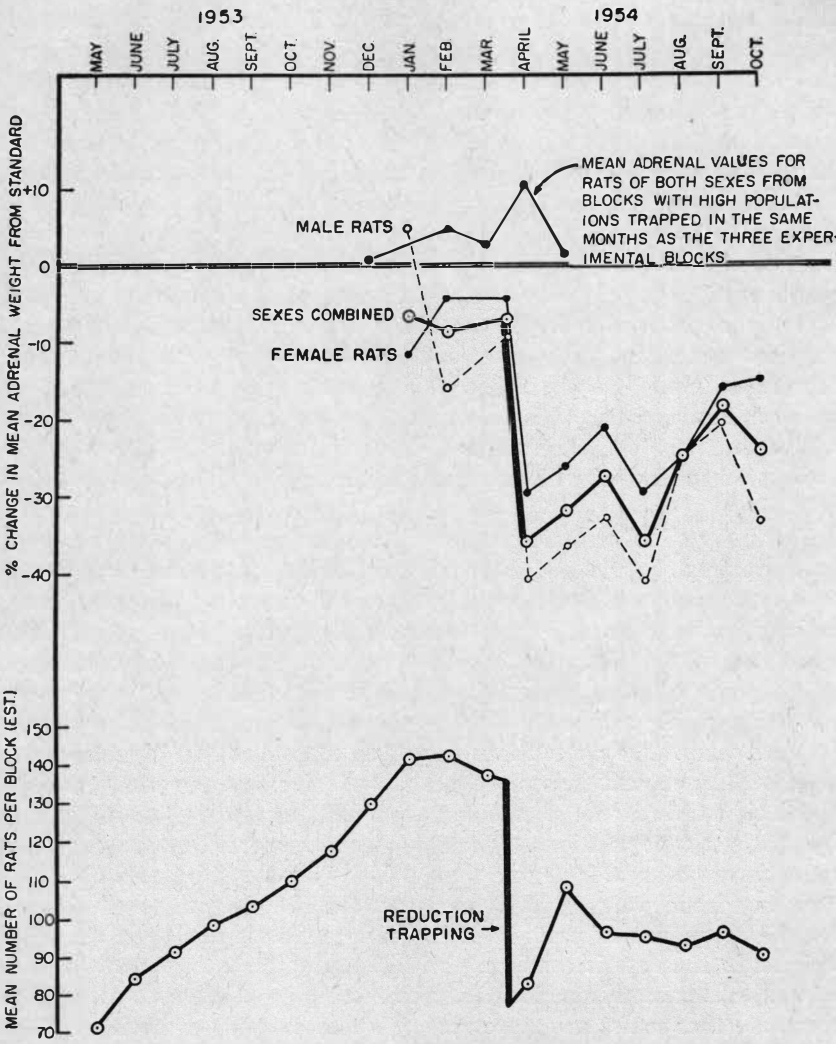

Figure 3. The adrenal weight and population data for blocks A, B, and C combined. The population history is given as the mean monthly population size extraplated from plots of the individual block populations. The changes in adrenal weight are the mean per cent changes from standard values (see text). The per cent changes in adrenal weight from the standards for several other high block populations are given for comparable months, but not the same year, as the experimental populations for comparative purposes.

tory and in populations of wild urban and rural rats (Christian, 1954; 1955a, b, c). The adrenocortical response was proportional to population size in the laboratory mouse populations and many of the rat populations in spite of an excess of food, water, and harborage, although inadequate amounts of these necessities can be major contributors to the activation of the adrenal cortex (Ershoff, 1952). The adrenal response in these experiments appeared to be a function of density per se acting through undetermined socio-psychological factors. Apparently any high population will exhibit a marked adrenocortical response, although the immediate mechanisms of its production may vary somewhat from population to population depending on the environment. Coincident with increased adrenocortical activity there is a proportional decrease in reproductive function, general physical condition, body size, and resistance to disease and other harmful stimuli (Selye, 1950). These facts have been reconfirmed on a population basis with freely growing populations of house mice (Christian, 1955c). The limited data so far collected for a wide variety of species of wild mammals indicate that these relationships hold.

This body of information suggested that reducing high populations to approximately one half of peak values and maintaining them there on a sustained yield basis should reduce the degree of adrenocortical activity and at the same time improve the general condition of the remaining population. In these experiments a population reduction of 42 percent below pre-existing high levels resulted in a prompt reduction in adrenal weight of 25 per cent in terms of reference values for both sexes. The populations were then maintained in the neighborhood of 32 per cent below the peak levels for seven months during which time the adrenal glands averaged 20.7 per cent below the values for the peak populations. The male rats responded to population reduction to a greater degree than did the females with mean reductions in adrenal values of 24.4 per cent and 16.7 per cent respectively for the seven month period. The greater reduction in male adrenal weights in the rats is consistent with the greater enlargement of the adrenal cortex seen in male mice in increasing populations. One might speculate that males are more responsive as a result of their greater aggressiveness or that females maintain a more nearly constant level of adrenocortical function as a result of the constant demands of reproduction. In general it may be assumed that these changes in rat adrenal weights result from changes in the amount of cortical tissue (Rogers and Richter, 1948; Christian, 1954).

Maintaining populations at approximately 50 per cent of the environmental carrying capacity as a management practice appears to have a sound basis in experimental fact. The 50 per cent level would provide a maximum sustained yield and at the same time the population would be maintained in good physical condition. It might be added that maintaining populations consistently below peak levels should avoid the problems of cyclic fluctuations. The obvious drawback in this procedure from a management point of view is the necessity of accumulating adequate data to determine the 50 per cent end point. However, for a particular population adrenal weight itself might be used as an index of relative density provided that adrenal weights from a series of known population densities were available with which to compare those from the unknown population. Some conclusions might be drawn from data of other species if adrenal weights from only peak populations were available. Further work along these lines is necessary, especially for gregarious species.

SUMMARY AND CONCLUSIONS

Three populations of Norway rats were allowed to increase until they reached high levels and increases in population were no longer apparent. The weights of the adrenal glands were determined for each sex for three months at the high population levels. The populations were then reduced 42 per cent by trapping and were maintained an average of 32 per cent the maximum sizes by monthly trapping for seven months. There was a mean decline of 26 per cent in the mean adrenal weight value of the two sexes combined immediately following population reduction. The mean adrenal weight value of the combined sexes for seven months following reduction was 20.7 per cent below the pre-reduction value. The response of the male adrenal glands was approximately 10 per cent greater than that of the female adrenal glands. The experiments were terminated when it became apparent that changes in the environment were altering the capacity of the habitat to support rats. It has been suggetesd that maintaining populations at half of their maximum sizes would be a sound management practice from the points of view of maximum sustained yield and sustained good condition of the animals.

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DISCUSSION

DISCUSSION LEADER LEHMANN: Thank you for a very able presentation, Dr. Christian.

DR. W. B. DAVIS [Texas A. & M]: I was just thinking about the techniques that might be involved in this. I don't know whether you want to go into a discussion of the techniques used to measure the adrenal glands.

DR. CHRISTIAN: Well, I think that I considered that several years ago. I'm skeptical about blood sampling techniques, and we settled on using weight in relation to body measurements. In other words, we simply use the relationship of adrenal weight to body weight.

I think to measure the kind of effect that you would have in a slowly rising population, and so on—it's slow with respect to adrenal response; that's what I'm trying to say—so that weight adequately reflects it, and we found it very useful.

DR. STOKES [Utah State University]: Didn't Southwick find in his mouse studies that high litter mortality actually preceded a drop in the birth rate? How does that tie in with what you said?

DR. CHRISTIAN: I'm not too familiar with Southwick's work. I know the work of Stokes and others, but when you actually plot the actual birth rates, there's a decline; but it doesn't decline in the same magnitude that the litter mortality does, and it's just a beautiful fit.

All I can say with respect to Southwick is that the conditions may not have been the same as ours. I don't know.

CHAIRMAN BELL: If there are no other questions at this time, I'd like to put a question to Dr. Christian. In view of the known relationship between the adrenocortical function and salt balance, such as exemplified in Addison's disease in man, I wonder whether you have done any studies on salt balance or salt demand in animals in which the adrenal is put under stress.

DR. CHRISTIAN: I haven't done any salt balance studies. The only thing I know is that Frank in Germany repeated your work.

DR. JENKINS [Prince Edward Island]: I was very interested, Doctor, in wondering if a heavy population of field mice over a period of years would cause a great increase in foxes. Field mice have been a problem, and our red fox have increased to a great extent, so that in Prince Edward Island litters of six and seven are nothing at all now—they are quite common—and we wonder what causes that.

DR. CHRISTIAN: I think that it has been a common experience with quite a few people that as the prey species increases in numbers, there is a subsequent increase in the number of predators, and that their reproduction is successful and maintained at a high level.

DISCUSSION LEADER LEHMANN: I don't think he would want you to take that too literally (addressing Dr. Jenkins), because sometimes the same conditions that favor increase in the predator do the same thing with the prey too.

CHEMICALS AND WILDLIFE—AN ANALYSIS OF RESEARCH NEEDS

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The use of chemical control agents is firmly established in agricultural practice. Since World War II, many completely new chemicals have been developed successfully as pesticides and have now found wide acceptance. Many of those now being employed as insecticides, rodenticides, or herbicides are extremely toxic. Considerable care is therefore necessary in the manufacture, distribution and field use of them to ensure against injury to human beings. Danger to human beings can be minimized by proper instruction and supervision of processors and applicators, but conservationists, already alarmed at the great potential disturbance of the biotic environment by widespread applications of such toxic agents, wonder when adequate attention will be directed toward minimizing the hazard to the plant community and its dependent wildlife.

Several papers on this subject have appeared since 1946. Most attention has been directed toward DDT (*e.g.*, Cottam and Higgins, 1946; Linduska and Surber, 1948) because it was the first of its type to appear and was heralded widely as a phenomenally effective insecticide. Its effectiveness was shortly placed in perspective but the sensational performance claimed for it alarmed wildlife students. Research work was demanded and in time executed.

Much the same history lies behind the extremely toxic rodenticide "1080" (Ward, 1946). Its early usage, so widely publicized, was followed quickly by experiment with wildlife species to determine degree of hazard.

Since the appearance of these important war-bred chemicals, dozens

[1]Federal Aid in Wildlife Restoration Act, Project California, W45-R.

of pesticides—some far more toxic than DDT—have been tested and marketed. None of these has been studied adequately in relation to wildlife.

One may well question why insufficient attention has been given to this aspect. Recreationally and esthetically wildlife constitutes an immense resource. Measures for its protection may axiomatically be considered desirable. We will not discuss here why studies concerning chemical effects on the biotic environment have been insufficiently supported. Instead, we would like to present a brief account of the study techniques employed by professional toxicologists and how these techniques may be adapted to wildlife investigations.

TOXICOLOGICAL CONSIDERATIONS

In evaluating the biological effects of a chemical agent, toxicologists customarily depend on two types of studies. Acute studies describe immediate toxicity and may be the result of a single exposure to a chemical or, according to some workers, of a multiple exposure over a few days' time. Chronic studies describe multiple or continuous exposure to a chemical over a long period of time. Some investigators do not consider the effect "chronic" unless at least six months' exposure has been achieved. Others consider all multiple dosages chronic; the terminology is not precise. Frequently, investigators confuse the biological effect and the time relationship. As an illustration, "acute" may mean both brief and severe in popular parlance but in toxicology it customarily indicates the effects of whatever degree from a single exposure to a toxic agent. "Chronic" implies multiple dosage and hence an extended period of time. Severity of effect should ordinarily not be used to separate these two classes. Single and multiple exposures may yield effects of any degree. In our work we have used "acute" to describe single exposure and "chronic" to describe multiple exposure. Usually, acute effects are manifest within two weeks' time, whereas chronic effects may appear at any time during an animal's life.

In single-dose studies the standard value most frequently sought is the LD50. The term indicates the amount of chemical administered per test animal which will result in loss of 50 per cent of the test population. The LD100 is also frequently sought. This is the lowest value per test animal to result in loss of all the test population. The amount of chemical administered is ordinarily related to a standard body mass. The usual expression is in milligrams of chemical per kilogram of body weight of animal. In multiple-exposure investigations it is usual to seek not only the values above but to watch especially for the lowest concentration which produces observable effects. For these studies the amount of chemical administered may be expressed in milligrams per kilogram per unit of time but is more often expressed as parts per million (p.p.m.) in the diet or, if inhalatory effects are in question, as parts per million in surrounding vapor or gas.

Standardizations of terminology are essential in laboratory studies and desirable in the field. However, in field studies a number of environmental variables complicate such standardization. Therefore it seems best to us to accept the critical value in field use to be the lowest concentration which results in any observable damage.

In the laboratory the route by which a test chemical enters the body of an animal is usually restricted to one, whereas rarely in field use does an animal receive a chemical by one route alone. Frequently it is difficult to decide which routes were involved and which route was responsible for the most severe effect. There are three routes of intake customarily studied by toxicologists:

Oral:—In acute studies a known amount of chemical may be administered orally in gelatin capsules, a non-toxic matrix, or by stomach tube. In field use a contaminated food supply would provide the equivalent route of intake.

Dermal:—A known amount of chemical applied to the skin on a cloth "patch" in the middle of the back is the usual method of determining toxicity of chemical by skin absorption. In field use, vegetation contaminated with sprays or dusts presents ample opportunity for dermal exposure.

Respiratory:—The respiratory or inhalatory effect is particularly pronounced with highly volatile formulations or with most formulations at the time of application. Toxic effects result primarily from assimilation in the lungs. In the field, wildlife may be affected by high vapor concentrations under dense vegetation and in some instances may be faced with a diminished oxygen supply (a form of fumigation).

ECOLOGICAL CONSIDERATIONS

Biologists cannot be concerned with chemical toxicity alone. Fully as important are the less-well-understood effects of chemical usage on cover and food supply. It is in this area of study that the wildlife student can best contribute to understanding of chemical effects in the field. Few chemists and toxicologists appreciate the diversity of environmental conditions and the complexities of ecological interactions.

It is a truism that animal life depends on appropriate plant cover. Alterations in density, species composition, fruiting propensity and chemical components of plants will inevitably be reflected by the dependent fauna. Two different situations are worthy of mention here. The first concerns the alteration of the chemical nature of certain plants by use of herbicides. It is widely believed that plants may be rendered toxic or that toxic plants may become palatable to animals by the use of herbicides. Generally speaking, such beliefs are poorly substantiated (S. N. Fertig, pers. comm.). Many reports involving domestic animals have proved upon investigation to be due not to toxic plants but to quite unrelated factors (*e.g.*, "hardware" disease; rodent poisons). Although reports of this type of poisoning seem exaggerated, it would be well to investigate this problem experimentally to determine the likelihood of such poisoning.

The second situation shows considerable promise as a tool in wildlife management. Herbicides may assist in altering plant cover with the goal of increasing carrying capacity for wildlife. Breaking up dense, uniform vegetation greatly increases the "edge" environment. Chemical treatment may be combined with mechanical means and reseeding to achieve the maximum benefit from this type of land management. There is little doubt that herbicidal usage will be expanded for this purpose.

Effects on food supply are unquestionably important. How important, we do not know. Couch (1946) showed that crayfish loss resulting from application of DDT seriously affected the raccoon population of an Illinois bottom land. Robbins and Stewart (1949) demonstrated that insectivorous birds were forced to leave treated areas of forest land because of a reduced invertebrate food supply. Yearly applications of DDT in the same area can effect an over-all decline in populations of birds (Robbins *et al.*, 1951). Mobile species would probably suffer little from localized chemical applications, but widespread reduction of food supply will inevitably be accompanied by population declines. Sedentary animals would suffer considerably. More severe effects may be expected during the reproductive season. The extent of such effects on diversified agricultural lands where chemicals are frequently applied is not known. Many sportsmen have expressed the belief that sharp declines in reproductive success in some game species may be attributed to agricultural chemicals. Reduced food supply for adults and young has been advanced as one factor in limiting reproductive success. We have demonstrated experimentally that three common agricultural chemicals will lower the reproductive success of penned ring-necked pheasants and, further, that each chemical affects different phases of reproductive activity (Genelly and Rudd, MS). Nonetheless, there are, at present, no data to indicate clearly that these effects occur under field conditions or how they may be influenced by reduced food supply. This subject merits considerable study.

At the present time the ecological effects of chemical usage are poorly understood. Extensive investigations may be necessary before some of the simplest questions may be answered.

FIELD REPORTS AND EXPERIMENTATION

Seldom can an incident of animal loss attributed to an agricultural chemical be understood by field data alone. Critical attention is given to an incident only if effects are severe and obvious. Attempts to reconstruct the prior circumstances causing wildlife losses are frequently fruitless. Accordingly, we must depend on other sources of information to determine the likelihood of loss in the field and to estimate the degree of hazard accompanying future use of the chemical. Ideally, the information necessary to determine the likelihood of animal loss in the field follows this sequence: determination of lethal values for all routes of intake on laboratory animals; determination of these values for common game species; consideration of reports of field use of chemical; experimentation in the field.

We have attempted to follow this sequence. Usually we have been forced to start with scanty field reports, to pool fragmentary toxicological information, and to design experiments which seem to bridge the deficiencies in data. To illustrate this pattern we would like to draw on personal files regarding animal loss in three important agricultural environments in California.

Rice fields.—The rice fields of the central valleys support high populations of pheasants and waterfowl. DDT is regularly applied to seed rice in efforts to control the depredations of tadpole shrimp and scavenger beetles. Treated rice is readily available to grain-eating birds. Several field reports indicate noticeable mortality of ring-necked pheasants (*Phasianus colchicus*) and mallard ducks (*Anas platyrhynchos*) during and immediately after the sowing season. The concentration of DDT on rice at recommended field levels is 20,000 parts per million. This concentration is many times greater than that necessary to induce serious damage in laboratory animals. Experiments with penned birds demonstrate quick acceptability of the contaminated food by pheasants and ducks, with death occurring in 4 to 13 days (Rudd and Genelly, 1955). In this instance there is no doubt that damage to certain species has occurred in the field and may be expected to occur in the future.

A serious outbreak of rice leaf miner in 1953 required emergency control measures. After field-testing to select the most effective chemical available, dieldrin was applied at the rate of one-half pound per acre. Elemental rules of proper pest control were not observed. Sprays were applied in strong winds, and roads and drainage canals were directly sprayed. Although local insect damage was severe, many thousands of acres were treated as "insurance." Reports of wildlife loss included many vertebrate species, but mortality was most striking in fish, herons, and egrets. We experimented with pheasants and ducks to determine the likelihood of loss of these important game species. Ducks were seemingly unaffected by doses of 500 milligrams per kilogram of dieldrin. Experiments with ducks were discontinued because the amounts necessary to cause mortality would not be encountered under field conditions. Pheasants, however, were sensitive to dieldrin. The acute oral LD50 value was about 10 milligrams per kilogram, many times more toxic than DDT. Pheasants were exposed to dieldrin by two other routes. To check dermal toxicity, the undiluted formulation was applied directly to the skin in amounts of 5.28, 10.56, and 15.84 milligrams of dieldrin. At one-half pound of dieldrin per acre, a surface deposition of 5.28 milligrams per square foot may be expected. A pheasant has approximately one square foot of absorptive surface. No effects were apparent from this treatment. Adventitious toxicity (the exact routes of entry are not known) was checked by spraying a pen area at the rate of 5.28 milligrams per square foot. Birds were thoroughly wetted during this treatment. There were no apparent effects from this treatment. We conclude that significant mortality of pheasants and ducks from single exposures of dieldrin at one-half pound per acre is not likely. It should be emphasized that these conclusions apply only to these species. The great difference in oral toxicities between these forms illustrates the folly of judging the response of all wildlife species by the known reaction of any one.

Irrigated pasture lands and alfalfa.—Like the rice fields, irrigated forage and hay crop lands sustain high pheasant numbers. Toxaphene at values of 2 to 3 pounds actual chemical per acre has caused some pheasant mortality. This chemical is often applied, particularly as dusts, in amounts up to 8 pounds per acre. The extent of pheasant loss is unknown. Experiments indicate an acute oral toxicity of about 90 milligrams per kilogram. Pheasants consuming 300 parts per million in the diet for several weeks showed considerable weight loss and marked degenerative changes in the liver but no mortality. Although field reports concerning pheasant mortality from toxaphene are reliable, it is difficult to understand the precise mechanisms inducing death under these conditions. Our data give no clues to them. Curiously, rodents (*Microtus californicus*) may be effectively controlled with a broadcast toxaphene spray at 4 pounds actual per acre in the field but could not be killed at much higher values in the laboratory (L. P. Tevis, Jr., pers. comm.). Further inquiry is obviously in order to discover why the field effect is stronger than experimentation suggests.

Toxaphene at lower rates was alleged to have been responsible for a considerable mortality of ducks and geese, both wild and domestic, in the San Joaquin Valley. In this instance, mosquito abatement personnel had applied the chemical. DDT and a DDT-dieldrin mixture had been applied successively to the same area earlier in the season. Two domestic geese at autopsy showed unusual hemorrhaging of the heart and spleen. Liver samples from these birds contained organic chloride cencentrations of 34 and 180 parts per million, considerably above values to be expected in normal liver. Toxaphene seems to have been responsible for deaths of these birds. Perhaps, prior exposure to DDT and dieldrin—both of which have a long residual toxicity—increased the susceptibility of these birds to the insecticide.

Orchards.—We have received several reports of vertebrate mortality in orchard situations. Two orchard habitats especially deserve attention here. One account describes an orchard type in which a permanent cover crop is maintained; the other concerns a type in which "clean farming" is practiced. No surface growth is maintained in orchards of this second type.

Cover crops on which domestic animals graze are maintained in many pear orchards. Such crops are considered valuable though they may be reservoirs of insect and rodent pests. Recently, attempts were made to control latent populations of an insect pest in cover crops by the use of dieldrin at one-half to one and one-half pounds actual chemical per acre. Vertebrate animals affected, as reported by orchardists, include California quail, ring-necked pheasants, cottontail rabbits, jackrabbits, meadow mice, gophers, snakes, and domestic dog. The effect on rodents led some growers to suggest the use of a dieldrin spray to control these pests. There is little doubt about the effectiveness of dieldrin but its use for any single purpose must be tempered by the hazard it presents to other forms of life. In this instance, it seems clear that more chemical was applied than was necessary to control the pest insect. State officials to date have not permitted the use of broadcast sprays to control rodents because of the high application rates. Neither immediate nor residual toxicities are sufficiently known to warrant such permission. One final comment on this situation might be made. Cover crops considered as livestock food are an asset to a grower. But damage to trees and fruit by insect and rodent pests, which are sustained in this cover, combined with costs of controlling these pests may render the forage value slight. Perhaps, a simple change in cultivation practices would solve many of the pest problems plaguing these orchardists.

The second orchard type is illustrated by the citrus groves of Southern California. Wildlife mortality in these orchards seems to be confined to animal occupants of bordering fence rows and to song birds in the orchard trees themselves. Cottontail rabbits, ring-necked pheasants and small birds have been reported killed in these orchards. Many agriculturists have reported loss of small birds. One county official remarked that some spray operators consider bird mortality an indication of adequate chemical treatment.

DISCUSSION

We have illustrated by example how the environment conditions affect the toxicity of agricultural chemicals to animals. Obviously, the usual toxicological information may not be used alone to determine chemical hazards to wildlife. Generalizations are better avoided. We cannot state too strongly the necessity to consider each local chemical-wildlife relationship separately. Factors operating in one area may not operate in another; or chemicals may be used at the same locality in different seasons or years without necessarily the same effects.

If generalization is not wise, how may we estimate chemical hazard to wildlife? Certainly we would not wish to depend on incidents of animal loss in the field for this estimation. The answer lies in continued support of research investigations which explore the problem from many viewpoints. Practically, we cannot expect any one research group to provide all of the necessary data. It appears to us that the best solution is active and cooperative investigative work by several interested agencies. The U. S. Fish and Wildlife Service, state conservation departments, universities and chemical manufacturers immediately come to mind as interested agencies. Each of these groups may be expected to approach this problem in a different manner. Combined data from these sources provide as much basis for estimating hazard as can reasonably be expected. The important ingredient of such a program is active, well-supported research by all agencies.

It is frequently suggested that all responsibility for testing be placed with the chemical manufacturers. We would agree that the basic producer of a hazardous chemical should assume an important role in research investigations. Responsible firms do so at the present time. However, we believe that it is impractical and unwise to require a manufacturer to conduct wildlife researches in addition to the other phases which he must pursue in order to sell his product. Most chemical firms do not have trained zoologists on their staffs. Although these firms should not be asked to assume the entire burden of investigation, in view of their self-interest, they may reasonably be expected to support in major part the research activities of other groups investigating field performance of their products.

A discussion of this subject is not complete without consideration of wildlife values. Those interested in conservation matters are acutely aware of differing perspectives on the utilization of natural resources. Heated controversies continue to rage on such issues as grazing rights, lumbering on public lands, and utilization of water resources. The same elements of controversy are present in the estimation of wildlife values. As an illustration, let us consider the concept of minimal loss. Definitions of the "minimum" are as varied as the interests involved. We have heard suggestions that if the usage of all agricultural chemicals were abolished, animal loss would not exist since there would be no exposure to chemicals. At the other extreme, occasionally we hear suggestions that agricultural production should not be hindered by "unrealistic" esthetic considerations, that "progress is inevitable," and that conservationists have no "right" to interfere in any business activity, be it farming or manufacturing. Animal losses could be extreme if such attitudes were common. Between these extreme viewpoints the "minimum" assumes subtler meanings and may not be defined precisely. It is our view that concern about chemical usage is warranted at the first perceptible disturbance to any phase of wildlife biology. Every effort should be expended to keep such disturbance nothing more than perceptible.

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DISCUSSION

MR. DAHLEN [Madison, Wisconsin]: Do you have the acute toxicity figures for California quail for various insecticides?

DR. GENELLY: No, we don't have. We have had on pheasants. For dieldrin it was 10 milligrams per kilogram for pheasants. For Toxaphene, it's around 80 or 90 milligrams per kilogram.

MR. F. S. BARKALOW [North Carolina]: Have you had any experience with granular formations?

DR. GENELLY: No, sir. We have used emulsifiable concentrate only. For DDT we use powder.

DISCUSSION LEADER LEHMANN: I think in addition to the effect of chemicals we might even go beyond. I have noticed down in South Texas, where aerial spraying of mesquite brush for range improvement has been carried out for the killing of undesirable plants, that lawsuits have developed from 65 miles away. We have noticed greatly increased earthworm activity, and we wonder if the effects might not carry only to earthworms but to microorganisms in the soil as well.

It seemingly is never too early to check and be on the alert in your own particular area for the laws governing the use of chemicals in large-scale operations, for very often the laws lag quite far behind the development of certain chemicals and their use.

HYDATID DISEASE (Echinococcus granulosus) IN SASKATCHEWAN BIG GAME

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Hydatid disease caused by the larval form of the tapeworm *Echinococcus granulosus* has been reported from time to time in various species of North American wildlife. However, to the writers' knowledge there are no definite previous records of this disease in Saskatchewan mammals. Although the possibility of hydatid infections in Saskatchewan big game species had long been considered, the first case did not appear until November, 1953. The absence of information on this disease in Saskatchewan combined with its significance to public health have prompted this preliminary investigation.

REVIEW OF LITERATURE

A search of literature has revealed few records of the occurrence of hydatid disease in North American big game species. Riley (1939) urged workers to publish findings on this important parasite, but to date the available information is meager and for widespread areas entirely lacking. The occurrence of hydatid disease has been reported in western Canada (Cowan, 1948, 1951) in the following species: coast deer (*Odocoileus hemionus columbianus*) one record from Vancouver Island, B. C.; mule deer (*Ococoileus hemionus hemionus*) one record, Jasper, Alta.; elk (*Cervus canadensis nelsoni*) two per cent of the Jasper National Park herd infected in 1946. Green (1949) reported about six per cent of the elk (*Cervus canadensis nelsoni*) in the Banff National Park herd were infected in 1946. Riley (1939) cites one record of hydatids in moose (*Alces americana*) from Le Pas, Manitoba, and another from a captive moose in Ontario. In addition he reports that hydatid cysts of *Echinococcus granulosus* were found in the lungs of 11 of 21 moose and of 1 deer collected from northeastern Minnesota. Additional work in Minnesota (Olsen and Fenstermacher, 1943) revealed a total of two infected white-tailed deer. A report by de Vos and Allin (1949) records three moose found infected during the fall of 1948 in northeastern Ontario. These workers also state that *Echinococcus granulosus* was the most common tapeworm found in the digestive tracts of timber wolves (*Canis lupus*) examined during the same winter. Sweatman (1952) also reporting on work done in Ontario found hydatid cysts in 17 of 29 moose and the adult tapeworm in 36 of 58 wolves. The only available evidence that might be considered as a Saskatchewan record is contained in a report by Miller (1953) in which he states "moose infected with hydatid cysts have been recorded from every province west of the Maritimes." This suggests a Saskatchewan record but does not substantiate it. Hydatid disease has also been reported (Banfield, 1954) in two specimens of barren ground caribou (*Rangifer arcticus arcticus*). One specimen was killed on the Hudson Bay Railway, Manitoba and the other at Nueltin Lake.

In Alaska, hydatid disease is reported (Rausch and Schiller, 1951) to be well distributed in both dogs and wild canids such as the Arctic fox (*Alopex lagopus*). The only important intermediate host was determined to be the tundra vole (*Microtus oeconomus imictus*). Skin tests with non-specific Echinococcus antigen on residents of St. Lawrence Island, Alaska, revealed a high incidence of human reactors (20 per cent-28 per cent). A more recent report by Rausch (1952) indicates the red-backed vole (*Clethrionomys rutilus albiventner*) is also an important intermediate host. In addition he reported the St. Lawrence Island species of *Echinococcus granulosus* differs from the mainland form, the latter infects big game species such as moose and caribou whereas the former infects rodents. The importance of this disease in humans is also indicated by Banfield (1954) who cites

"Davies (1946)" (no reference listed in bibliography) as reporting 37 cases in Canadian hospitals in the past 15 years. Miller (1953) reports that within a five-year period prior to 1953 information was collected on 141 patients with proven or suspected autochthonous hydatid disease from British Columbia, Alberta, Saskatchewan, Manitoba, Ontario, Yukon and the Northwest Territories. Of these, three cases occurred in Eskimos, 136 in Indians and the remaining two cases in whites. He also reports that of 114 dogs autopsied, 32, or 28 per cent were positive for *E. granulosus*. Although Miller assumes a large reservoir of this parasite in wild canids, he states that the dog is of prime importance as a means of transportation of the disease to the Indian population in northern Canada. Schiller (1954) demonstrated experimentally that eggs of *Echinococcus* spp. could be transmitted from contaminated feces to a susceptible secondary host. He states that "human infection in northern countries most probably results from direct contamination through close association with canine animals harbouring the adult cestodes; however, the possibility that fly vectors may be important in the epidemiology of hydatid disease in any area where *Echinococcus* is endemic should not be overlooked."

PROCEDURE

This report covers a period of investigation from November 1953 to February 1955, during which time an attempt was made to collect specimens from all species of Saskatchewan big game. Specimens were collected in the field by the writers and other Department of Natural Resources field staff or forwarded to our laboratory by interested sportsmen. When immediate examination was not possible specimens were preserved by freezing. Most of the specimens were collected during the various big game seasons chiefly by contacting hunters in or near the hunting areas. The majority of the specimens collected were from moose killed during the six-day open seasons of December 1953 and 1954.

Specimens examined included lungs, liver, heart and other tissue but varied from small portions of these tissues to entire carcasses. However, because hydatid cysts were found exclusively in lung and liver tissue these parts only will be considered in this report. During this investigation other parasites were noted but are not included in the data presented.

Lung and liver tissues were examined for hydatid cysts chiefly by manipulation and excision. Small cysts in lung tissue were not readily found although most of these could be located by compressing

TABLE 1. INCIDENCE AND OCCURRENCE OF HYDATID DISEASE (*ECHINOCOCCUS GRANULOSUS*) IN SASKATCHEWAN BIG GAME

Species		No. Specimens		Immature No. Negative	Immature No. Positive	Adult No. Negative	Adult No. Positive	Unknown No. Negative	Unknown No. Positive	Total Positive	Per cent Positive
Moose	Male	48		11	1	18	13	4	1		
(*Alces americana*)	Female	32		3		11	8	8	2		
	Sex unknown	16	96	1		1	1	10	3	29	30.2
Barren ground caribou	Male	10		1		7	2				
(*Rangifer arcticus arcticus*)	Female	4	14	1		2	1			3	21.4
White-tailed deer	Male	10				6	1	3			
(*Odocoileus virginianus*)	Female	10	20	2		3	1	4		2	10.0
Mule deer	Male	1				1					0
(*Odocoileus hemionus*)	Female	2	3	1		1					
Deer	Male	1						1			
(*Odocoileus* unclassified)	Female	2				1		1			
	Sex unknown	2	5			2					0
Elk	Male	1				1					
(*Cervus canadensis*)	Female	1	2			1					0
Antelope	Male	3				2		1			
(*Antilocapra americana americana*)	Female	4	7			4					0
Total		147		20	1	61	27	32	6	34	23.1

the tissue by hand on a smooth hard surface. If cysts were not located by this method the specimen was carefully sectioned. Cysts found in this manner were excised for identification.

All specimens were examined by the writers and in most cases their identifications were confirmed by other laboratories.

RESULTS AND DISCUSSION

During the study a total of 147 specimens were examined (Table 1) including 96 moose, 14 barren ground caribou, 28 deer, 2 elk and 7 antelope. Of these, 29 moose, 3 barren ground caribou and 2 white-tailed deer were found infected with hydatid cysts. The locations of the cysts as found in the three secondary host species are presented in Table 2 and cysts typical of those found in lung and liver tissue of moose are portrayed in Figures 1 and 2. It is apparent that the parasite was encysted more frequently in lung than liver tissue which agrees with data presented by Sweatman (1952) who compared site of encystment of the parasite in man and other animals.

The distribution of infected animals is presented in Figure 3. Of the 29 infected moose, 7 were from the Meadow Lake area where 21 specimens were collected, 3 were from the Prince Albert-Nipawin area where 27 specimens were collected and 19 were from the Hudson

Figure 1. Hydatid Cysts in Lung of Moose.

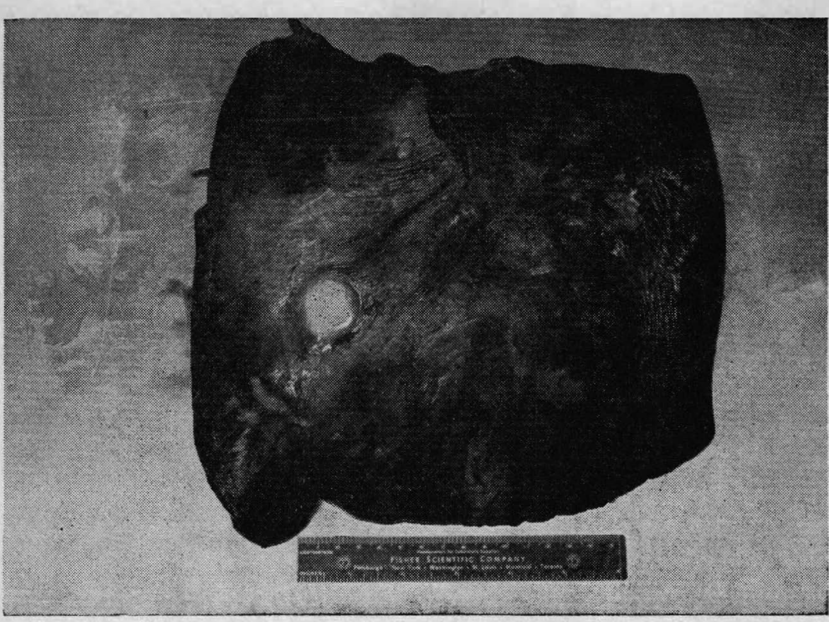

Figure 2. Hydatid Cysts in Liver of Moose.

Bay area where 48 specimens were collected. The concentrations of infected moose in the Meadow Lake and Hudson Bay areas may not be significant since both hunting pressure and specimen collection were intensified in these regions. However, it is evident that hydatid infections in moose occur throughout most of the southern portion of their range.

Although 30.2 per cent of the moose specimens were infected, this figure may not give a true picture of incidence in surviving moose populations. There was a greater tendency for hunters to submit abnormal rather than normal-appearing specimens, which tended to increase the recorded incidence. On the other hand, and perhaps of greater significance, the recorded incidence may be lower than actual because many specimens were limited to small portions of lung or liver tissue. It is felt that a more complete examination of each animal would have resulted in an increased incidence of infection and therefore the actual incidence may be higher than that reported.

The barren ground caribou specimens were collected during a timber wolf control program conducted along the boundary of Saskatchewan and the Northwest Territories. Of the three positive hydatid infections two were collected in Saskatchewan and one at Wholdaia Lake in the

TABLE 2. LOCATION OF HYDATID CYSTS IN TISSUES

Host	No. of livers examined[1]	No. of livers with cysts	No. of lungs[1] examined	No. of lungs with cysts
Moose	67	7	69	25
Deer	22	2	22	0
Caribou	10	0	10	3
Totals	99	9	101	28

[1]Entire liver and lung tissue were not examined in all cases.

Northwest Territories. The number of barren ground caribou examined was small, but the data suggest a relatively high incidence of infection.

It is of interest that with the exception of the two white-tailed deer specimens from the Moose Mountain Park area of southeastern Saskatchewan no infected deer were found. This is particularly interesting when it is considered that the majority of the deer specimens were collected in areas having a high incidence of infection in moose as well as having relatively high wolf populations. Also, the Moose Mountain Park area has for many years supported few moose and few if any timber wolves. There are, however, relatively high coyote (*Canis latrans*) and deer populations in this area which suggests a coyote-deer cycle rather than the frequently accepted timber wolf-moose cycle of the parasite.

The absence of hydatid cysts in antelope, elk and mule deer cannot be considered significant due to the small number examined. Further investigation may, however, reveal infections in these species. Sex and age differentiation of infected animals is recorded in Table 1 but it is felt that these data are also insufficient.

In an attempt to determine the terminal host species of this parasite in Saskatchewan, two dogs, two coyotes, and two red foxes (*Vulpes fulva*) were examined with negative results. Although not associated with this study but worthy of note is work conducted by one of the writers (Ruttan, 1951) who examined 44 coyotes and one red fox from the farming region of south central Saskatchewan. All were negative for *Echinococcus granulosus*.

The implications of hydatid infections to big game have not been fully explored, and in this study the effect of hydatid infections upon the secondary host species was not established. Some infected animals examined were impoverished and appeared to have a lowered vitality but this did not hold true in all cases. It is logical to assume that well-developed infections in lung and/or liver would result in a general debility of the host animal. This factor could be indirectly responsible for mortality during periods of stress and suggests a possible manage-

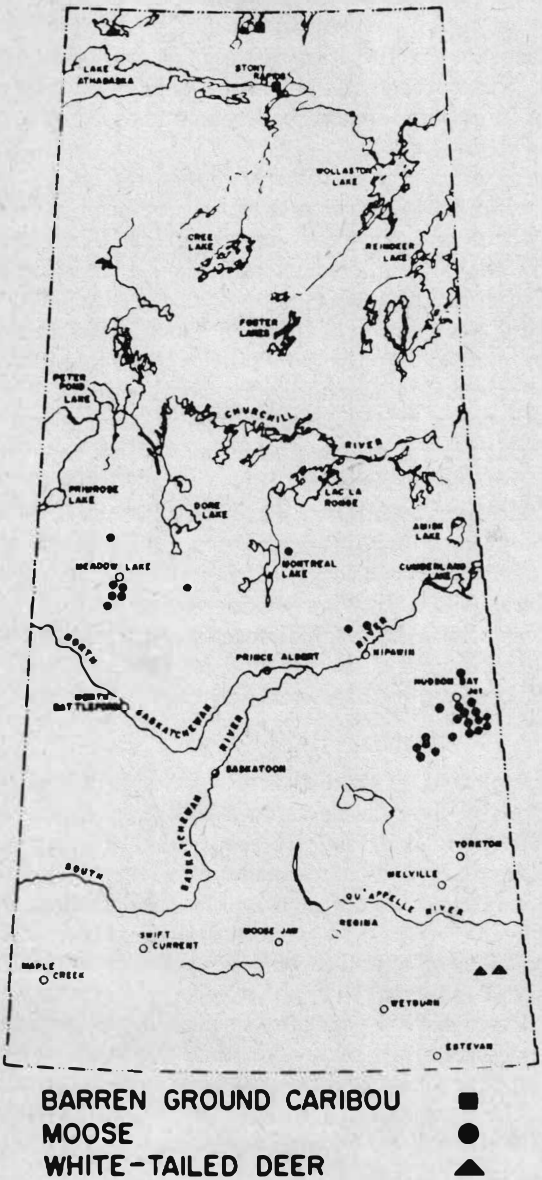

Figure 3. Distribution of big game animals infected with *Echinococcus Granulosus* in Saskatchewan. November 1953-February 1955.

ment problem. From the writers' field experiences it was evident that most Saskatchewan hunters lost much of the pleasure of their hunt when a serious hydatid infection was found in their animal and in some cases the entire carcass was abandoned. This fact points to possible difficulties in the harvesting of a big game species if the disease was widespread and of high incidence. However, under conditions of increasing demands for game such a problem might be of little significance.

Control programs have not been established, but both timber wolf and coyote poison-bait control programs have been in effect in Saskatchewan for three and six years respectively. These programs have been effective to varying degrees and should assist in reducing the incidence of the disease in big game species. More liberal harvesting of big game and destruction of infected material have been encouraged with a view to reducing the possibilities for completion of the life cycle of the parasite. In addition, northern residents are being encouraged to use a suitable vermifuge in their dogs. This latter practice is basically an aid to public health but can provide similar benefits to big game species.

Hydatid infections in humans in North America have been reported periodically but most studies have been limited in scope. Even so, these studies have shown a large number of cases in the human population (Miller, 1953). In view of increasing human populations in northern areas where hydatid disease is most likely to be found, it would seem that hydatid disease and its implications to that population should be given greater consideration.

SUMMARY AND CONCLUSIONS

During the period of study, from November 1953 to February, 1955, lung and/or liver tissue from a total of 147 specimens of Saskatchewan big game including 96 moose, 14 barren ground caribou, 24 deer, 2 elk and 7 antelope, were examined for cysts of *Echinococcus granulosus*. Of this total 29 moose, 3 barren ground caribou and 2 white-tailed deer exhibited positive hydatid infections. Six canids including 2 dogs, 2 coyotes and 2 red foxes were also examined and were found negative for the tapeworm *Echinococcus granulosus.*

The importance of this parasite to wildlife and human populations has not been definitely established, and, from the foregoing discussions, there appear to be several problems which the writers feel indicate a need for more basic and detailed studies devoted to such aspects of the disease as sylvatic cycles, method of transference to humans and practical control measures.

ACKNOWLEDGMENTS

The writers wish to express their appreciation and thanks to Dr. M. K. Abelseth, D.V.M., Animal Pathologist, Saskatchewan Department of Public Health, Division of Laboratories, Regina, Saskatchewan, who generously provided personal assistance, advice and laboratory facilities throughout the study; to Dr. T. W. M. Cameron, Director, Institute of Parasitology, Macdonald College, Quebec, for verification and identification of specimens; to the Saskatchewan Government Photographic Services for photographs; to Messrs. S. Prystupa and F. W. Terry, Game Management Officers, for the collection of barren ground caribou and other specimens; to D. B. Smith, Conservation Officer, Hudson Bay, for assistance in collection of moose specimens and to the other members of the Saskatchewan Department of Natural Resources field staff whose assistance and cooperation made this study possible.

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DISCUSSION

CHAIRMAN BELL: I'd like to ask a question. Generally in infestation with metazoon parasites that produce cysts there are two effects that may result in mortality. One is the mere physical presence of the parasite, the expanding parasite in a vital organ; and another one is the overwhelming infestation and the anaphylactic reaction resulting from a broken cyst.

In our work in Minnesota I encountered two specimens that had extremely high infestation of *Taenia pisiformis.* There were cysts that were completely calcified, and in those two specimens we encountered amyloidosis, a disease which is usually associated with chronic infection.

I want to ask Mr. Harper if he has ever encountered amyloidosis in the animals he has studied.

MR. HARPER: A very short answer, Dr. Bell—no.

MR. HENRI PINARD [Montreal]: I may be sticking my neck out, but you mentioned in your talk where the animals that you examined seemed infected. Apparently it happens everywhere except in Quebec. Is there any special reason?

MR. HARPER: Well, the animals that we examined came only from the Province of Saskatchewan. Some of the literature that I have reviewed prior to preparing the report indicated specimens from British Columbia, Alberta, and one case in a moose in Manitoba and seven moose in Ontario were infected, but as far as I know there are no reports of infections in Quebec. However, there is no reason to believe that there should not have been.

You have the mechanics available. You have wolves and moose and other big game species, and it would seem to me it would be logical to assume there would be specimens.

MR. PINARD: We never were told by anybody to investigate for that.

MR. HARPER: We were conducting our own investigation in our own province. We did not examine the specimens from any other area.

MR. PINARD: What you got were reports? They were not specimens?

MR. HARPER: I reported on specimens collected only in our own province. That is all. Any other information that I stated was from the literature that has been reported by other workers.

MR. PINARD: Oh, I see. I didn't understand you rightly. You got literature from the provinces, and that's how you reported; but you didn't examine the specimens—or am I wrong?

MR. HARPER: I collected literature from quite a number of places, including some correspondence with Macdonald College, with Dr. Cameron.

MR. PINARD: I'll have to read your paper again to make sure. I thought I understood you to say that you had examined specimens from everywhere, even Yukon. I didn't hear this province mentioned, and I was just wondering, that's all.

MR. HARPER: I think I mentioned the evidence of the disease in other areas, and I think perhaps that's where I confused you. I'm sorry.

MR. PINARD: It's all right; it's my fault.

A PRELIMINARY EVALUATION OF QUAIL MALARIA IN SOUTHERN ARIZONA IN RELATION TO HABITAT AND QUAIL MORTALITY[1]

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The southwestern quails are known to exhibit violent fluctuations in numbers. These fluctuations are apparently not periodic in nature, but unusual abundance may be followed by several years of extremely low numbers. The possible role of disease and parasites in these periodic disappearances has been considered by several investigators.

Malarial infections including both *Haemoproteus* and *Plasmodium* have been investigated periodically in the Southwest since O'Roke (1928) discovered a *Haemoproteus* infection in California valley quail (*Lophortyx californica vallicola*). Further investigations of this parasite in California were reported by O'Roke (1930, 1932), Herman and Glading (1942), Herman and Bischoff (1949) and Kadner (1941). Campbell and Lee (1953) reported on their studies of quail malaria in New Mexico. Both Gambel's (*Lophortyx gambeli gambeli*) and scaled quail (*Callipepla squamata pallida*) exhibited a mixed infection of *Haemoproteus* and *Plasmodium* in a state-wide survey.

There have been few extensive studies of quail malaria under natural conditions. Most of the important findings have resulted from experiments with captive quail.

From 1952 to 1955, the Arizona Cooperative Wildlife Research Unit has conducted an investigation of Gambel's and scaled quail movements, survival and production in southern Arizona. This work has been coordinated with measurement of the degree and intensity of blood parasitism.

Louse flies were first observed on quail during banding operations in 1952. Specimens of these flies were identified as *Stilbometopa impressa* Bigot by Dr. J. C. Bequaert of Harvard University. This is a known carrier of *Haemoproteus* infection (Herman 1945; Herman and Bischoff 1949). This louse fly has been found on both Gambel's and scaled quail on all study areas and throughout most of the year. *Stilbometopa impressa* is believed to be the major malaria vector in this area. It was reported as an external parasite by Gorsuch (1934) during his study of the life history of Gambel's quail in Arizona.

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[1]This study is a contribution of the Arizona Cooperative Wildlife Research Unit, supported by the University of Arizona, U. S. Fish & Wildlife Service, the Arizona Game & Fish Commission and the Wildlife Management Institute.

During the banding operations of the current study as many as four flies were found on one quail, but usually less than one per ten quail was recorded.

Dr. Carlton M. Herman of the Patuxent Wildlife Research Refuge has identified the blood parasite as *Haemoproteus lophortyx* O'Roke. This was the only malarial parasite found in the blood slides examined.

The quail research conducted by the Arizona Cooperative Wildlife Research Unit has been of an intensive nature. Investigation of movement, food and water relations, reproduction and population changes has been mostly confined to three major study areas. Each area is representative of one of the major vegetative zones included in the distribution of Gambel's and scaled quail in southern Arizona.

Area A is located in a subdivision of the Lower Sonoran desert typified by saguaro cactus (*Cereus giganteus*) and palo verde trees (*Cercidium microphyllum*). This zone has the lowest annual precipitation and also the highest average and maximum temperature. While much of Arizona's quail habitat is of this type quail density is usually lowest here. The study area is on the grounds of a private school and is 1,000 acres in extent.

Area B is located in the desert grassland typified by yuccas and mesquite in association with semi-desert grasses. The study area is a 640-acre experimental range owned by the University of Arizona. Area B is the only area including scaled quail habitat. Scaled quail are seldom found in other vegetative types in southern Arizona.

Area C lies within a transition zone between the desert grassland and oak grassland dominated by Emery oak (*Quercus emoryi*). It is within this zone, with the resulting edge and a choice of food and cover plants, that Gambel's quail reach at least as high a density as anywhere in southern Arizona. Area C, about 1,200 acres, is within the boundaries of a combination guest ranch and cattle ranch.

METHODS AND PROCEDURE

On each of the study areas, as many quail as possible were trapped and banded. Major trapping efforts were conducted during fall and winter of the years 1952 through 1954. A high percentage of all of the quail on these three areas were banded in the fall and winter of 1953 and 1954. On the low desert Area A, trapping was continued periodically throughout 1953 and 1954 for repeated blood samples of individual quail. In general, trapping effort was most successful during fall, winter and early spring when quail were most gregarious and were in large coveys.

Blood smears were made in the field from trapped birds. A drop of blood was taken by pricking a superficial wing vein with a sharp needle. The microscope slides were labelled with band number, sex and age and stored in dust-tight boxes until they could be taken to the laboratory for staining. The slides were then stained with dilute Geimsa's stain and examined under oil-immersion for blood parasites. Blood slides were made from all banded birds during the first two years of the study. When enough individual slides had accumulated to determine the degree of parasitism on Areas B and C, only those birds which had been taken the previous year were sampled for changes in infection.

Three age classes were recognizable by field examinations. Adult birds were identified by the lack of juvenile primary coverts and were at least one year old. Juvenile birds, with primary coverts retained from the juvenile plumage, were less than one year old but of adult size and weight. This basis of age determination was described by Leopold (1939). Young birds were those which had not yet attained full juvenile plumage and were under eight weeks of age as indicated by molt pattern.

The basis used for degree of infection was the number of parasites per 10,000 red blood cells. Immature stages of parasites were tabulated separately from mature parasites but included in the total. No slide was considered negative to infection until it had been examined for at least ten minutes. Any questionable parasites were compared with positively identified slides. No malarial organisms other than *Haemoproteus lophortyx* were found in the slides examined. *Plasmodium* organisms were reported by Campbell and Lee (1953) in New Mexico and were expected to be found in southern Arizona.

Average infection was determined for both Gambel's and scaled quail. Degree of infection was determined for Gambel's quail on each of the three study areas, each representing one habitat type. Degree of infection was compared in adults, juveniles and young quail. Survival with varying degrees of infection was calculated and compared with the yearly survival of all banded quail. On one study area individual quail were sampled repeatedly at intervals of one month or more to determine the course of infection throughout the year and during the life of the individual quail.

RESULTS

Percentage of Infection: The percentage of individual Gambel's quail infected is based upon a total of 881 slides taken primarily from the three main study areas. Approximately 1,550 blood slides have been taken during the last four years, but repeated samples of individual birds were not included for average infection unless at least one year had passed since the previous sampling. Results are given in Table 1. The average percentage of birds infected in a sample of 881 Gambel's quail from all areas was 94 per cent. Campbell and Lee (1953) found the highest infection among Gambel's quail in the Rio Grande valley of west central New Mexico. In this area 77 per cent exhibited infection of *Haemoproteus lophortyx, Plasmodium* sp. or a combined infection of both. Herman and Glading (1942) found 84.3 per cent of valley quail infected on the San Joaquin Experimental Range in California. Juveniles were 93.5 per cent infected. O'Roke (1930) in his original survey of *Haemoproteus lophortyx* infection in quail in California found infection present in most quail habitats throughout the state. Forty-five per cent of all quail examined were infected.

TABLE 1. PER CENT OF BIRDS INFECTED.

	Adults	Juveniles	Total	Per cent infected	Per cent adults infected	Per cent juveniles infected
Gambel's Quail						
Area A (Desert)	123	122	245	91	95	88
Area B (Desert Grassland)	79	34	113	97	94	96
Area C (Oak-Grassland)	331	192	523	95	94	97
Scaled Quail						
Area B	47	64	111	28	30	27

There appeared to be only a slight difference in infection of adults and juveniles in the current study. Infection in young birds was much lower. Campbell (1945a.) followed the course of infection of both *Haemoproteus lophortyx* and *Plasmodium* sp. in 12 young captive Gambel's quail. The young birds were captured at 3-5 weeks of age and kept in captivity in a large pen under semi-natural conditions. None showed infection until about eight weeks of age. During the course of the investigation, all but one developed an infection with no apparent harmful influence on the quail. In the present study, very young quail were found difficult to trap and handle. Trapping operations during the nesting season were also a possible cause of unnatural mortality. For these reasons few young were sampled. Among 18 young Gambel's quail, five or 28 per cent were infected. Apparently young Gambel's quail become infected during late summer as juveniles.

Infection among adult and juvenile scaled quail was 28 per cent from a sample of 111 birds. The majority of these birds were sampled on Area B in the desert grassland type. On this study area Gambel's and scaled quail coveys occasionally intermingled, particularly at water developments and trapping stations. The infection among scaled quail is appreciably lower than in Gambel's quail from the same area. The louse fly vector of this blood parasite was found on both species.

Degree of Infection: The degree of infection is expressed as the number of parasites observed per 10,000 red cells. Herman and Glading (1942) established an arbitrary classification intensity of parasitemia. This classification was found adaptable to the current study and is included in the tabulations in Table 2.

O'Roke (1930) classified infection by its physiological effect upon the bird. His four classes of infection were: "mild chronic, mild acute, moderate chronic and heavy acute." It should be pointed out that on a physiological basis all but a very few of the birds sampled during the current study were of the "mild chronic" type. They exhibited no discernible outward symptoms of disease and appeared normal except for the presence of the parasites in the blood.

The highest degree of infection found in the present study was 564 parasites per 10,000 red cells and was found in an adult female on Area C in the Oak-grassland. Two others were found with over 500 parasites per 10,000 red blood cells. These were the highest degrees of infection found in quail which had not been held in captivity.

Gambel's and scaled quail were kept in captivity to gather information for aging techniques and vitamin A requirements. Blood samples were taken from these captive birds when they were first confined and periodically thereafter. Under these unnatural, penned conditions, and in some cases with a lowered nutritive level, parasitemia increased. The highest incidence recorded for Gambel's quail was 1,415 parasites per 10,000 red blood cells. The average for 33 Gambel's quail which had been in captivity for one month or longer was 154 per 10,000 red blood cells. These results are included to show the possible effects of captivity on the course of infection.

The average degree of infection in unconfined adult Gambel's quail from all areas was 57 parasites per 10,000 red blood cells in a total sample of 830 infected birds. Juvenile birds had a slightly higher average infection than adults. In a total sample of 100 juvenile and 100 adult Gambel's quail from Area C during November and December, the average number of parasites per 10,000 red cells was 32 for adults and 38 for juveniles. These data showed no correlation between sex and degree of infection.

TABLE 2. DEGREE OF INFECTION ON STUDY AREAS AND VARIATION IN DEGREE OF PARASITEMIA.

Area and species	Total sample	Number infected quail	Average number parasites per 10,000 r.b.c.	Highest number parasites per 10,000 r.b.c.	Numbers of quail: No infection	1 to 10[1]	11 to 50[1]	51 to 100[1]	101 to 500[1]	501 to 1000[1]
Gambel's quail										
Area A .... Sonoran Desert	245	227	43	336	118	42	119	54	12	....
Area B .... Desert Grassland	113	110	58	370	3	6	52	43	9	....
Area C .... Oak-Grass	523	496	63	564	27	77	228	122	66	3
Penned for experiments	33	33	154	1,415	Not determined					
Scaled quail	111	20	31	512	91	7	7	3	2	1

[1]Number parasites per 10,000 red blood cells.

Scaled quail averaged 31 parasites per 10,000 red blood cells in a sample of 20 infected adults and juveniles. Apparently juvenile infection averaged slightly higher than adult infection, although the highest degree of infection was in an adult with 512 parasites per 10,000 red blood cells. An additional 91 adults and juveniles were not infected. The sample of scaled quail was too small to make more than tentative conclusions.

Each of the study areas was representative of one of the major vegetation zones included in the normal ecologic distribution of Gambel's quail. Table 2 compares the degree of infection and gives the highest incidence of infection on each area. There is a slight difference in the degree of infection between areas. This might be caused by either the differences in quail density or by the density of the louse fly vector. Area C has the highest per acre density of quail and also the highest degree of parasitism. During banding operations the greatest number of louse flies were seen on this area. Herman *et al.* (1954) recognized differences in degree of parasitism between seasons and between areas in an extensive study of bird malaria in Kern County, California.

Course of Infection: The course of infection in individual Gambel's quail was studied by repeated trapping and blood sampling on Area A. The course of infection throughout the year was plotted by counting the degree of infection for the individual quail on each repeated slide. Infection was followed in one individual from December 1952 to February 1955. Others were repeated for shorter periods. Twenty-five birds were followed for one year or longer with varying periods between recaptures. Intensity of infection apparently was not correlated with bird age or the season of the year. Infection intensity did not consistently increase with age nor did those quail with high intensities as juveniles consistently have a lower infection intensity as adults. There was no season of the year when parasitism uniformly increased or decreased in all individuals. In four different individuals blood samples showing a lack of infection were followed by positive samples taken a month or more later. A similar example was observed and reported by Herman and Bischoff (1949) in a captive California valley quail. This quail was confined under conditions in which reinfection was impossible. They recommended caution in assuming recovery of a quail once infected with *Haemoproteus lophortyx* and concluded that quail infected with this parasite remain infected throughout life. One Gambel's quail captured on Area C in the Oak-grassland had the second highest intensity class of infection in 1952 but showed no infection when recaptured during trapping operations one year later and again two years later. This one example was the only possible recovery observed.

Intensity of infection was determined by counting the number of parasites (immature and adult gametocytes) seen per 10,000 red blood cells. Both the adult gametocytes and the developing gametocytes were counted, but each was treated separately. The number of immature parasites present in the blood smear gave another index to the course and stage of infection. Herman and Bischoff (1949) graphed the course of infection in a captive California valley quail over a one year period. This quail was penned under conditions in which reinfection was impossible. The highest peaks of total parasitemia were preceded by a sharp increase in the immature-mature parasite ratio. They reported this effect was observed by earlier investigators but no references were cited. In the current study most of the repeated blood slides from the same individual were taken at too long an interval between blood slides to clearly demonstrate this effect. Occasionally individuals showed a high immature-mature parasite ratio; but when blood slides were repeated later, they showed only an average parasite intensity. One outstanding case where this effect might have been important was in an adult scaled quail which had an unsually high immature-mature parasite ratio of 504:8. The average total infection intensity for scale quail was 31 parasites for 10,000 red blood cells with the normal immature-mature ratio for both Gambel's and scaled quail less than 1:1. The immature-mature parasite ratio may be another index to the possible effects of malaria on quail populations.

Conclusions concerning the course of infection are tentative and considerable investigation will be necessary to fully describe this process. The picture of the course of infection may be quite different during years less favorable for quail.

Relation of Survival to Degree of Infection: This relation is perhaps one of the more significant points considered in this study. The degree of infection based upon the number of parasites seen per 10,000 red blood cells was compared with survival to determine if birds exhibiting a higher degree of parasitism had less chance of surviving. Data for this determination were taken on Area C in the oak-grassland type. The largest sample that included two consecutive years was made on this area. Data from other vegetative zones were not incorporated because of the variation of average infection with range type. During the fall of 1953, blood samples were taken from 302 banded Gambel's quail on this area. Of this number, 83 survived and blood samples were again taken during the fall of 1954. The classes of infection, including non-infected birds were tabulated on the basis of 1953 infection and the percentage of survival of each intensity was determied. Table 3 shows the results of this comparison.

The coefficient of correlation relating the five classes of infection intensity to per cent of survival was 0.169. The average per cent of survival of the 567 banded birds on this Area for the same one year period was 35 per cent. There was apparently no relationship between degree of parasitism and survival in Gambel's quail. As a possible comparison with the overall survival percentage, Sumner (1935) reported a 26.8 per cent survival for one year in California valley quail.

The significant effect of any parasite on a game species is the amount of direct or indirect loss caused by the parasite. Management of game birds of a high reproductive potential must concentrate on the factors limiting productivity and on the causes of unnecessary loss. *Haemoproteus lophortyx* is capable of causing death in Gambel's and other quail. Several losses have been reported in captive birds but there is only scant evidence of any extensive losses in nature. The data reported fail to show that there is any appreciable loss under normal conditions in good habitat. The study was conducted during years of good quail production. It should be continued during less favorable years to determine the possible role of this parasite in periods of rapid decrease of Gambel's or scaled quail. The high percentage of infected or "carrier" birds makes quail malaria a potential threat under unfavorable circumstances. Other phases of this problem to be considered are the effects of quail malaria upon reproductive physiology and the result of infection in very young birds. Quail malaria will remain as a threat to southwestern quail, and its effect during periods of high loss or lowered reproduction should be considered.

MANAGEMENT IMPLICATIONS

Under normal conditions in good Gambel's quail habitat, the blood parasite *Haemoproteus lophortyx* was not a mortality factor to be con-

TABLE 3. COMPARSION OF INFECTION INTENSITY AND SURVIVAL IN JUVENILE AND ADULT GAMBEL'S QUAIL FROM AREA C.

Class of infection[1]	Number in infection class Fall 1953			Per cent of survival to Fall 1954		
	Adults	Juveniles	Total	Adults	Juveniles	Total
	6	16	22	17	25	23
1-10	23	20	43	30	50	41
11-50.	68	72	140	22	22	22
51-100	25	37	62	44	32	36
101-500	12	23	35	25	17	20
Average				28	29	29

[1]Number of parasites per 10,000 red blood cells.

sidered in southern Arizona quail management. Insufficient data were obtained for scaled quail, but no obvious effects of malaria were observed in this species. No practical method of controlling bird malaria in wild quail was suggested in the literature reviewed. Campbell (1945b.) reported scaled quail in New Mexico and probably Gambel's quail used as a dusting area a spot where engine oil had been spilled on the ground. These were preferred to non-oily spots as "delousing" areas. Should control of the louse fly vector of quail malaria become necessary or seem advisable, it might be possible to achieve some measure of louse fly control by a modification of these oil dust baths. An oil solution of DDT or other insecticide in a concentration non-toxic to quail might make an effective and lethal "delousing" station.

ACKNOWLEDGMENTS

Most of the blood slides for Areas B and C were gathered during banding operations by Dr. Lyle K. Sowls, Leader of the Arizona Cooperative Wildlife Research Unit, and by Mr. Lynn A. Greenwalt, Unit Fellow. Dr. Sowls generously supplied the average survival figures for Area C from his unpublished data. Other Arizona Cooperative Wildlife Unit personnel who contributed were Harold D. Irby and James O. Keith. They did much of the parasite counting. The identification of the vector by Dr. J. C. Bequaert of Harvard University and of the parasite by Dr. Carlton M. Herman of the Patuxent Wildlife Research Refuge is appreciated. Dr. Charles H. Lowe of the University of Arizona Department of Zoology aided in statistical analysis and both he and Dr. Albert R. Mead, also of the zoology department, reviewed the manuscript.

SUMMARY

The louse fly *Stilbometopa impressa* was found to be the major vector of quail malaria in southern Arizona. It was encountered during all seasons of the year and on both Gambel's and scaled quail.

Haemoproteus lophortyx O'Roke was the only malarial parasite found in all blood slides examined. Ninety-four per cent of adult and juvenile Gambel's quail were infected and 28 per cent of adult and juvenile scaled quail had this parasite. Young Gambel's quail before the juvenile stage were infected to a lesser degree.

Percentage and degree of infection were determined for three study areas representative of the major vegetative zones of Gambel's quail habitat in southern Arizona. Differences in degree of parasitism in juvenile, adult and young were determined on the basis of the number of parasites per 10,000 red blood cells. The ratio of mature to immature parasites was also considered.

The degree of parasitism in wild birds from the three vegetative types was compared with the degree of parasitism in penned birds. The average parasitimia was raised by the unnatural conditions involved and by the lowered nutritional plane during pen experiments.

The course of infection was followed in 25 wild birds by repeated capture for blood samples over one year or more.

The correlation of survival with degree of infection was tested. There was apparently no relationship between degree of infection and rate of survival for a one-year period, as indicated by a correlation coefficient of 0.169.

Malarial infection in Gambel's quail and scaled quail is a possible decimating factor under certain conditions and its effect during periods of high loss or lowered reproduction should be considered.

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SOME NEW PHEASANT DISEASES IN CALIFORNIA[1]

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Since the appearance of the comprehensive publication, "Diseases of Upland Game Birds" published in 1937 by Shillinger and Morley, separate contributions on the same subject have been published. Most of the articles have dealt with those diseases encountered on game farms, with an occasional note being published on pathological conditions found in the wild. Generally, the publications have been considered from the aspect of the disease *per se,* and as a result the tendency has been to overlook the more important factors of their influence on wildlife management.

These factors are:

1. A game farm can be the focal point of a disease which can be widely distributed to a wild population when the game-farm birds are released.

2. The introduction of exotic species can serve as a dangerous means of disseminating a virulent disease to a highly susceptible wild population.

3. Scavengers and cover can mask the mortality rate of a disease in the wild, thereby leading to a false evaluation of its importance.

4. Diseases considered to be rare and thus unimportant provide for a cumulative drain on the over-all population.

During recent years some new diseases affecting pheasants have been encountered in California. The purpose of this paper is to present an account of these occurrences and to relate their significance to the above factors. The diseases that will be briefly discussed are avian encephalomyelitis, spirochaetosis, Asiatic Newcastle, botulism, and visceral lymphomatosis.

AVIAN ENCEPHALOMYELITIS

Jones first observed the disease in poultry of Massachusetts in 1930. Once it had been accurately described as a separate entity, epidemic tremor, as it is commonly called, was found to be widely distributed in poultry throughout the United States and Australia. Later isola-

[1]In the absence of the authors this paper was read in title.
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tions were made from turkeys, ducklings, and young pigeons. Jennings, (1954) suspected avian encephalomyelitis in a colony of Great Tits and Blue Tits under experimental conditions. The mortality rate was one hundred per cent. Laboratory animals have been found to be resistant.

The causative agent is a virus with no apparent relation to other encephalitides (Olitsky, 1939). An average incubation period of 9 to 21 days is given for chickens.

A private game-bird breeder in the San Joaquin Valley suffered an increasing mortality in his Mongolian Pheasants chicks during the breeding season of 1954. Examination of the birds showed definitive symptoms. There was an ataxia, staggering and falling over, and the pheasant chicks would sit on their haunches while at rest. A few of the birds would demonstrate a scarcely visible, rapid tremor of the head and neck when excited. Microscopic examination of the brain of several birds showed evidence of neuronal degeneration and lymphocytic cuffing, thereby indicating the disease to be of an encephalitic nature. The other encephalitides were ruled out by the Viral and Rickettsial Disease Laboratory of the State of California Department of Public Health. The disease was reproduced in the laboratory by the inoculation of infected brains intracerebrally into 25 one-day-old ring-necked pheasants from a California State Game Farm (Mathey, 1955).

The origin of the disease was traced to domestic poultry raised by the same breeder. He had maintained separate management of the chickens and pheasants. However, he did place some fighting cocks in the brooder yard of the pheasants, and the fighting cocks died immediately prior to the appearance of the first symptoms in the pheasants. The evidence pointed to transmission from chickens to the fighting cocks, and thence to the pheasants. The chickens had a low mortality, but the fighting cocks were almost a total loss. About one hundred per cent of the pheasant chicks were infected, with a mortality of 25 per cent, and another 25 per cent being sacrificed as a result of the deformity remaining after apparent recovery.

Jungherr and Minard (1942) believe that the virus may be passed through the intestinal tract, which would lend support to the theory of the chicken origin mentioned above. In addition, they state along with Van Roekel (1939), that the virus may be transmitted through the egg. In an attempt to substantiate this work, eggs were taken from the game farm toward the end of the outbreak and incubated at Davis. The pheasant chicks hatched from those eggs were normal, as were the chicks hatched at the site of the disease. This sudden abatement is characteristic of epidemic tremor (Olitsky, 1952), but unfortunately it did not afford any additional evidence for egg transmission.

Acting on the provisions of the Fish and Game Code of the State of California, and the Rules and Regulations of the California Fish and Game Commission, letters were circulated to all of the licensed game bird breeders and licensed game bird clubs to the effect that any pheasants known to have been in contact with the farm where the outbreak occurred would not be approved for release. In affect, this provided for a quarantine of the infected farm. In addition, rigid inspections were made of all pheasant farms within the state, but no other evidence of epidemic tremor was found. The breeder killed all of his stock and disinfected the premises.

SPIROCHAETOSIS

A disease well known in Europe, Asia, and Africa for many years, spirochaetosis was first reported in domestic fowl in California in 1946 by Hinshaw and McNeil, and Hoffman *et al.* (1946). In October, 1953, Mathey and Siddle found the disease in pheasants.

The causative organism of the disease in pheasants is one of several strains that have been described throughout the world (Kligler *et al.*, 1938) and is presumed to be different from that affecting domestic poultry in California. Transmission of the organism has been generally accepted to be through the fowl tick, *Argas persicus;* the common red mite, *Dermanyssus gallinae;* and culicine mosquitoes. However, in the case of the pheasants, careful search failed to reveal any ticks or mites. Inoculation of fluid from the vent of a laboratory infected duck into a chicken resulted in successful transmission of the spirochaete (Mathey and Siddle, 1955). This corroborates the results of other workers in California, (Loomis, 1953; Hoffman and Jackson, 1946; and McNeil *et al.*, 1948), who believe that droppings provide the most direct and common means of transmission.

At the private game farm where the infection occurred, approximately 10 per cent of the infected pheasants succumbed. Sick birds were ruffled with drooping wings, but erect heads. Extravasated blood was found under the back, breast, and leg skin. Minute hemorrhages were observed on the gizzard and heart. Necrotic areas were found in the cardiac and skeletal musculature. The spleen was small and pale in contrast to the spirochaetosis of turkeys which results in an enlarged, mottled spleen.

The origin of the disease was not determined. Experimental transmission to pheasants resulted in a comparable mortality to the natural infection, with similar pathological findings. Chickens, Muscovy ducklings, and turkeys were found to be susceptible to the spirochaete. A practicing veterinarian had treated the birds with penicillin and brought the disease under control.

ASIATIC NEWCASTLE DISEASE

Pneumoencephalitis, or Newcastle Disease, is a virus disease of domestic poultry of widespread occurrence throughout the United States. It has a mortality rate of from 5 to 10 per cent (Beach, 1952). On the other hand, Asiatic Newcastle Disease is an extremely virulent strain of the same virus with a mortality rate close to one hundred per cent.

A Contra Costa County bird fancier purchased some exotic species from a dealer in Hong Kong, China. The birds, consisting of Chinese and Elliot pheasants, quail, ducks, and partridges, arrived on his premises during the middle of March, 1950. The partridges were dead on arrival. Shortly thereafter two pheasants and several bantams that had been on the grounds when the infected birds were delivered died. In the meantime, distribution of part of the shipment took place through the sale of the infected birds to other dealers in Central California. The disease was further disseminated with repeated selling, and shipment of contact birds to five other states.

Mathey (1950) diagnosed the disease as Asiatic Newcastle through hemagglutination inhibition and neutralization tests, both of which were positive. The presence of a foreign disease was suspected through the unusual pathology and higher mortality. Other disease entities were ruled out by bacteriological procedures.

Hemorrhagic lesions were present in the digestive tract, being most prominent in the proventriculus. The usual cloudy air sac membranes were observed.

Notification of the presence of Asiatic Newcastle Disease brought representatives of the California Division of Animal Industry and the Federal Bureau of Animal Industry to place quarantine restrictions on all implicated premises. Consultation of the officials resulted in the decision to slaughter all contact birds as a protective measure for the poultry industry. Reimbursement was made to the bird fanciers, with the State of California and the United States sharing equally in the cost.

BOTULISM

A considerable amount of research has been devoted to botulism and much has been written on its occurrence in waterfowl. In addition, it has been identified as a disease of some importance in pheasants on game farms. However, there has been no general recognition of the seriousness of botulism as it affects pheasants in the wild. Undoubtedly, this is the result of the difficulty of detecting the remains of dead birds.

Pheasants tend to seek the dense growth that lines the irrigation canals for water and cover. There is sufficient toxin formed by the organic matter in the water to kill some of the pheasants. Prior to their death, the sick birds seek out heavy cover and die there. The mortality increases rapidly as decomposition of the carcasses proceeds. Fly larvae ingest the decaying carcass thereby concentrating toxin, and the maggots are eaten by other pheasants. Under the heavy cover, desiccation by wind and sun is negligible, so that each bird serves as a focal point of intoxication for longer periods of time.

According to Hart (1952), the disease reaches epidemic proportions during early fall. It presents a difficult problem to attack. Elimination of the heavy cover on the ditch banks would be synonomous with clean farming, which is not a good game management practice. The changing of the water levels, while a good procedure on flooded lands (Rosen and Bischoff, 1953; Rosen and Cowan, 1953), it is not practical in irrigation canals.

The disease occurs early in the fall. Pheasants released at that time are likely to frequent toxic areas. If the birds could be retained until the higher temperatures abated and the rainy season set in, it would minimize the likelihood of contact with contaminated canals. This practice might not be possible due to insufficient pen space to hold pheasants; therefore an alternative method might be the release of pheasants in areas where contact with toxic locations would be minimized. However, this would provide no protection for the wild population. In this latter case, the only remaining procedure would be to maintain sanitation of the environment by removing the carcasses serving as focal points of botulism.

VISCERAL LYMPHOMATOSIS

It has been estimated (Winton, 1955) that lymphomatosis in 1953 took a toll of 53,000,000 adult chickens worth at least $73,000,000. In a 13-year period of study at the Regional Poultry Research Laboratory, East Lansing, Michigan, 73.4 per cent of all cases of lymphomatosis were the visceral form of the disease (sometimes called "big liver disease"). It is thought by the workers at the regional Poultry Research Laboratory that the various forms of lymphomatosis (visceral, neural, and ocular) are caused by different etiological agents or viruses. These workers have found evidence that the virus causing visceral lymphomatosis may be transmitted through the eggs produced by apparently normal hens. They have found also that the disease could be produced by inoculation of susceptible chicks with cell-free inocula prepared from oral washings or from fecal material. There have been other views on the cause and spread of lymphomatosis, but the bulk of evidence seems to make the virus concept most likely.

Jennings (1954) found visceral lymphomatosis affecting the liver and kidneys of the sheldduck and a little owl, and neural lymphomatosis in a partridge. Jungherr (1939) reported the occurrence of neural lymphomatosis in game farm pheasants.

A wild pheasant taken in Butte County, California had a typical case of visceral lymphomatosis. The liver was greatly enlarged, greyish pink, and granular in consistency. Histo-pathological examination showed a massive lymphoid infiltration distributed unevenly throughout the parenchyma. No other organs were involved as judged by both gross and microscopic examination.

If this condition in the pheasant should be due to a virus as that in the chicken seems to be, it might become a menace to the pheasant population, especially on game farms. Winton (1955 A) states that ample evidence has been found that the virus may be spread through incubator debris found on hatching trays. Thus it might be that pheasants hatched in an incubator used for chicken hatching, or raised in contact with chickens, might develop the disease.

DISCUSSION

Of what significance is the occurrence of these diseases in pheasants? It is more than academic interest, or the mere acknowledgment of their presence. It is of additional importance to the outlining of their cause, the losses occasioned by them, and the limited knowledge available for their control.

These diseases serve to illustrate and re-emphasize some salient principles of game management which are too often neglected or forgotten.

A heavy concentration of animals facilitates the occurrence of disease. If those animals are held in captivity there is a greater possibility of disease appearing. The continued use of the same area for several years by large numbers of captive animals imbeds the soil with disease producing organisms, and limits the natural disinfecting action of sun, rain, and the soil itself. These indisputable facts are proved by the disease problems encountered in game farms which are rarely found in wild populations. Therefore, a game farm becomes at once the source and the means of distributing disease to the wild. It is possible that a game farm, rather than serving to increase the number of birds for the hunter, could actually decrease the available birds in the wild through the spread of a disease such as epidemic tremor.

It is obvious that the importation of exotic species may be of considerable consequence. Several diseases have been brought into this country, and their eradication was time-consuming and costly. Others have been self-limiting in that all of the imported birds died, with no survivors left to perpetuate and spread the infection. Still other maladies gained entry into the United States and became established, resulting in far greater expenditures to treat or control them both in wild and domestic stocks. The most serious consideration is that our present populations represent highly susceptible hosts to foreign disease, particularly in that they will not have acquired resistance to the exotic maladies. The result of such introductions is almost always a disastrously high mortality rate. This fact may be partially responsible for the heavy losses suffered during the short exposure to Asiatic Newcastle.

Birds that succumb in the wild may die under heavy cover, are subject to rapid decomposition, and scavengers leave little evidence. These factors obscure both the cause of death and the extent of the die-off. Beyond the evaluation of the importance of the various diseases affecting pheasants, there must be considered the advantage of knowing what is happening in the wild so as to assess all of the factors governing population dynamics. Botulism in pheasants apparently contributes a considerable influence on the population. Lymphomatosis might be just a pathological curiosity, but when it is coupled with all other such rare phenomena it does provide a drain on the numbers in the wild.

All of the above discussion would best be terminated on a delegation of responsibility. Federal and state departments of agriculture assume the responsibility for quarantine restrictions on imported fowl. Wherein do the state departments of fish and game assume responsibility? First in maintaining the health of all pheasants to be released from state installations. Secondly, in carrying out rigid inspections of all private game farms that are releasing pheasants. Finally, to maintain a satisfactory quarantine period on all game birds imported for propagation or stocking purposes. The individual biologist or field man has the responsibility of reporting outbreaks of disease in the wild and submitting specimens for examination so that the present meager knowledge of wildlife disease may be extended. This will directly support wildlife management on a factual basis.

SUMMARY

A brief description is given of five diseases recently found in California pheasants: avian encephalomyelitis, spirochaetosis, Asiatic Newcastle, botulism, and visceral lymphomatosis. A discussion of the manner in which they occurred serves to illustrate some principles that should be considered by the wildlife manager. These principles may be briefly outlined as follows:

1. A game farm can be the focal point of a disease which can be widely distributed to a wild population when the game-farm birds are released.
2. The introduction of exotic species can serve as a dangerous means of disseminating a virulent disease to a highly susceptible wild population.
3. Scavengers and cover can mask the mortality rate of a disease in the wild, thereby leading to a false evaluation of its importance.
4. Diseases considered to be rare and thus unimportant, provide for a cumulative drain on the over-all population.

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TECHNICAL SESSIONS

Monday Afternoon—March 14

Chairman: W. J. K. HARKNESS
Chief, Division of Fish and Wildlife, Department of Lands and Forests, Toronto, Ontario

Discussion Leader: J. HARRY CORNELL
Chief, Fish Division, Wildlife Resources Commission, Raleigh, North Carolina

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WETLANDS AND INLAND WATER RESOURCES

INCREASING SUMMER STREAMFLOW

KENNETH A. REID
Whitney Park, Sabattis, New York

There is something about a trout stream that has always appealed to me more than any other feature of the landscape. In the spring, when they are bank full of clear, cold water, all trout streams—and many more which then appear to be capable of holding trout, but do not—are things of beauty. But far too commonly the same stream may be scarcely recognizable two months later with its miserably shrunken volume of much warmer water trickling amongst the sun-baked rocks of its exposed bed.

As a chain is no stronger than its weakest link, so a trout stream is no better than its low-water flow. And excepting the snow-fed streams of the high mountains and a relatively few big spring streams of some other sections, the low water of summer with its attendant high temperature is certainly the conspicuous weak link in the trout growth and production chain. Critical drought flows were natural phenomena in some trout streams, but Man's thoughtless activities in the watershed have aggravated or created many more. Fortunately, by giving the streams the consideration they deserve but seldom get, Man can greatly alleviate this weakness on many streams and actually improve on the natural flow in some.

Over the years I have spent many hours pondering this problem. On most streams, the minimum flow is less than 1 per cent of the maximum; and both extremes are bad for trout. Obviously the solution is in finding means for holding back some of the water when there is too much and releasing it when there is too little. More than 20 years ago I had worked out a plan that seemed feasible, but not until five years ago when I came to Whitney Park in the Adirondacks did I have the requisite large "outdoor laboratory" to put the plan in actual operation.

Our first application of the principle was on Sperry Pond, tributary to Little Tupper Lake. Across the 100-foot-wide outlet we built a low concrete dam with an 18-inch pipe and slide valve in the bottom and two 12-foot-wide spillways, one on either side at the 2-foot elevation. When I discussed the plan with an engineer friend, he could not understand why I would not develop the full storage potential by impounding at least a 6-foot head of water. Like too many engineers, he failed to consider the important natural values which would be destroyed by the higher dam—including some 20 virgin white pine which graced the shoreline when the first white man gazed upon it.

On paper, a 2-foot impoundent on Sperry's 122-acre bosom would hold 10,628,640 cubic feet of water. Assuming a hypothetical drought of 90 days, the release of this stored head spread evenly over that period would add 615 gallons per minute or 1.353 cubic feet per second to the flow of the outlet brook. Adding this to the minimum natural flow of 200 g.p.m., or a scant ½ c.f.s. when the dam was completed in August 1951, would make the new minimum flow nearly 2 c.f.s., or four times the natural minimum (Figures 1-2).

Actually, the minimum flow has always been greater than the estimate (which did not count on a drop of rain during the 90-day hypothetical drought period). And in our first summer of operation we were so conservative that the fall rains caught us with half of our stored head still behind the dam. During the past two summers, with the benefit of this experience, we have increased the valve opening to pass a minimum of 3 c.f.s.—and during much of the summer it is considerably more. Also, we added an auxilliary flood spillway 4 feet wide at the bottom level around one end of the dam. This is closed during the summer and open during the winter and early spring as a precaution against damage to shoreline timber from high levels being maintained too long.

In July, 1953, we completed a similar dam on Handsome Pond at the head of Bog Stream. Here, with a larger pond surface and a smaller watershed above, the release of the 2-foot stored head over the

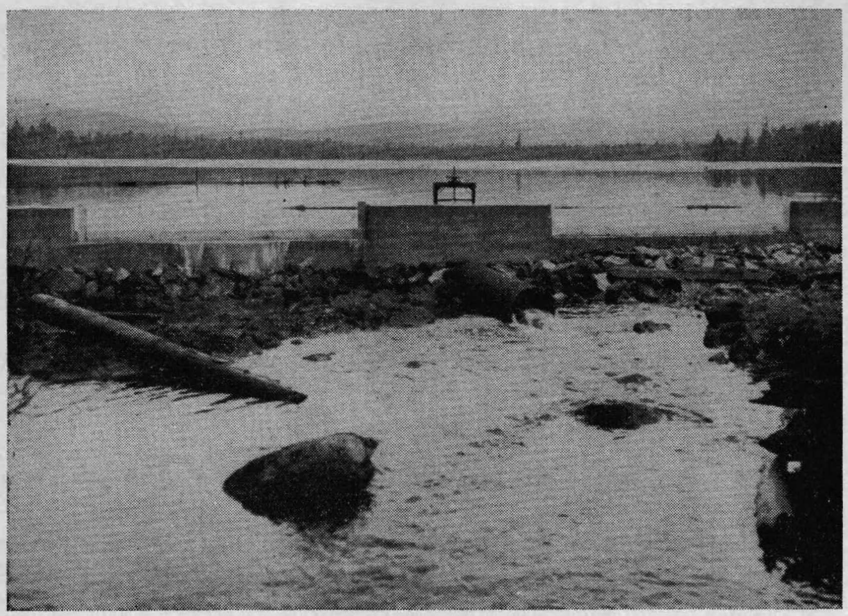

Figure 1. Sperry Pond Dam—122 Surface Acres.

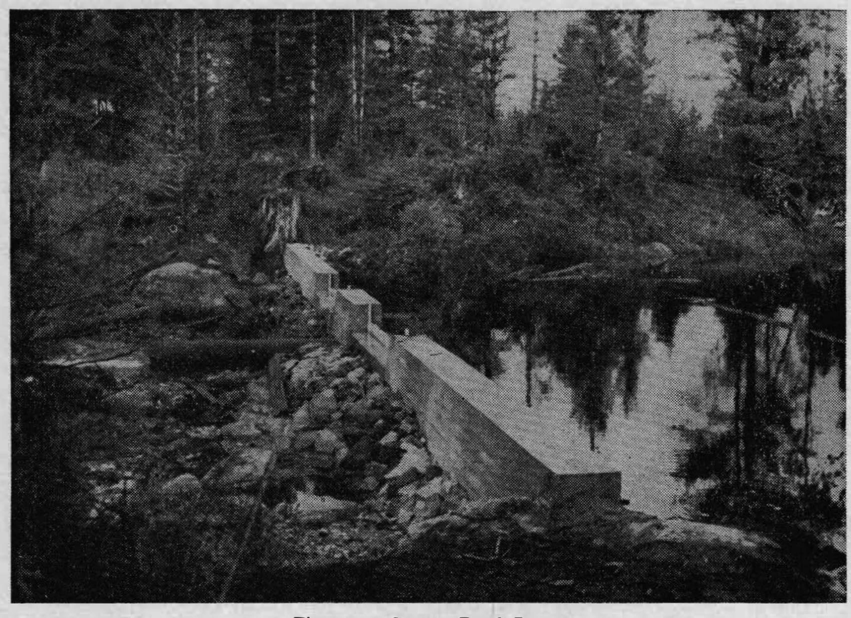

Figure 2. Sperry Pond Dam.

same 90-day period makes the new low flow six times the natural. And again, the actual flow proved to be appreciably greater than the paper calculation.

After two years of study and recording of water elevations, valve openings and volumes at Sperry, we find it seldom necessary to change the valve set all summer long. After the spring runoff, when the valve is wide open, we cut it down to approximately 6 inches. Without rain, the pond level may drop as much as four or five inches in a week, but one good rain will put it back up. The restricted opening causes the pond to act as an automatic "breather" for levelling out the flow of the outlet brook.

It is not unusual for a hard summer rain to raise the pond level as much as ten inches. But while all the rain may fall during one night, it is often three days later before the pond level reaches its maximum; and ten days more without any rain for it to drop back. During this whole period there is only a moderate fluctuation in the discharge, as the hydrostatic head in the pond rises and falls.

The third and largest unit in our program to date, the rebuilt dam on Round Lake, was completed in December, 1953. Here, with a watershed of 66 square miles above the dam, including both Sperry and Handsome Ponds, we installed two 36-inch valves in the main outlet, and two more of the same size under the flood spillway. The flow records from a gauging station over a four year period show a low of 3¾ c.f.s. and a high of 845 c.f.s.—a ration of 260 to 1. With a surface area in Round and connected Little Tupper of 3,072 acres, a 3-foot stored head (which is less than has already been flooded by the old dam) released over a 90-day period will add 51.6 c.f.s. and multiply the natural low flow by 15! The high-low ratio also changes from 260 to 1 to something less than 15 to 1 (Figure 3).

Actually the flow in the main or east outlet for some 400 yards before it is joined by the west or flood channel, varies only slightly throughout the year—from 50 to 100 c.f.s. When the volume exceeds 100 c.f.s., the excess goes out the west channel, so the east channel is subject to neither floods nor droughts.

On account of this constant volume and because the east channel had been dry for eight years since the old dam was built, we blasted and bulldozed a new channel through the mass of accumulated debris and finished the job with hand work as we thought the trout would like it. What they thought of our work was answered in early May when our flies took a goodly number of brook trout up to 2 pounds 6 ounces.

Unquestionably these trout came from Round Lake through the

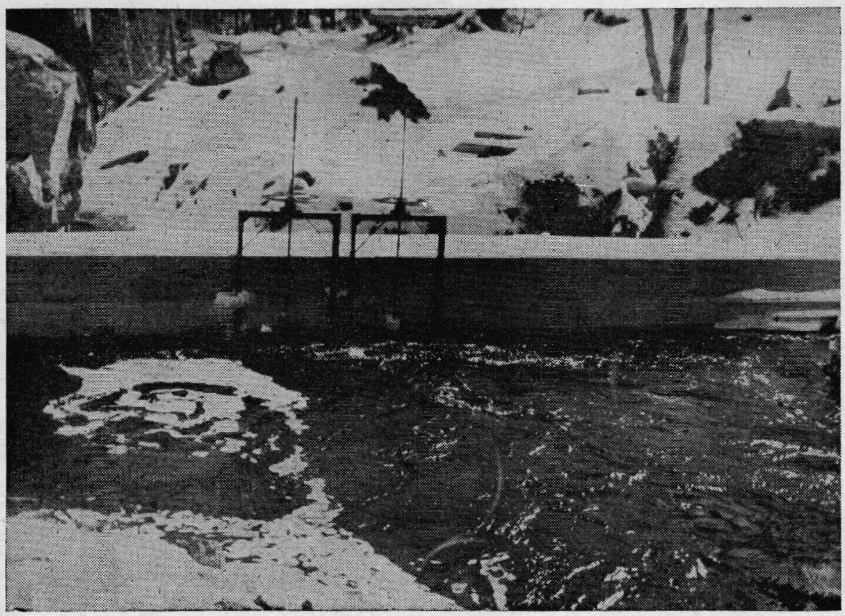

Figure 3. Round Lake Dam—Lake Surface above two 36-inch valves.

pipes, for at the lower end of the controlled stretch the water pitches over a high ledge, which would certainly discourage, if not preclude, upstream migration. The outlet pipes and valves were designed to provide two-way passage through the dam. They were located at the bottom with their downstream ends submerged more than 2 feet; trout or salmon should have no difficulty in making their way upstream, as well as down, through the 7½-foot lengths of 36-inch pipe.

The fourth major unit in our program is on 230-acre Moose Pond, tributary to Forked Lake. Here, to provide a road crossing, we made a 10-foot fill instead of a bridge across the outlet. In the bottom are two 24-inch pipes with slide valves. At their top elevation are two 5-foot open pipes to serve as flood spillways (Figure 4). The additional cost for the breather dam was simply the cost of the two valves—a little over $100 (Fig. 4).

By the time the ice goes out this spring (1955) we will have in operation our fifth major structure, a rebuilt dam on the outlet of Forked Lake operated on the same principle as the others. With a surface area of 2.2 square miles on the lake and a catchment area above of 181 square miles, this regulating dam will have an important equalizing effect on the flow of the main Raquette River.

During the current year we will install the same type of control structures on Bottle and Flatfish Ponds at the very head of the main or south inlet to Little Tupper Lake. These two will complete the major structures now contemplated above Little Tupper Lake, and by increasing the summer and fall flow of the inlet, should greatly increase the spawning potential for the native trout, and especially the recently introduced salmon.

But the opportunities for increasing summer streamflow are by no means limited to such major structures. A careful study of the topography and drainage reveals numerous smaller basins, many of which can be similarly utilized. Here are typical examples of a few which are already in operation.

On the main inlet to Handsome Pond is an old earthen beaver dam 7 feet high and over 100 yards long with over 10 acres of a ghost forest in the flowage above it. It was an effective barrier to upstream migration of trout, and its large impoundage of warm, acid water made both the pond and the outlet brook untenable for trout throughout the summer. After trapping the beaver and blowing their big house (under a nuisance permit) we breached the dam, laid an

Figure 4. Moose Pond Dam showing two 5-foot pipes for flood spillways.

18-inch pipe through it in the streambed and then filled in the breach to the original height. On the upstream end we attached a simple wooden slide cover, which is closed down to about a 3-inch opening after the spring runoff and opened wide with the coming of the fall rains. All summer long it functions automatically in regulating the flow of the outlet brook. I know trout can swim up through this 12-foot pipe, which is not even submerged but has a drop of about 5 inches into the pool below, for I watched two 10-inch trout do it.

On Dead Brook, where four years ago a travelling beaver stopped long enough to plug a narrow opening between two rock walls, which killed all the bottomland timber for nearly a half mile upstream—including 21 white pine of saw-log size—we installed a length of 20-inch pipe in the streambed, dumped a load of gravel and rock over it and equipped the pipe with the same type of wooden valve. After each hard rain the water gradually rises over the old flooded area as its volume exceeds the capacity of the valve opening, and falls again as the incoming volume diminishes. An identical installation on the outlet of a saucer-shaped treeless basin gives us another 10 acre-feet of water storage for summer use on this same small tributary.

At this point it seems proper to digress a bit on the effects of beaver dams on trout. My studies do not confirm the oft repeated claim that beaver dams are valuable for maintaining summer streamflow. Like a natural lake or pond they will exert a very slight influence in retarding runoff during the transition periods after a rain when the stream is rising and then falling, to the extent of holding temporarily a very thin sheet of water on the surface. But when this slight storage runs off and we get into a rainless period, the contribution of the pond to streamflow is nil; in fact worse than nil by the amount of increased evaporation from the pond surface. If we could educate the muskrats to eat holes through the dams in early summer and the beavers to defer repairs until the arrival of the autumn rains, they would be of some value, but barring leaks sufficient to effect drawdowns during the summer period, they are not.

The basic principle in augmenting streamflow below a lake or pond is to narrow the outlet so the water level must rise appreciably before there is any great increase in the outlet flow, thereby storing in the pond most of the water which otherwise would have raced quickly away. Taking Sperry Pond as an example, with its natural wide mouth of 102 feet, the storage from a hard summer rain was only a fraction of an inch while the freshet raced through the wide mouth in a matter of hours. With the control dam, all the water now has to go through an 18-inch pipe—usually restricted considerably by the valve—until the pond level rises 2 feet to the flood spillways. In the process, over 200-acre feet of water is stored for later release as needed.

Beaver dams commonly have much wider mouths than natural lakes or ponds. In fact, the entire dam constitutes the mouth or spillway, and this frequently extends around two, and sometimes three sides of the impoundment. That is the reason why a beaver dam is inferior to a natural pond and really worthless for maintaining summer streamflow.

On Moonshine Brook, tributary to Bog Stream, one of our secondary roads crosses just below an old beaver flow with a 36-inch pipe through the fill for the brook. To the upstream end we attached a long radius elbow with an opening cut in the bottom sufficient to pass the normal flow without impounding any water. But when a good rain comes this opening will not carry all of the increased flow, so the excess water backs up over the beaver flow until the level reaches the top of the elbow, when the full capacity of the culvert becomes available. Then the level gradually falls as the stored water is discharged automatically through the bottom opening over perhaps a week's period.

The principle of utilizing road fills for impounding water by the simple installation of an elbow on the upstream end of the culvert pipe, with the addition of a vertical riser if greater depth is desired, can be used in countless places where existing roads cross small rivulets as well as in the construction of new roads. It presents a splendid opportunity to transfer the effect of a road from the debit to the credit side in our stream accounting.

In addition to the "dry dam" outlined above, this device may be employed for permanent impoundments of two types:

1. Constant level ponds. Here, the water level is determined by the elevation of the top of the vertical riser. It has no streamflow regulating value, but can be valuable as a pond.
2. Fluctuating level ponds. Here, assuming we use an 8-foot riser, an opening sufficient to pass the normal flow might be made at the 6-foot elevation, which would then be the normal pond level. When rains swelled the volume of the brook, the pond level would rise 2 feet to the top of the riser, and then drop gradually to the 6-foot level. This compromise between the dry dam and the constant level pond provides limited benefits to streamflow.

Where either type of impoundment is located in the watershed of a trout stream, it can be made to lower, rather than raise the water temperature, by providing a small opening in the bottom of the elbow sufficient to pass about two thirds of the low water flow. Where the depth is in excess of 6 or 8 feet, the temperature of this bottom water will likely be at least 3 degrees and sometimes more than 10 degrees below that at the surface.

Existing dams with conventional over-the-top spillways can also be made to discharge cooler bottom water instead of warm surface water by the installation of a very simple and inexpensive U-shaped baffle dam in front of the existing spillway. The top should be a few inches higher than the summer pond level, the bottom a few inches above the pond floor, and the ends of the U tied in to the side walls of the spillway. The cool bottom water then passes under the open bottom of the baffle dam at the base of the U and up and over the regular spillway while the warm surface water is held back by the top and sides of the structure. Since hydrostatic pressure is the same on both sides of the baffle, it need be only a simple board structure. While such a device does nothing toward increasing summer stream flow, it can help lower summer water temperature.

Still other opportunities are presented for improving summer streamflow. For some three miles of its upper course, Bog Stream slowly winds through a great sphagnum moss bog where the banks are uniformly about two feet above the surface of the water. At several points side ponds of considerable area are connected to the stream by short channels through the bog. A series of low dams through this stretch with their crests about one foot above low water level and center openings of about four feet will automatically hold back and then slowly discharge a good many acre feet of water with each good rain. These will supplement the contributions to summer flow already made by Handsome and Sperry Pond dams.

The effect of these structures on summer streamflow is twofold:

1. Evenly distributing over the entire period the uneven precipitation occuring during that period.
2. Adding to the summer flow by the gradual release of water held back and stored from the spring runoff.

Some of the smaller structures function only in the first category, but levelling out the flow from summer precipitation alone commonly results in about doubling the low stages. The major structures with their large seasonal storage, of course function in both categories.

Unfortunately our major structures on the natural lakes do not appreciably lower the temperature of the outlet water because the dams are low and the water is shallow for some distance above them. They present quite a different picture from an artificial reservoir where the deepest water is commonly at the dam. Occasionally there is a drop of 1 degree or less, but usually it is the same. But indirectly they do effect a slight lowering of temperature in the stream below by submerging many rocks and shoals, which with a lesser flow were exposed to the sun and then radiated some of their solar heat to the surrounding water.

In the three years the Sperry Pond control has been operating, the outlet brook has changed from a shrunken summer trickle where it was practically impossible to find a decent trout until the fall rains brought in the spawners from the lakes, to a bold, live stream with a considerable resident trout population.

Such a program benefits fishing not only on Whitney Park, but equally on state lands through which Bog River flows for some five miles to Big Tupper Lake. And the new Forked Lake development will benefit the main Raquette River below. But its benefits go far beyond fishing. The whole public shares the gains from increased and more uniform summer streamflow. And, incessant propaganda of the great dam builders notwithstanding, this is of greater public importance than the more spectacular and expensive flood control programs, which have been draining the U.S. Treasury these many years.

If streams flowed a constant volume there would be no need for any of the large dams for power or flood control. In working toward a condition of uniform streamflow, we serve all these purposes and many more. For example, power companies down the Raquette River need more water for their turbines at the same time the trout up here need more water flowing down the streams. And when floods are threatening downriver is the very time we will be holding back water here.

Recognition of this fact resulted in a contract with the power company for the sale of water on this principle which justifies economically the capital investments in the structures. The terms permit gradual delivery of the water from early summer to the end of the year if need be. There will be no "dumping" of water with resultant flash floods followed by nearly dry stream beds, so characteristic of the usual power development.

If, in planning our water development programs, we will think in terms of maintaining a constant streamflow, we will automatically take care of many of the flood and power needs, and the effects will all be in the asset column. The whole secret of a sane and publicly beneficial water program is appreciation of and consideration for all values involved. We are not attempting to develop the "maximum potential" of anything—not even summer volume of the streams—

for when one does, he seriously impairs or destroys other important values. This is the essential difference between ours and the publicly financed programs of the great dambuilders, whether they be called power, irrigation, navigation, flood control or that misleading catchall, "multiple purpose."

I have long contended that hydro power developments could benefit rather than harm trout streams if they would give proper consideration to other values involved and modify plans to protect them instead of seeking only the "maximum potential" in kilowatt hours. Our developments have demonstrated that this can be done. We hope, at a time when many people are thinking and talking about upstream water control, that our operating program may serve as a working model for guiding and stimulating similar programs elsewhere.

SUMMARY

Low water and high temperature during summer are the weak links in the trout growth and production chain. Summer volume can be multiplied by using natural lakes and ponds and other depressions for moderate storage. Shoreline timber is not impaired when impoundage is under two feet for only part of the year. Merely restricting the outlet of a lake will cause it to act as a breather levelling out flow. A bottom pipe and valve is better. Road fills by the addition of an elbow and riser on upstream end of culvert function in same way. A small opening in the bottom of the elbow will draw off cool bottom water. Beaver dams with wide overflows are valueless. An open wood baffle dam in front of conventional reservoir spillway will draw off cool bottom water. A series of low dams in streams with high banks and low gradients is a useful auxilliary measure. Three low dams on natural lakes with bottom valve outlets have multiplied drought flows by 6, 8 and 15. Increased and more even summer streamflow benefits whole public in many ways beside fishing. Structures are self-liquidating from power revenues on a water release schedule suited to the needs of trout. Public relations of program are excellent. Development of one watershed of 66 square miles will be completed this year and the first two major installations on a watershed of 181 square miles will be operating. Three years observation of the first major development shows great increase in resident trout population of stream.

DISCUSSION

DISCUSSION LEADER J. HARRY CORNELL: Thank you, Mr. Reid. It appears that these ponds on headwaters offer a real possibility of improving the summer downstream flows. Unfortunately, some parts of the trout country do not have the type of terrain where ponds are possible. In the southern Appalachians the trout streams are trout streams by virtue of altitude. The temperature is maintained as a function of altitude. The fall is extremely precipitous, and ponds are impractical in most cases. The same problem exists in that part of the world, though, and it has been approached by an entirely different method. That is, the reduction of transpiration by vegetation in the watershed with somewhat comparable results.

If we might take a minute or two of the discussion period, I would like Dr. McArdle, Chief of the Forest Service to tell what has been done at Coweeta Experiment Station in solving this problem.

DR. RICHARD MCARDLE [Washington, D. C.]: I heard I was to say something about this just as the meeting started today. It has been some years since I had intimate contact with this experimental watershed, although I was Director of The Forest Experimental Station at Asheville, North Carolina when we started these experiments.

This is an area in western North Carolina of some five or six thousand acres where we get about six feet of rain a year pretty well distributed through the year. Our thought was in this area, while we were not working primarily to see what we could do about good habitat for trout, we did definitely bring that into the picture. We were trying to see how we could handle the vegetation on this area so as to increase the yield of usable water and keep it spread out over the time of year when it would be needed the most.

We cut all the vegetation, and we kept it cut back by going in every year and cutting the sprout growth that returned to the area. That stream, coming from that 33-acre watershed, the first year, increased 65 per cent in its yield of high-quality water. That amounts to about 17 inches of rainfall or about 50 million gallons of good water. It has continued to provide a large amount of water, much more than it yielded before cutting, but the duff and the material on the surface of the ground are breaking down. There is soil under the vines and briars, and so on, and whether that will enable us to have a soil there that will store as much water as the original forest soil, and whether it will provide a soil that will get the water into the ground, I don't know. But it is still producing, and we think it will continue to produce as long as that original cover on the surface of the ground stays constant, at least as good as it is now.

But on a nearby area of some two or three acres, I want to call your attention to the fact that we put in a cornfield and did just exactly as the local people did. For three years nothing happened. We got a yield of corn, but it progressively got poorer and poorer and poorer until they had hardly any kernels in the ear. In the third year that soil began to break down and it began coming down the hill, and it has been coming down the hill ever since.

On another area we cut all of the vegetation, all of the original hardwood forest growth, and we let it grow back naturally. The first year, again, we got about a 65 per cent increase in yield of high-quality usable water, but as the trees came growing back and getting taller and more of them, the yield of water began to taper off—that is, the increased yield—and nine years, now, after the experiment was started, there is only about a 25 per cent increase over what it was originally.

In another area we cut all the riparian vegetation for a distance along the stream to a vertical height of about 15 feet above the stream, which meant that in some places there was only a narrow strip and in some places quite a wide strip. That increased the yield somewhat less than 10 per cent.

In still another place we cut all of the very dense growth of rhododendron and laurel. You who have been in western North Carolina know that the growth of rhododendron and laurel is very lush. The bushes can get as tall as this room. We thought if we could cut this undergrowth, we could increase the yield of usable water and still leave a forest cover there which would develop soil conditions necessary to getting the water into the ground.

Unless you get water into deep storage, you are going to be running into trouble.

In brief, what we are trying to find out there, is by managing the vegetation on the surface of the land, to see if we can control in some way the amount of water you get and the time of year you get it, and still get good water, not muddy water, but water that is usable.

Similarly, in Colorado, we have an area where we are attempting to increase the yield of water by manipulating the snowfall. Most of the snow in that area is up above where President Eisenhower has his summer vacations, and there most of the precipitation is in the form of snow. It lodges in the trees and evaporates. It never gets to the ground. But, by getting that snow on the ground by cutting the trees, we can very greatly increase the amount of water that will come off the area. However, if you cut all of the trees, the snow will melt rapidly, and you get it at a time when you don't need it, and there are floods. But we found that by cutting the trees in strips and in other ways, we can get it on the ground and still provide shade, so that the snow melt is delayed until the middle of the summer when they need it the most.

I haven't time and you haven't tlime to listen to me tell you about all the experiments that are going on across the country in a real attempt to handle the vegetation areas in a way that will result in increased yield of water and variation in that yield for the time of the year. This is not a substitute for an artificially constructed method such as dams, but it may be something that we can use to our advantage. [Applause]

DISCUSSION LEADER CORNELL: Thank you, Mr. McArdle. I think that is a very interesting comparison of methods to accomplish the same result.

Are there any questions to be addressed to either of these gentlemen? If so, please do so now.

DR. G. E. SPRECHER [Wisconsin]: I would like to ask Mr. Reid if he recommends the eradication of beaver in areas other than where trout streams are located.

MR. REID: I'll answer that question this way. In a large natural area where man has not disturbed nature's arrangement of doing things, the beaver is no problem. But when you have lumbering which increases, and encourages the increase, of beaver, then you have a lot of sentimental damn foolishness about the beaver that gives it ultra-protection, and most of its predators are killed off including the Indian who regularly trapped him, and he just gets clear out of bounds.

From the standpoint of forest economics, forgetting trout, I have advisedly made the flat statement that in the central Adirondacks the beaver, in the last 30 years, have destroyed more timber than all the forest fires during that period. I have invited anybody to challenge that, and nobody wants to challenge it. That is something worth thinking about. In other words, I will repeat just briefly: In a natural environment, I think the beaver is no problem. I wouldn't remove him; he is part of nature. But when you give him all of the favorable things on his side, he can become a darned nuisance, and that is what he has become up our way and in many parts of the United States.

I always used to make an exception of the high country in the West, but something I saw at the head of the Big Hole River two years ago in Montana made me question even that. They practically ruined the upper end of one of those fine tributaries of the Big Hole River.

I want to emphasize this. The longer beaver are in the territory, the worse are the effects. When they are first in, they are fine little fellows, very interesting, and they don't seem to harm anything. But 10 years later it is a different story and 12 years later it is still worse.

TROUT MANAGEMENT RESEARCH IN ALBERTA

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During the past decade there has been a gradual change in the use of hatchery-reared trout. As evidence accumulated to show that survival of hatchery fry and fingerlings was poor, larger and larger trout were planted. The practice of "put-and-take" stocking is now widespread; *i.e.,* hatchery trout of legal size are released in streams to be captured by anglers the same season. However, the anglers seldom recover half of these trout, and those not captured fail to survive to the next angling season, or survive in extremely small numbers. Very little research has been done to elucidate the reasons for either the poor returns in the first season or the failure to survive to a second.

In Alberta, with its thousands of miles of trout streams on the eastern slopes of the Rocky Mountains, the problem of the proper use of hatchery trout has been a very important one. Accordingly, in 1950, the Government of the Province of Alberta and the University of Alberta jointly established a Biological Station to study, among other projects, the survival of trout in streams.

GORGE CREEK: THE TEST STREAM

Gorge Creek was selected for the experiments. It is a small tributary of the Sheep River of the South Saskatchewan River system. It is reasonably typical of the east slope trout streams; the width varies from 15 to 30 feet and the current from 1.5 to 7.0 miles per hour. It flows through a deep shale cut and forms numerous small falls and rocky pools. The elevation, at the mouth, is 4,800 feet. The stream contains a resident population of cutthroat trout (*Salmo clarki*) with a density of 800 to 1,000 fish per mile. These trout average 3.0 ounces in weight and range from 1 to 7 ounces. No other fish species is present; rainbow trout, planted at various times in the past, have failed to survive, although some of the cutthroat show evidence of rainbow blood.

A more detailed description of the stream is given in an earlier paper (Miller, 1954a).

DESIGN OF THE EXPERIMENTS

The purpose of the experiments carried out at Gorge Creek was to observe, as accurately as possible, what happened to hatchery-reared trout released in the stream. To accomplish this enclosures were made in the stream by installing wood and chicken-wire fences from one-half to three-quarters of a mile apart. The fences were maintained during the period June to September, not always successfully. Into these enclosures hatchery-reared trout were liberated. Before release each trout was weighed to the nearest quarter-ounce and provided with a numbered Petersen tag. A daily patrol of the enclosures was made, during which dead fish were removed and recorded, and living fish were caught by angling, weighed and released. In this way mortality and weight changes of several lots of pond-reared hatchery trout (cutthroat) from 1 to 4 years old were recorded.

In the summers when the screens were successfully kept in place a very accurate record of mortality was obtained. The dead fish either wash against the lower screen where they are recovered during the daily cleaning, or they lie in pools. As the water is very clear the white underparts of the trout and the red and white tags make these dead fish very conspicuous. Fish-eating birds and mammals are rare in the area and it is believed that no mortality occurred through their activities.

Mortality over the winter cannot be determined as accurately as the screens are not in place. However, it has been shown (Miller, 1954a) that trout that have been retained by screens in a certain area of stream for 30 or more days remain in the same area for at least two years. Therefore trout found in the stream during the second summer should give a reasonable estimate of over-winter survival.

In order to assess the possible effects of tagging and angling the same experiments were performed using wild trout caught in a part of Gorge Creek above the experimental area.

Two experiments were conducted using fish which had been hatched in a hatchery but held prior to planting for 10 to 18 months in a natural stream.

Finally two lots of pond-reared fish were released in a section of Gorge Creek from which all resident trout had been removed by application of "Fish Tox."

In all, mortality and weight records have been obtained for seven lots of pond-reared cutthroat trout, two lots of hatchery trout held in a stream before release and two lots of transplanted wild trout. Some of the results of these experiments have been published (Miller, 1952, 1954a and 1954b).

In the present paper only those experiments where the enclosures were successfully maintained all summer will be used. Comparisons of first summer mortalities of trout of different origins are thus accurate and reliable.

POND-REARED TROUT WITH COMPETITION

Three lots of pond-reared cutthroat trout were released, and successfully retained during the season of planting, in enclosures in Gorge Creek. These fish were enclosed with a resident population of 300 to 500 wild trout. One of these lots was of 199 3-year-old cutthroat trout, planted in June, 1950, with an average weight of 6.4 ounces. These fish were larger than the resident wild trout. Despite this fact, 35 per cent died before fall, mostly in the first two weeks after planting. In the second summer 3.5 per cent were recovered and, in the third summer, none, During the first summer a steady loss of weight occurred; the average loss was one-fifth of weight at planting after 50 days. Thereafter a slow recovery occurred and, by 100 days, the fish had almost returned to planted weight.

A second lot of 189 3-year-olds was planted in an enclosure in July, 1952. These had an average weight of 3.1 ounces, and were, therefore, of the same size as the resident fish. Before fall, 66.2 per cent of these trout died. Only 3.2 percent survived to the second summer. These trout also lost weight, but too few were recovered to obtain an accurate indication of the amount.

In July, 1950, a group of 201 2-year-olds with an average weight of 2.6 ounces was released in Gorge Creek. These fish were slightly smaller than the average size of the resident trout. Approximately 85 per cent died before fall and none survived to subsequent summers. Loss of weight was rapid; 35 per cent of planted weight was lost after 30 days in the stream and no recovery was noted.

The general results of these three experiments are the same. There is an early, heavy mortality, followed by a long period during which the survivors lose weight. One group, larger than the resident trout, began regaining after 50 days but had only achieved planted weight by fall. The behavior of the trout was the same in all three lots. Instead of taking cover, they bunched together in the current. This unnatural behavior persisted for about six weeks. The theory was formed that mortality was due to a combination of exhaustion and starvation; and that the behavior was a result of territorial activities of resident trout.

TRANSPLANTED WILD TROUT WITH COMPETITION

The mortality and weight loss of the pond-reared trout could, in the absence of full information, be attributed to the tagging and to the handling involved in recapture. To explore this possibility, a similar experiment was performed with wild trout. These trout were captured in the upper parts of Gorge Creek by fly-fishing. Petersen tags were attached, and the fish carried, in pails of water, to the enclosure. In the first experiment 152 trout were placed in an enclosure in July, 1951. These trout had an average weight of 2.6 ounces and were mostly 2- and 3-year-olds. At least 62 per cent of these trout survived to fall (actual recorded mortality was only 2.4 per cent) ; 46 per cent survived to the second summer and 29 per cent to the third summer. These results are very different from those obtained with pond-reared trout of the same size range, of which only 15 per cent survived to fall and none survived the first winter. The weight record, too, was very different. The transplanted wild trout lost weight for 30 days, but only an average maximum of 7.3 per cent; thereafter they gained rapidly and, after 40 days, were heavier than at the time of transplanting.

In 1953 a second lot of 209 wild trout was tagged and transferred to an enclosure. Only 4.3 per cent of these died in the enclosure; survival to the fall was 95.7 per cent. The experiment was not continued to a second summer. Weight losses up to 14.7 per cent occurred, but lost weight was regained after 50 days.

In both these experiments the transplanted trout behaved very differently from the pond-reared trout. Instead of schooling in the current, they dispersed rapidly in the stream; for a short time after transfer many collected at the *upper* end of the enclosure and attempted to go through the screen. They did not drift aimlessly; the pond-reared trout invariably drifted until stopped by the *lower* screen of the enclosure.

PARTIALLY STREAM-REARED HATCHERY TROUT

The transfer of pond-reared trout from the relative calm of their rearing pond to the turbulence of the stream is a possible cause of their poor showing. In order to assess this factor two experiments were carried out with hatchery fish which had been held, prior to planting, in a natural stream that contained no other trout.

In June, 1952, 58 tagged cutthroat trout were released in an enclosure in Gorge Creek. These fish were 3-year-olds, of 2.2 ounces average weight. They had spent one summer in a rearing pond, followed by 18 months in a natural stream. The retaining screens were lost early in this experiment so the exact first summer mortality is not known. However, at least 46.6 per cent survived to fall (27 were caught by angling). Survival to the second summer was 17.2

per cent. After 40 days these fish had lost an average of 8.8 per cent of their weight; after 50 days the majority weighed more than at planting.

In July, 1954, 76 tagged cutthroat were placed in an enclosure in Gorge Creek. These trout were 2-year-olds, average weight, 1.1 ounces. After one summer in a rearing pond, they had spent 10 months in a natural stream. The survival to fall was 81.6 per cent. The weight record shows a loss up to 11.7 per cent in the first 10 days followed by rapid regaining of lost weight and weights in excess of planted weight after 20 days.

These trout showed mortalities between pond-reared and transplanted wild trout. To summarize, the first summer mortalities were:

Pond-reared trout (6.4 ounces), 35 per cent
Pond-reared trout (3.1 ounces), 66.2 per cent
Pond-reared trout (2.6 ounces), 85 per cent
Transplanted wild trout (2.6 ounces), under 10 per cent
Partially stream-reared trout (1.1 ounces), 18.4 per cent

From these experiments it was concluded that all trout, irrespective of their origin, would lose weight when superimposed on a resident population; but that pond-reared trout lost more weight and suffered much greater mortalities than wild trout or partially stream-reared trout. The three levels of mortality may be correlated with natural selection as was done by Miller (1945b) and Wales (1954). Thus, pond-reared trout have been exposed to a minimum of natural selection, partially stream-reared, to a moderate amount, and wild trout a very great amount of natural selection.

EFFECT OF COMPETITION WITH RESIDENT TROUT

In all the experiments described thus far the trout were placed in an enclosure that contained the normal resident wild trout population of 300 to 500 fish of catchable size (over-yearlings). In the fall of 1953 the lower section of Gorge Creek was treated with "Fish Tox" and the trout population completely eliminated. In the spring of 1954 two one-half mile enclosures were constructed; the upper one was in an area where the resident population had not been poisoned. The lower enclosure was one mile below the point where poisoning had been begun.

The upper enclosure, called Control Enclosure, contained approximately 350 over-yearling resident trout (a marking and recovery experiment gave a population of 339). The lower, called Experimental Enclosure, should have contained no resident trout, but evidently a few either escaped the poison or drifted down during the winter; 10 were caught and removed during the summer. This is an insignificant number compared to 300 to 500 trout which this area formerly contained.

On June 22, 1954, 200 hatchery-reared 2-year-olds, average weight, 1.5 ounces, were released in the control enclosure. Each fish had been weighed and tagged before planting. On the same day 200 hatchery-reared 2-year-olds, average weight, 1.03 ounces were liberated in the experimental enclosure. These had also been individually weighed and tagged before planting. These two groups of trout were one mile apart in the same stream; they were, therefore, subjected to identical environmental conditions; the only difference was the absence of a resident population in the experimental enclosure.

Control Enclosure Results. The trout began dying the day after planting; after 10 days, 73 had been found dead; after 20 days, 97 had died. At the end of 60 days the mortality rate had become low and a total of 109 trout (54.5 per cent) had been found dead. This mortality figure agrees well with the findings, recorded above, for hatchery trout in previous summers.

Weight checks were obtained on 164 of these trout after planting. These show that the trout lost weight for at least 60 days. The maximum loss recorded was 13.1 per cent. These observations again confirm previous findings for hatchery trout with competition. The detailed records are shown in Tables 1 and 2.

Experimental Enclosure Results. The behavior of the fish after planting in this enclosure was in sharp contrast to the usual actions of hatchery fish released in a stream. Instead of bunching in the current and drifting more or less rapidly downstream, they dispersed immediately. The day after planting none was visible, despite the clear water and the conspicuous red and white tags. Angling revealed that they were occupying the full length of the enclosure.

TABLE 1. COMPARISON OF MORTALITIES OF HATCHERY-REARED 2-YEAR-OLD TROUT[1] PLANTED WITH AND WITHOUT COMPETITION FROM RESIDENT TROUT.

Days after planting	Mortality in each 10 day interval (per cent) With Competition	Without Competition
0-10	36.5	11.0
11-20	12.0	1.0
21-30	3.0	1.5
31-40	1.0	0.5
41-50	0.0	0.5
51-60	2.0	0
over 60	....	0.5
	54.5	16.0[2]

[1]200 in each case.
[2]2 fish found dead, but time of death unknown.

TABLE 2. COMPARISON OF WEIGHT CHANGES IN HATCHERY-REARED TROUT PLANTED WITH AND WITHOUT COMPETITION FROM RESIDENT TROUT.

Days after planting	Weight expressed as percent of planted weight With Competition	Without Competition
0-10	98.2 (84)[1]	104.6 (21)[1]
11-20	98.2 (28)	116.6 (2)
21-30	88.9 (9)	103.6 (5)
31-40	93.5 (16)	130.3 (8)
41-50	99.2 (14)	127.3 (14)
51-60	86.9 (9)	163.0 (10)
over 60	103.0 (4)	......

[1]Figures in parenthesis are numbers of fish used to compute the average.

After 10 days in the stream 22 of the trout had been found dead. Thereafter, mortality was low and after 60 days a total of 32 fish (16.0 per cent) had died. This is less than one-third as many as died in the control enclosure. The weight record is even more remarkable. All of the 60 fish for which weight checks were obtained showed gains. After 50 days in the stream these trout weighed an average of 163.0 per cent of planted weight. The detailed records are shown in Tables 1 and 2.

When, three weeks after the experiment began, so few of the experimental enclosure fish had died, it was feared that many had escaped from the enclosure. There was no reason to suspect that fish had escaped except the difference between the results in the two enclosures. It seemed advisable to check the results by adding more fish to the experimental enclosure. Accordingly, on July 16th, 24 days after the experiment began, a second planting of 121 hatchery-reared 2-year-olds was made. These fish had an average weight of 1.6 ounces. A very careful watch of the screens ensured that none of these fish escaped. Twelve of them died in the first 10 days and only four in the succeeding 50 days, a total of 13.2 per cent. Weight checks were secured on 73 fish. These all showed gains, per 10-day interval, as follows: 2.8, 1.6, 6.3, 10.5 and 21.6 per cent in excess of planted weight.

These results confirm those of the first planting. It is interesting to note that the weight gain was less, presumably because of competition with the trout of the first planting. The two plantings together totalled 321 fish, which is not in excess of the normal resident population.

Discussion and Conclusions

The experiments conducted with hatchery-reared fish, superimposed on a resident population, indicate a general inability to survive. This poor survival is to some degree a function of size at least during the season of planting. Thus when the hatchery trout were larger than the resident trout, 65 per cent survived the summer. When the hatchery fish were the same size or smaller than the resident fish survival was only from 15 to 34 per cent for the first summer. More than half the first summer mortality occurs in the 10 days following planting. Thereafter, mortality is light, but loss of weight continues, particularly in the smaller fish, which fail to begin gaining during the summer. Survival over the first winter appears to be negligible for all sizes.

Wild trout superimposed on a resident population exhibit the same reactions as the hatchery fish, but to a very much smaller degree. Mortality over the first summer is less than 10 per cent, and survival over the winter is the same as in the resident population. Weight losses occur, but these are less than for the hatchery fish, and the lost weight is regained after 30 to 50 days.

Trout, raised in a hatchery, but held for 10 to 18 months in a natural stream before planting, exhibit weight losses and mortality greater than transplanted wild trout, but less than hatchery trout fresh from the rearing pond.

It is obvious from these findings that the same forces act against all trout superimposed on a resident population in a stream. The ability to resist these forces is greatest in wild trout and least in pond-reared trout. A reasonable assumption is that natural selection accounts for these differing abilities. The wild trout are survivors of a much more rigorous struggle for existence than are the pond-reared trout; trout exposed to natural forces for 10-18 months before planting have undergone an intermediate degree of selection.

Hatchery-reared trout, planted in a section of a trout stream from which the resident population had been removed, exhibited very high survivals and gained weight from the day of planting. These fish were subjected to the identical environmental forces; only competition with resident trout was lacking. The weight loss occurring in all superimposed trout must be entirely due to competition. The mortality cannot be attributed solely to competition, since 13 to 16 per cent died in the absence of competition. This percentage must die as a direct result of physical forces in the stream. The additional 40 per cent that die when competition is present presumably also succumb to physical forces, but only because prior occupancy of all the niches in the stream forces them to remain out in the current. Death is possibly from exhaustion, but no biochemical tests have been performed.

The very low survival over the first winter is probably a result of weight loss. As winter is the lean season, trout in poor condition have a small chance of surviving. The poor condition is, of course, a result of unsuccessful competition with resident trout.

Efforts to train pond-reared trout to take cover before they are released would seem to be based on a correct understanding of the principal cause of mortality. However, it is by no means sure that such trout would be rugged enough to put their training to effective use when the stream is fully occupied by a vigorous wild population.

When trout are superimposed on a resident population they normally do not remain in the place where they were planted. Wild trout move purposefully away, in these experiments, upstream. Hatchery trout drift in the current and hence move downstream. The failure of anglers to take greater numbers of trout stocked on a put-and-take basis is probably due to two things: a high mortality in the first days after planting and the drift of the planted trout.

TROUT MANAGEMENT POLICY IN ALBERTA

The findings reported in this paper plus the general experience with hatchery trout in other areas of North America have influenced the management policy in Alberta. First the administration conducted a campaign to acquaint the angling public with developments. Then the planting of trout in streams was discontinued. The only exceptions are where it is considered desirable to introduce a new species, or where suitable trout waters (above falls or dams) contain no trout population.

The size limit on trout was abolished in all provincial waters. This was because, over the province as a whole, there is no consistent relationship between size and maturity. Further, high natural mortality rates in streams rob the public of too many fish when a size limit is enforced.

In one large drainage area seasons were abolished and trout fishing is legal all year around. In this same area the smaller tributaries are alternately opened and closed to fishing. This is because streams that have been closed for one year provide much better angling, for larger trout. It seems likely that this scheme will ultimately be extended to all the drainage areas.

Hatchery trout are being planted mostly in lakes; the lakes planted are those which contain no breeding trout populations. Good returns have been achieved in the first two or three years from these lakes. Thereafter the uncaught large trout seem to prevent effective survival of subsequent plants. It is likely that a system of regular poisoning every three years may have to be introduced.

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DISCUSSION

MR. J. P. CUERRIER [Canadian Wildlife Service]: We do have the responsibility of managing some of the waters. I was wondering, if following the investigations of cutthroat in the streams, how much the stocking of fish has changed today compared to what it was 10 years ago. What is the percentage of decrease in the distribution of fish from the hatcheries by the Provincial hatcheries—the total stocking of cutthroat trout?

DR. MILLER: As a result of the investigations which I have described this afternoon, and also some others which I have not mentioned here, we have modified our policy on hatcheries almost totally, and have now completely abandoned the stocking of waters which contain a resident population of trout.

We confine our stocking to areas where we wish to try to introduce a new species or to areas which do not at the present time contain trout, or to a few watersheds where there is no natural reproduction.

DISCUSSION LEADER CORNELL: There are some parts of the world where put-and-take stocking is something of a necessity.

MR. B. W. CORSON [New Hampshire Fish and Game Department]: I would like to ask Dr. Miller if he takes into consideration the variants that exist in hatchery trout. By that I mean we all know that in some places trout are more tolerant than others. Is there any history of stocked trout survival rate? That is, where the fish have been subject to diseases or have been in fast-moving water, or whatever circumstances existed in the hatcheries from which they came.

DR. MILLER: We tried to eliminate those variables by using only wild stock; that is, the eggs were taken from wild trout, so that we have no history of previous generations of hatchery culture. We thought, in that way, we would be working with the most favorable hatchery stock you could work with. It is just one generation removed from wild stock.

DISCUSSION LEADER CORNELL: Based on your experience, what are your recommendations for those places where put-and-take fishing is necessary?

DR. MILLER: I realize that streams degenerate to the point where they cannot support a resident trout population. Certainly, not a self-containing one, and, there, obviously the only way you can obtain angling is by put-and-take stocking. I think the method that is now widely used, there is no reason to question; that is, a number of small scattered plantings throughout the open season. Then, the bulk of the fish will live for a week or so following that planting, and if you make the planting small enough and often enough, there is every reason to expect a very good harvest.

Of course, that technique of management presupposes an extremely heavy and active population of anglers.

MR. JOHN C. HERMAN [Pennsylvania]: I would not like to open up an entirely different field of discussion here, but I wondered if Dr. Miller had made any experiments with supplemental feeding when competition has been increased by stocking.

DR. MILLER: No, sir. We have not tried that. For the fish that survive that first ten-day or two-week period, it might be quite a sensible thing to undertake. But, since some 50 to 60 per cent of the planting dies within the first ten days after introduction, I don't think it is connected with food. It is too brief a period for them to have suffered very much from a lack of food. I think a trout can go for 30 days or so before it begins to suffer too much from the effects of starvation.

I believe it is the straight, physical forces of the environment which are killing them in that early period, rather than shortage of food. But, certainly, for the 30 or 40 per cent which do manage to exist after that first period of mortality, then I would think supplemental feeding would equip them to perhaps come through the winter which they don't do at the present time.

MR. HILBERT SIEGLER [New Hampshire]: One of our biologists in New Hampshire is carrying on an almost identical study to yours, Dr. Miller, and his results are almost identical. This study is being carried on in a mountain stream, and we have been wondering how to find out what is happening to the fish when they die. Do you find that there is mortality?

DR. MILLER: Our mortality which I have quoted to you is on the basis of dead fish recovery. It is the minimum mortality. Undoubtedly there are some we miss, but we are fortunate in the experimental area we have chosen in that there are no fish predators so that we know there is no hidden mortality in the form of birds or animals. In this clear stream these fish that die either wash against the screen or lie at the bottom of the pools where their red and white tags and white bellies make them very conspicuous. We are quite certain that we recovered all the fish that died, or very close to all.

MR. SIEGLER: This evidently was not in a rubble stream, then.

DR. MILLER: No. It is a stream that is cut through a very deep gorge. The bottom is quite clear and smooth. It is a very ideal one to work with for that purpose. We have started another series of experiments in a different kind of stream, using brown trout. These are choked with trash and rubble. We have run into that type of problem. We don't know what happens to the fish. They just disappear.

MANAGEMENT OF QUEBEC TROUT LAKES

GUSTAVE PRÉVOST.

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SUMMARY

The concept of fish conservation is quite different today from what is was in the past. Today we try to make maximum use of our renewable fish resources and not *let fish die of old age.* So the tendency is to liberalize fishing policy, instead of restricting it.

This policy has of course to be built on solid grounds, and to get maximum results we will have to consider each lake individually, the way an agronomist manages a portion of land. Seeing how vast is the number of lakes in this province, we realize that it is not going to be easy to put the new policy into effect. But it should be done.

When that time comes, many of the present regulations will have to change. For instance, instead of a day bag limit there will be a year bag limit. Because what is important is the annual crop. And this in turn will be largely determined by what our research is bringing to light about size limits, stocking, food, dams and pollution.

To prepare the way for these changes, biologists must bring these new concepts home to the public, doing so through newspapers, radio, television, etc. Anglers then will have a clear idea of new methods that will benefit the sport, and will ask the Government to adopt them.

The ultimate aim of good fisheries management is to keep and improve our renewable fish resources for the benefit of our anglers and fishermen of not only the present generation but also of future generations.

We must first point out that the term management is used here in its broadest sense. It implies not only the administration of fisheries but also fundamental and applied research. Today it is well recognized that we cannot soundly manage our natural research. Today it is well recognized that we cannot soundly manage our natural resoruces without intensive research. We must also take account of the human element. It has been well said that success in fisheries management often depends more on management of people than on management of fish.

The purpose of this paper is to let you have a glimpse at our organization; also at the problems we have to deal with and the results obtained. In going along, we will express opinions on ideal methods, hoping to evoke the comments of this assembly. For the sake of clarity, we have limited ourselves to the management of speckled trout lakes.

For those who are not familiar with the term speckled trout, we may add that the scientific name is *Salvelinus fontinalis fontinalis,* and the fish is also known in popular language as brook trout, red trout, squaretail and salmon trout. We must not however confuse this fish with the so-called Quebec Red Trout which has a forked tail: on the sides, no blue haloes around red spots; on the back, no spots at all.

Actually the speckled trout is not a trout at all, but a char. However, the name of trout has been familiar for so many years that it is certainly not going to be lightly changed in popular language. Let us call it speckled trout for the sake of clear understanding.

It is the fish most anglers in the Province of Quebec go fishing for. It is found practically all over; in fact it inhabits *thousands upon thousands of lakes in an area of 600,000 miles.* The exact number of lakes is unknown since the northern part of Quebec has not yet been fully mapped. The estimate of water surface is 70,000 square miles. Our lakes can be divided into eight categories: park, reserve, club, outfitters, private, public, municipal and finally fish and game associations' lakes.

To fish those lakes everybody must buy an annual permit and obey the general law. Each organization, however, may establish its own more restrictive regulations. Let us see how everyone makes out.

1) *Park Lakes* admit the general public at a nominal price, and anyone who likes to have hotel accomodation and guides can have them at the commercial rate. The area covered by the parks is approximately

10,000 square miles, and there is a strict regulation for the exploitation of park forest, as well as for fishing.

2) In *Reserve Lakes* anyone can fish without charge. The area is also 10,000 square miles. Regulations for the exploitation of the forest and fish are less rigorous than in the parks.

3) *Club Lakes* are leased from the Government by sportsmen who have obtained an exclusive right of fishing. The area covers 22,000 square miles and their number is 1,500. Territories are allowed only to people who are ready to take good care of them.

4) *Outfitters' Lakes* are leased from the Government by commercial people who can make a profit out of them by organizing fishing trips for the sportsmen, to whom they supply lodging, food, and guides.

5) As to *Private Lakes,* in the past the Government used to sell lands with lakes and there remain a few privately owned lakes, the owner enjoying exclusive fishing rights. The Department no longer sells lakes, however.

6) *Public Lakes* are open to anybody without charge.

7) As to *Municipal Lakes,* civic authorities, for the purpose of keeping public lakes in good shape, have taken under their control public lakes located in their area and have established rules for the benefit of the population. They are open to anyone at no charge, or in some cases at a nominal charge. Municipalities are encouraged to get more public lakes under their control.

8) In the case of *Fish and Game Associations' Lakes,* active associations have the same setup as municipalities but cover a larger area. The associations are also encouraged to get more public lakes under their control.

These numerous categories of lakes complicate management, especially when we know too that human settlements with a population of 4,000,000 occupy approximately only 1/20th of the whole territory. This means that the problems encountered vary greatly according to the districts concerned. Let us see how we are organized for the management of all these lakes.

Organization

The Government of this Province has realized so well the importance of research that twelve years ago it founded the Biological Bureau and asked the University of Montreal to supply the necessary laboratories so that our scientific staff could be in close contact with the professors. It was hoped that good results might result both ways from such association. And so it has proved in fact. The experiment has been very successful and we can say now that it is desirable to promote such an association whenever possible. Those of the Biological Bureau who may be inclined to be too practical are checked by the professors; those among the professors who may tend to be too theoretical are brought back to earth by the scientists of the Bureau. The two-way influence works slowly and often inperceptibly, but it works every day of the year. This state of things brings about a better understanding between laboratory men and administrators.

The Biological Bureau counts 10 permanent scientists and takes on 10 temporary ones for the summer months. We have a bioligical station in the Mont Tremblant Park, 80 miles north of Montreal, where we can study lakes right on the spot. There we can accommodate 25 persons or more. Any Scientist from any country who wishes to carry out scientific studies in our district is welcomed. To such visitors we supply free room and board plus part of the equipment required for their work. The only thing we ask from them is a report of their observations. We firmly believe that such a station is the backbone of any good management program, but it should operate

Figure 1. Mount Tremblant Biological Station

the year around with at least 20 scientists. There might well be not just one but many such stations in the Province. Some stations should be established in districts which have not been touched by civilization, so that we may know the original conditions and then see the effect of different factors brought by civilization.

And we should organize and even construct a number of experimental lakes. We should take account of everything we put into these man-made lakes, keeping constant check. These lakes should be arranged in such a way that we can empty them any time for observations.

Some of our biologists have a territory to look after and, at the same time, are in charge of trout hatcheries. So the hatchery is not only a producer of fish but also becomes an experimental center for all sorts of aquatic problems.

We know that in some countries biologists have complained of the poor results of restocking lakes with some species of fish and have recommended the closing of hatcheries. Before making such recommendation it is important to ascertain whether the hatcheries could not be used as experimental centers; quite often it has been demonstrated that it is better to keep what we have and transform its use, than to risk losing everything.

It is good to recall that even with all the good work of biologists, we will not go very far in managing trout lakes if we do not know how to manage people. Therefore we have a public information man, not for the sake of giving credit to individuals but to help educate the public as to our problems, so that everybody may know how to help us solve those problems. It is urged that every angler become a member of a Fish and Game Association, because such groups, well informed through their Federation, are able to formulate reasonable requests, which their Government is well disposed to grant.

So the principles of sound management must be told to the public through books, newspapers, radio, television. We do not spend enough time on those means of education. We are likely to take it for granted that everybody knows the importance of our work; and if our appropriations are not high enough, we recriminate. Very often it is our own fault; we have not made our work known; have not descended from our ivory tower. Now that we have an idea of the methods of managing both lakes and people, let us see the problems that we usually have to deal with.

PROBLEMS

1) *Bag limits.* Present legislation permits a maximum catch of 25 trout, but limited to a total 15 pounds a day. This number has been

determined by sportsmen as a limit to the human fishing appetite and is not based on any reliable data. We must admit however that for the time being we cannot do much better, but every one of the organized groups already cited is entitled to set a lower mark. They may not set a higher mark, however, unless they can obtain a special permit for an experimental purpose.

The general law cannot fit all particular cases; it may even do harm in some instances. Let us take an example.

Suppose that after a good study of a lake we decide that 1000 fish should be the annual crop. The present law by its arbitrary limits does not prevent a higher catch since anglers may take 2,000 by the end of the year. Such overfishing will damage the lake. Suppose on the contrary that only 200 fish are taken out, the lake would have not given its full results, and there will be a loss of fish because many fish will die of old age. It is therefore urgent to establish the annual crop of each individual lake, doing so after the manner of a farmer planning the exploitation of a section of land. Then instead of a day bag limit we shall have a year bag limit.

2) *Increase of population.* a) Restocking: Very often, as soon as a club is formed, the average angler may say "Let us improve the fishing," adding, "Restock it." For he thinks that by putting in more fish he will have better fishing. He does not realize that the food is limited as in a pasture field, so that an overpopulation of fish may do the lake more harm than good. And this is the hardest thing for a biologist to drive home with many an angler. It is not a simple matter to the biologist, either.

Since no one knows the actual population of a lake, nor yet the factors that affect the population, it is very difficult for the biologist to do what every scientist wishes to do: make a decision based on facts. Many an angler is surprised to hear a biologist say "It may be so, but . . . "; he thinks we should have the answer down pat, for we are biologists, are we not? But more and more it is being realized that our problem, which looks so simple, is really very complex.

Today most restocking is done on request from anglers. But one day we hope our work will be so well organized that we shall be able to keep tab on every lake's state and needs, and when it is time to restock we shall automatically know it and do it.

To determine the timeliness of restocking, we have to rely on very scanty information. We try to find out the number of trout caught per angler per hour, and if it is less than one we recommend restocking provided there are no detrimental factors other than lack of fish.

We have found that generally, in our province, a lake can be stocked with speckled trout so long as it is 15 feet deep and there are no undesirable fish.

In order to work with as much data on hand as possible, we urge the different organizations to keep statistics. Most important of all, we like to know the number of fish per rod-hour and the average weight and length of the fish. With such help we can do a more logical management job, one that is bound to be more satisfactory to all concerned.

Since most lakes are remote from the regular road system, we restock by dropping the fish from an aircraft, a method which, originated in this province in 1934, has proved highly successful and been adopted in other countries. Most restocking is done in the fall.

What size of speckled trout do we plant? In rehabilitated lakes close to civilization we restock with 8- to 10-inchers. We use the put-and-take method, which is costly but gives excellent results. We plant trout fry in lakes with no other fish in them, virgin lakes, so that the little fish are in no danger from bigger ones; and when there are already trout in the lakes we plant fry in the inflowing streams.

Restocking will always be necessary for lakes near to civilization, because of the large amount of fishing done. But in remote lakes where fishing is good now, we should be able to keep it good without recourse to restocking.

b) Other means: We must also consider spawning facilities, reduce predator fish and predator mammals, increase food, and so on — all rational means about which we know so little in their practical application.

3) *Closed season.* The dates of the closed season are supposed to correspond to the spawning time of the fish. However, we have found that in some districts spawning comes earlier than the law has provided for. We really ought to determine the spawning time for each lake, if we wish our closed seasons to be effective. And then again, it isn't always necessary to have a closed season; we have found this to be the case as regards the smallmouth black bass in certain districts.

4) *Size limits.* Any speckled trout smaller than 7 inches must be returned to the water. This is based on the fact that so small a fish would appear to be too young to have spawned, and so by removing it from the water we are cutting off a source of future fish. But in many lakes, for example in the Laurentides Park, speckled trout may average 5 to 6 inches at the adult stage. Every time you catch a fish you break the law, and it is a good thing the Park fishwardens are understanding on this point and let you keep your fish. But we find the same undersized adults elsewhere in the province, especially in

cold-water streams. There should be a size limits for each lake and stream.

5) *Improvement of size.* Very often people want bigger fish in the lake and ask what we can do about it. There are many things we can do to produce larger fish. It would appear that a fish remains small or grows large according to food supply, heredity and such environmental factors as temperature. We have found by actual test that hatchery fish grow bigger on the same food than do small-size fish that have been taken from a lake. Also, when we turn the tables and put the hatchery fish into the lake, they grow bigger there too than do those already in the lake, and so they boost the average size. We need more research to ascertain the effect of each factor.

What about fertilization? It has succeeded in the warm-water lakes of the United States, but it has yet to be really proven that it can yield good results in our acid, cold-water lakes of the north. We are still working on this subject.

Other helps include the introduction of plants, fallen trees and the sort of tiny invertebrate creatures on which fish like to feed. To deal with an overpopulation we may have to resort to overfishing, to the destruction of eggs on the spawing beds, or to a partial kill with rotenone. We never plant bait minnows but, on the contrary, keep away from them as a pest which has done a lot of harm to many of our speckled trout lakes.

6) *Coarse fish.* Neither minnows nor any coarse fish should be introduced blindly. Millions of dollars worth of good fishing have been lost this way in our province. Fortunately we are able to rehabilitate any lake containing undesirable fish by killing all the fish with rotenone and starting afresh with only trout. By this means we can restore good fishing in a short time. Of course we must take precautions against reintroduction of coarse fish. The way we operate today permits us to kill all fish even bullheads. To restore all lakes to trout fishing would cost millions of dollars, but there is no doubt that if we want to bring fishing to the people's door we will have to perform many more operations like this.

There is another way that could be tried for small lakes. Intensive continuous fishing might decrease the coarse fish. But unless it is continuous it is not likely to succeed. More research is necessary in this field, in order to find out at what level coarse fish are detrimental to trout.

To prevent coarse fish from spreading we will have to educate the people. Since this cannot be done in a short time, the Federation of Fish and Game Associations is considering asking the Government to forbid not only fishing with minnows but even minnow transportation outside the St. Lawrence River, in order to be sure of stopping the plague.

7) *Dams.* There are many dams installed at lake outlets. Most are used for logging. Nowadays more and more dams are being built by beavers. What are the effects on trout fishing? It is very difficult to give an answer. Each lake has to be studied individually.

Fishways are sometimes recommended to facilitate migration from one lake to another. In general practice however, fishways are not very well cared for and do not function properly. Even if they worked properly, do we know how much the fish use them? If they use them, what are the real results? Very often we have had complaints that fish were accumulated below dams. When we asked to be advised when this was actually happening, so we could go and see for ourselves and use electrical fishing methods to take the fish and restore them to the lake, we never got any answer. So we do not really know the effect of logging dams. Of course, when dams let no water at all pass, the effect is bad. When there is no regular flow, many of the food organisms are killed and even the fish. We have had only a few cases reported and they had to do with hydro-electric dams which, unless well managed, are conservation's worst enemies.

But there may well be a silver lining here too, because dams create great water basins in which it should be possible to develop very good fishing.

8) *Pollution.* We have industrial and domestic pollution, especially in the St. Lawrence and Ottawa Rivers, and the problem is being vigorously worked on. Very few trout waters are polluted, however. But here again we need to take precautions. In mining districts streams and lakes can become fouled with industrial waste. In areas of forest exploitation, the banks may become eroded if denuded of their trees. In these areas, too, bark torn off in logging operations accumulates on the bottom, and we have not yet pushed research to the extent of finding out to what extent this sort of contamination can be tolerated by fish.

We could go on and talk of many more problems. But this is just to give you an idea of our main problems. And of course in our management in the province of Quebec, besides speckled trout, we have to look after other sport fish, like muskie, pike, smallmouth black bass, walleyed pike, lake trout, rainbow trout, Arctic char, brown trout, Quebec red trout, Atlantic salmon, landlocked salmon and more recently grayling and splakes.

All that has been said is only a summary of a vast subject. But it can be summarized still further by saying that the biologists have a great deal to do, in scientific research and practical work, towards conserving sport fishing as a great natural heritage with which our province has happily been endowed. But the biologists cannot do it all by themselves. They need the understanding and cooperation of anglers, especially anglers' associations; in fact biologists need wider and ever wider recognition of the importance of their task, which is

Figure 2. Laurentides Hatchery, St. Faustin, P.Q.

that of making it possible to get the most out of our great fishing harvest but without in any way impairing or damaging the capital stock from which the harvest derives.

In terminating, I should like to observe that the conservation of our natural resources is no local matter. It is a continental question. And so it is encouraging to meet here in the Mount Royal Hotel, Montreal, with representatives of our two great democracies, Canada and the United States, all in fundamental agreement on our basic need.

DISCUSSION

DISCUSSION LEADER CORNELL: Thank you, Dr. Prevost.

I think to say these biologists have a major problem ahead of them may be the understatement of the week. We have 10 biologists to determine the desirable harvest from 70,000 square miles of lake, and then establish an adequate harvest on that basis. I am sure they are going to be busy men.

Are there any questions to be asked of Dr. Prevost at this time?

DR. G. E. SPRECHER [Wisconsin]: Dr. Prevost, you say that any lake can be stocked with speckled trout that is 15 feet deep and has no undesirable fish. Does temperature make any difference?

DR. PREVOST: I am pleased that you asked that question. In most of the lakes, as long as you have 15 feet, you have a temperature cold enough in our waters. It may happen that in some lakes in the south you have to take temperatures, but usually we do not take any temperatures of the lakes as long as they are 15 feet deep and they are in the Laurentian Mountains.

DISCUSSION LEADER CORNELL: Are there any other questions for Dr. Prevost to answer at this time?

MR. B. W. CORSON [New Hampshire]: I would like to ask how Canada operates their reclamation program to insure, or guarantee, the kill of bullheads.

DR. PREVOST: You want to know how we do it? It is pretty hard to demonstrate it here. [Laughter] I will just say, first, that we have trouble with the bullheads. Most of the time the bullheads are in very shallow lakes. As a matter of fact, we found in deep lakes that you can kill bullheads with a very weak concentration of rotenone. When you come to a lake of about 10 feet deep and about five, six or ten feet of mud, then you have to use a concentration as heavy as four parts per million. You did not have success because you did not use enough of the rotenone.

DR. R. B. MILLER [Alberta]: I wonder if Dr. Prevost has given any thought to eliminating the size limit on trout.

DR. PREVOST: We certainly have. Of course, I think if you want real good management, you will have to treat each lake separately. We certainly go for some lakes with no size limit.

DR. MILLER: We have eliminated the size limit in Alberta. There is no longer any size limit.

DR. PREVOST: I am very pleased to hear that, Dr. Miller.

DISCUSSION LEADER CORNELL: What intrigues me is, what is the angler to do when he gets a 16-pound trout with a limit of 15 pounds. [Laughter]

CHAIRMAN HARKNESS: Thanks very much, Gus, for a very interesting and informative paper.

I would like to go back and refer to Ken Reid's beaver for a moment simply because in the Province of Ontario we harvest between 100,000 and 130,000 a year, and Quebec, likewise, our sister Province, harvests a very great number. As Mr. Reid said, they are wonderful back in the forests, but just as soon as you get them out on the farm lands, certainly in Ontario, they are a headache. We have more trouble with nuisance beaver now than we have with any other animal, on a nuisance basis, in Ontario.

THE EFFECT OF DRAWDOWNS ON LAKE TROUT REPRODUCTION AND THE USE OF ARTIFICIAL SPAWNING BEDS

N. V. MARTIN
Ontario Department of Lands and Forests, Maple

The following paper deals with studies of the effects of water level drawdowns on lake trout reproduction in certain Ontario lakes. This problem is becoming of increasing importance with the greater demands on water resources for electric power. From a questionnaire submitted to the 22 administrative districts of the Department of Lands and Forests it has been ascertained that in eight the problem may be considered to be of major importance while it is of some significance in six others. Eight districts indicated they had no such situation either because there are no lake trout populations or hydro storage basins in their districts.

SPAWNING OF LAKE TROUT

As little information on the reproduction of lake trout in Ontario was available as a basis for the investigation, a study of the spawning habits of the species was initiated in Lake Louisa, Algonquin Park, in 1947 and extended to other lakes in the following years. This lake was particularly suited for such a study as it had a large population of trout and clear water. Details of this investigation may be found in Martin (1948).

Lake trout spawning in small inland lakes of this region of Ontario usually occurs in the last half of October. It takes place at night on open shore lines usually with a westerly exposure. The bed itself is of broken rock or rubble varying from 2 to 6 inches in diameter and often interspersed with larger boulders. Sand, mud or detritus are usually absent. The depth of spawning varies with the body of water. In Lake Louisa eggs were found between depths of 8 inches and 4 feet while in Lake Opeongo spawning occurs between 3 and 12 feet.

The length of the incubation period in the lakes that have been studied has varied. Time of hatching has been as early as mid-February and as late as the end of March. Therefore there is a period of 3½ to 5 months when the eggs are vulnerable to water level fluctuations.

DRAWDOWN AND LAKE TROUT REPRODUCTION

With the preceding background of information a survey was made of three lakes of the Madawaska River drainage which are subject to water level control by the Hydro-Electric Power Commission of Ontario. This drainage arises on the height of land in Algonquin Park and flows in an easterly direction to enter the Ottawa River (Figure 1). In general water is held in certain headwater lakes of this system in spring and summer and drawn off in the fall and winter. This water is held in storage basins at Bark and Kamaniskeg lakes for use at three power developments further down the river. The time and amount of drawdown varies from year to year in each lake depending on the amount of precipitation, the needs of local interests such as logging operators, and power demands.

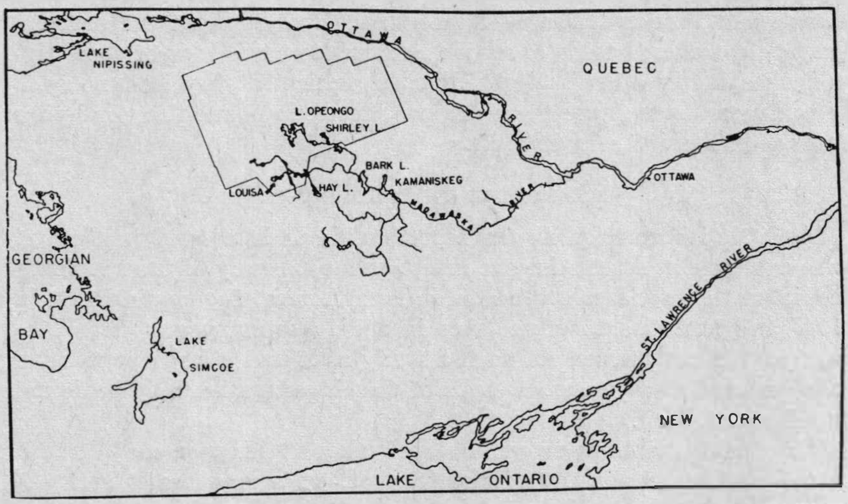

Figure 1. Madawaska River drainage, Ontario. Water is drawn off headwater lakes in fall and stored in Bark and Kaminskeg lakes for use at power plants farther down the river.

The procedure followed in the investigation was to find spawning areas by night observation or locate beds by the recovery of eggs with pumps or other means after spawning. These areas were marked at this time. If drawdown occurred the beds were sampled at regular intervals to determine what proportion of the egg depositions were exposed.

Lake Opeongo. Such studies were started on Lake Opeongo in 1948. This lake has an area of approximately 20 square miles and supports the chief lake trout fishery in Algonquin Park. Data on water level changes in the lake obtained from the files of the Hydro-Electric Power Commission are summarized in Table 1. Maximum drawdown subsequent to spawning has been about 2 feet in most years. Because

TABLE 1. APPROXIMATE WATER LEVELS[1] IN LAKE OPEONGO AT DIFFERENT TIMES DURING THE SEASON FOR THE YEARS 1946 TO 1955.

Year	Summer high	Spawning time	Winter low
1954	5.5	5.2	2.6 (to date)
1953	4.6	2.9	1.7 (Feb. 22)
1952	5.5	4.2	2.0 (Feb. 24)
1951	5.8	4.4	2.2 (Mar. 16)
1950	5.2	3.7	2.3 (Mar. 3)
1949	5.8	4.2	2.4 (Feb. 5)
1948	5.2	4.7	3.2 (Mar. 17)
1947	6.1	4.0	2.0 (Mar. 7)
1946	4.8	4.3	3.0 (Mar. 2)

[1]As taken from Hydro-Electric Power Commission staff gauge.

of the physical features of the lake it is impossible for the level to fall much lower than this.

A majority of the spawning beds have been located in Lake Opeongo and with the exception of one area, spawning usually occurs in over 3 feet of water. It is apparent from this that there has been little if any harmful effect of drawdown on lake trout reproduction in Lake Opeongo. It has been recommended water level manipulation be continued on its present basis.

Hay Lake. Hay Lake has an area of about 2,800 acres and spawning has been followed there since 1949. Water level manipulation has been made more complicated in Hay Lake because of the variety of interests on the lake. A large logging operation and sawmill is located there as are a number of hunting and fishing camps. A summary of water level conditions is shown in Table 2.

Hay Lake has exceptionally dark water and it has been difficult to locate spawning areas. Five have been found to date. Drawdowns previous to spawning have been small except for that in 1953. A drawdown of 6 feet in this year exposed a large proportion of the usual spawning beds. As a consequence the lake trout spawned on less favorable marginal areas of these beds where insufficient depth of rubble gave little protection to the eggs. Swarms of bullheads, *Ameiurus nebulosus,* were observed feeding on lake trout eggs and

TABLE 2. APPROXIMATE WATER LEVELS[1] IN HAY LAKE AT DIFFERENT TIMES DURING THE SEASON FOR THE YEARS 1946 TO 1955.

Year	Summer high	Spawning time	Winter low
1954	8.57	7.4	3.07 (to date)
1953	10.1	4.0	4.0 (Oct. 31, 1954)
1952	9.9	7.5	6.9 (Nov. 13, 1953)
1951	9.9	7.5	6.1 (Jan. 6)
1950	9.8	8.5	6.1 (Nov. 21, 1951)
1949	9.5	7.5	7.3 (Dec. 10, 1950)
1948	10.1	6.0	2.8 (Dec. 31, 1949)
1947	10.0	9.5	3.3 (Dec. 27, 1948)
1946	10.0	8.0	2.8 (Dec. 15, 1947)

[1]As taken from Hydro-Electric Power Commission staff gauge.

loss was likely high in these areas. This species is also one of the chief predators on lake trout eggs in other lakes of this region.

Drawdowns after spawning have generally been small since 1949 in Hay Lake although major changes were of common occurrence at this time before 1949. A 2½-foot drop in lake level after spawning in 1950 resulted in some egg loss (Figure 2) but this represented a small percentage of the total deposition.

In 1954 the Hydro-Electric Power Commission on request main-

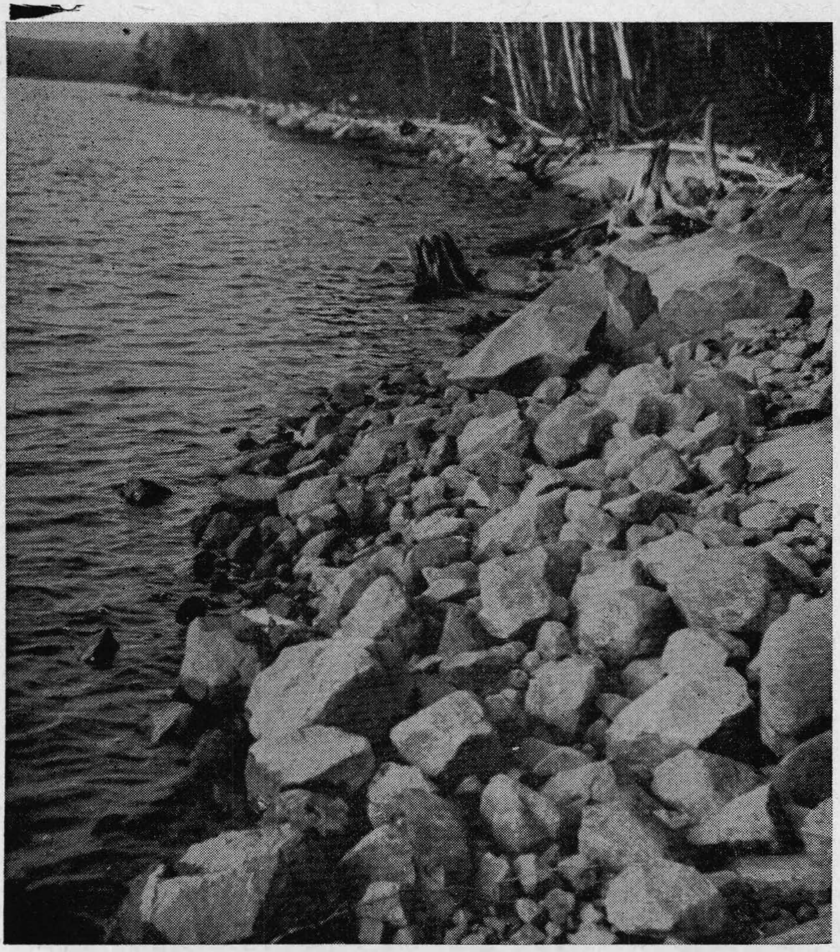

Figure 2. Hay Lake, November 1950, showing partially exposed spawning bed. Lake level (6.1) has dropped 4 feet from summer high and 2½ feet since spawning. Numbers of exposed eggs were located in this area.

tained the level of Hay Lake until after spawning. Unfortunately the final drawdown could not take place until mid-December when the lake had already frozen over. This made it somewhat difficult to assess the effects of the drawdown. Spawning areas marked out during October were re-visited in January. One of these beds, covering an area of approximately 50 feet x 20 feet was completely exposed at this time. This bed was partially uncovered of ice and large numbers of eggs were located in a small area. Egg survival had been high before drawdown and most had reached the eyed stage before exposure. An estimated 5,000 eggs were lost on this bed due to drawdown. As other known spawning areas in the lake were approximately at the same depth it is likely a drawdown of 4 or 5 feet after spawning exposes most of the egg deposition in Hay Lake.

It is impossible to take off a large volume of water in Hay Lake without loss of either spawning beds or eggs. Drawdowns of this kind occurred in 1954 and in the years before 1949. If these major drawdowns are unavoidable the greater part should take place before spawning. It is felt spawning success is likely greater in those years when the drawdown does come before spawning.

Shirley Lake. In 1950 a study of water level changes as they affect lake trout reproduction was began in Shirley Lake. This 1,200 acre lake, although only accessible to canoe parties, is becoming increasingly popular as a lake trout fishery. Data on water level changes are included in Table 3. The major part of the drawdown in most years occurs before spawning and the drop in lake level after spawning is usually less than a foot.

Most of the rocky areas where lake trout might spawn in Shirley Lake parallel the shore in 10- to 15-foot-wide strips. A 5- or 6-foot drop in lake level therefore exposes most of these areas. An exception to this was a steep talus slope at the north end of the lake where trout could spawn in spite of lower lake levels. As a result of this drawdown most of the rocky areas were either completely unavailable

TABLE 3. APPROXIMATE WATER LEVELS[1] IN SHIRLEY LAKE AT DIFFERENT TIMES DURING THE SEASON FOR THE YEARS 1946 TO 1955.

Year	Summer high	Spawning time	Winter low
1954	7.2	3.5	3.0 (to date)
1953	8.3	1.6	1.6 (Nov. 22)
1952	8.1	2.6	1.7 (Feb. 27, 1953)
1951	7.5	2.2	1.8 (Feb., 1952)
1950	8.4	2.7	1.7 (Mar. 4, 1951)
1949	8.0	2.8	1.7 (Feb. 2, 1950)
1948	8.5	5.9	1.5 (Mar. 6, 1949)
1947	8.5	4.3	1.8 (Mar. 1, 1948)
1946	6.4	2.3	1.8 (Feb. 27, 1947)

[1]As taken from the Hydro-Electric Power Commission staff gauge.

to the trout or they were forced to spawn on the lower fringe of the beds where the eggs are subject to considerable bullhead predation.

ARTIFICIAL LAKE TROUT SPAWNING BEDS

The use of artificial spawning beds set in deeper waters and below the range of fluctuating lake levels has been considered as a means of circumventing fall and winter drawdowns. The success of various rock-filled baskets and trays in collecting eggs when lowered on to the spawning beds indicated it might be possible to construct a large-scale spawning bed.

In 1951 exposed rocks on the shore of Shirley Lake were thrown into deeper water. Rock was also blasted from a cliff backing the spawning bed, broken up and also thrown into the lake. However, the trout failed to use these areas. It is felt there was insufficient depth of rubble and the rock size was too uniform.

In 1954 it was decided to repeat the attempt to make beds with the use of heavy equipment. Three areas along the east and exposed side of the lake were selected. The first of these was at the location of the 1951 bed and near a known spawning location. The other two were on exposed points. As the low lake level left these completely exposed each year it was not known if spawning had occurred near there previously.

Rock was obtained from the waste pile of a feldspar mine some 15 miles from Shirley Lake. The Road and Maintenance crew of the Petewawa Management Unit, Department of Lands and Forests, trucked some 200 tons of this rock to Shirley Lake. A large dock was built at the shore and the rock dumped on this.

Large pine logs obtained from an abandoned kedge crib were used to make two large rafts 35 feet long by 15 feet wide and these were sheeted with rough planking. These rafts were brought alongside the dock and loaded by hand. Dirt and powdered rock in the pile were washed into the lake with a fire pump before loading. Approximately one-quarter of the total tonnage was waste.

Distance to the three spawning beds was half a mile, a mile, and one and three-quarter miles. Up to 5 tons of rock could be taken on each raft at a speed of about 1 m.p.h. when these were powered by two 8 h.p. outboard motors. As far as possible large rocks were used for the bottom of the beds and increasingly smaller rubble near the top. The three beds varied somewhat in size and thickness. The first bed was approximately 45 feet by 15 feet and 2 to 2½ feet thick. The other two were slightly larger but not as thick. With the use of diving equipment it was possible to go down and smooth off the beds. At the same time ordinary water pails were buried in the rock to show whether there was any egg deposition on the bed and also provide information on egg density and survival.

A total of approximately 127 man-days was spent by the Petewawa Management Unit in bringing the rock to the shore of Shirley Lake. This involved the setting up of a camp, the construction of a mile of road, the building of a large dock and the actual loading and trucking of the rock. Six trucks, a loader, compressor, tractor, grader and 17 men were involved in this phase of the work at a cost of $1,454. Approximately 100 man-days and an expense of $725 were incurred in transporting the rock up the lake to the spawning beds. Both these figures are total costs and include such items as labor, maintenance, provisions, fuel, lumber supplies, purchase of rock, etc. The total cost of the operation was therefore approximately $2,200.

The beds were observed at various times during the spawning period which lasted from about October 14 to October 31. A maximum figure of approximately 75 lake trout was seen on the first bed but none was noted on the other beds during this time. It is thought the first bed was used as it was near a small spawning area previously used by a few trout whereas no previous spawning was known to occur near the other beds.

The pails buried in the first bed have been lifted at regular intervals during the fall and winter. A maximum of 276 eggs were taken from one pail with an average of about 65 eggs per pail. Total eggs shed on the beds should therefore be of the order of 50,000 eggs. This shows agreement with the total egg deposition that would be expected from the number of spawning fish.

CONCLUSIONS

The investigations of water-level fluctuations and lake trout reproduction that have been carried out to date point out the fact each lake is a separate problem. Levels fluctuate differently from lake to lake and year to year in one lake. In addition the trout spawn at different depths in various lakes.

The lowering of water levels before spawning is of limited value, at least in the lakes that have been studied, as there may be no available spawning areas when they are required. Furthermore with this practice the lake trout may be forced to spawn on unfavorable areas where egg loss may be high.

The use of echo sounders and diving equipment to locate possible rocky areas in deeper waters is desirable. It will be difficult to estimate the effects of various lake levels on egg depositions without this information. Such equipment will be used in Hay and Shirley Lakes in 1955 although it is unlikely there are any such areas in these lakes as were found by Cuerrier (1954) in Banff National Park lakes.

A check of the age composition of the lake trout populations in the storage lakes through a creel census may also be of value and give positive evidence of the effect of drawdowns. Year classes in Hay Lake from the years immediately previous to 1949, when there were major drawdowns after spawning, should be weaker than those derived from the 1949-53 period. These year classes should be passing through the fishery now. The year class spawned in 1954 may also be expected to be weak.

The Hydro-Electric Power Commission cooperates in water level manipulation as far as is possible within the limits of its water requirements. However in an exceptionally dry year drawdowns may be severe. The use of artificial spawning beds may be of use in circumventing situations where harmful drawdowns cannot be avoided. Obviously many situations will not lend themselves to their use. The cost of construction of artificial beds of course will vary considerably with such things as the accessibility of the lake, the cost and availability of a rock supply, hauling distances, labor rates, etc. No doubt the cost in many lakes would be less than that of the Shirley Lake project.

The present problem is to make the lake trout spawn on the unused spawning beds in Shirley Lake. Their use of the one bed near where some previous spawning had occurred may indicate they have either become familiar with this area over a period of time or it has some characteristic odor which attracts them to it. An attempt will be made in 1955 to attract trout to the other beds in Shirley Lake by penning mature fish on the beds or introducing some odor such as milt. If this succeeds, it may allow a greater choice of sites for artificial beds with a corresponding reduction in cost.

ACKNOWLEDGMENTS

The investigation was instigated by Dr. F. E. J. Fry, University of Toronto, and to whom the writer is greatly indebted for advice and assistance. The 1947 phase of the work in Lake Louisa was supported by a grant from the Ontario Research Council. The work since that time has been largely carried out by the Research Division, Department of Lands and Forests.

The cooperation of the Hydro-Electric Power Commission of Ontario in manipulating water levels and making available water level data has been appreciated.

The writer wishes to thank the Building and Maintenance crew of the Petewawa Management Unit under the direction of B. Boyle for the excellent work done in the construction of the Shirley Lake spawning bed. A large part of the expense of the operation was borne by the Pembroke District Office, Department of Lands and Forests. Without this aid the project would not have been posisble. Murray Brothers Lumber Company, Madawaska, Ontario, also assisted in this phase of the work.

The continued advice and help of N. S. Baldwin and Dr. R. R. Langford in all phases of the programme have been greatly appreciated. H. G. Holmberg gave valuable assistance in the field work.

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DISCUSSION

DISCUSSION LEADER CORNELL: Thank you, Mr. Martin. It appears that Dr. Prevost was correct in his assumption. An analysis of each lake is necessary because the water fluctuation apparently reduces the possibility of natural spawning. The lakes studied by Jean Paul Cuerrier, reported in the *Canadian Fish Culturist*, in April, showed that drawdowns in the lakes he studied increased reproduction but decreased food supply. Is there any comment on that aspect of the problem?

MR. J. P. CUERRIER [Canadian Wildlife Service]: This investigation was published in the No. 15 issue. Dr. Sprecher may correct me on this, but Mr. Verne Hackert, I think, was working in Green Lake, where, at that time, they had introduced lake trout, and for several years had stopped the plantings, and they were catching only very large trout. They were assuming there were no natural spawning facilities. Then he put up divisional spawning beds. They were using wooden boxes, and he succeeded in collecting lake trout eggs, and getting the lake trout to spawn in these artificial beds.

His problem was, as I recall, the *Necturus* were very active in eating the lake trout eggs. I was wondering if the same problem was occurring. Also, at that time, we discussed with Verne Hackert that maybe he could put some sort of screen on top of his boxes. The screen would be too small for the *Necturus* to feed on the eggs.

DISCUSSION LEADER CORNELL: There is one other aspect to this lake trout problem that I am sure someone may wish to comment on, and that is lake trout fishery of the Great Lakes, the aspect of reproduction of the lake trout in the face of lamprey depredation and the possibility of the value of hatchery fish. Does anyone care to comment on this aspect of the problem?

DISCUSSION LEADER CORNELL: Apparently that problem has been entirely solved or else the information is so limited it would be difficult to discuss it. There are some very interesting studies going on, however. I am sorry no one will comment on that for the benefit of those who might be interested.

FISH CULTURES FOR AGRICULTURAL WATERS

VERNE E. DAVISON

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This paper discusses the kinds of fish culture needed for several types of impounded waters and outlines the cultural problems to be solved. It is limited to the management of agricultural waters.

We are living in a period of resource development in which water conservation and storage are taking leading roles. The building of large numbers of farm ponds and reservoirs is a facet of this development. Fish production is one of the profitable uses of these agricultural waters.

As used here, "agricultural waters" refer to farm and ranch impoundments which can be managed as part of the farmers' operations Such an agricultural impoundment, if well designed, can be managed for high yields of fish: The quantity of water is manageable, its productivity can be increased, and the fish can be harvested as desired.

Many landowners and agricultural leaders in the United States believe that fish culture will expand the agricultural economy and produce food-fish and good fishing for extensive urban consumption. Thus they are beginning to supplement their incomes from irrigation ponds, fish ponds, and livestock waters.

We encounter, however, different kinds of water and a desire on the part of pond owners for information about various kinds of fish management. No single fish culture will permit us to enjoy the potential benefits of all these promising but varied waters. We need several kinds of fish culture in order to use our agricultural waters profitably and well.

VARIOUS TYPES OF AGRICULTURAL WATERS

Agricultural waters may be fresh, brackish or salt. They may be permanent or seasonal, maintaining constant water levels throughout the year or fluctuating significantly between wet and dry seasons. They include warm and cold waters, and perhaps an intermediate group of cool waters. Virtually every community in the United States has, nevertheless, water suitable for pond culture to grow food-fish or bait-minnows or provide satisfactory fishing.

Ponds with stable water levels, where self-perpetuating populations of fish are wanted. Fishing is the usual method for the harvest of the fish crop from a large percentage of our ponds. Many pond owners want this simplest kind of fish farming—stocking the pond only once for continuous reproduction. Small, family-size ponds managed in this way provide fishing, recreation and food for the family. Larger bodies of similar water often are managed to provide a financial return from the sale of fishing permits.

We have the bass-and-bluegill culture for warm-water fishing ponds in the South. In other parts of the United States, we need a similar combination of fish and management for ponds with cold waters. (Still another such combination may be needed for intermediate cool-water ponds, which are too cold for bluegills and too warm for trout.)

Ponds in which waters may be managed for commercial fish production. We can use several cultures which will produce marketable fish for sale by the pound or by the hundred. Bait-minnow culture is well developed now for the profitable production of goldfish, golden shiners, and fat-head minnows in warm waters. Commercial production of trout for market is a known culture for cold waters here and in other parts of the world.

The use of our warm-water ponds for the production of food-fish, such as catfish and buffalo fish, and possibly carp, requires profitable technologies. In poultry-raising sections, the waste products of the broiler industry are available as a resource to produce marketable fish. In the rice-growing sections, there are extensive possibilities and considerable interest in fish-farming in rotation with rice. Each of the 2,000,000 acres used to grow rice in the United States has suitable soil and water for fish production. The potential acreage is still higher, since much land now in farm ownerships is undeveloped.

If we are to serve this need of food-fish farming, we must have (a) intensive cultures that will produce a crop of fish in the short period of 4 to 7 months; (b) others that will produce a fish crop only once a year; and (c) a system that will grow a fish crop in two years or longer. We have no profitable, dependable disciplines in America for such food-fish farming, though some attempts are under way.

Ponds with fluctuating waters. Many of our ponds and irrigation reservoirs offer limited opportunities for fish production because water levels or surface areas are not maintained constantly. What can we produce in waters which will be lowered severely by irrigation-pumping or regular periods of dry weather at one season of the year?

The reservoirs which are pumped dry annually, or any short-lived pond, can use a variation of the 4- to 7-month culture of food-fish.

A promising culture for the permanent, but widely fluctuating, warm-water reservoir appears to be a combination of bass, bluegills, crappie and catfish, with the sale of fishing permits providing much of the income. This kind of water is similar to the natural conditions where crappie prosper with bass. These fluctuating waters also offer opportunities for the combined production of fish and wild duck foods, such as smartweeds and wild millets.

Salt-water and brackish ponds. It is feasible to construct ponds along our coasts, where the water ranges from full-strength ocean saltiness to slightly brackish. Only a few salt-water ponds (half sea-strength or more) have been built, and the studies of how to manage them are far from complete. Enough is known, however, to assure us that they are suitable for shrimp, crabs, mullet and other salt-water fish. Numerous species of fresh-water and salt-water fish have sufficient promise for the brackish waters to assure us that research and trial can develop a successful choice. The grey mullet and milk fish cultures of the East Indies and the Philippines give us some encouraging leads, but they do not provide sufficient guidance for us to make similar use of our salt and brackish waters.

The necessary soils, waters, and topography for fish ponds are available on thousands of farms in the coastal areas of the North American continent, Puerto Rico and the Virgin Islands. Such opportunities are rare in Hawaii, however.

Thus the development of desirable cultures for growing fish requires research, field trials, and experience. Right now we are in a stage of limited research and pioneering with fish pond management, even with the three cultures in common use—the bass-and-bluegill, bait-minnow, and marketable-trout cultures.

PRACTICAL PROBLEMS AND SOLUTIONS

Much confusion has resulted in recent years from (1) reports of high yields of fish in other parts of the world and (2) American controversies over principles and details of management. I have had opportunity to study various fish cultural systems and results in the United States, Asia, Europe and the West Indies. I have found the differences reconcilable in most respects. The basic relationships between fish foods, fish populations, and water management are universally alike. Only the practices vary in accordance with the conditions encountered. We can eliminate most of our controversies by recognizing that there are several fish cultures and sound variations of each.

Experienced fisheries technicians agree on most of our problems and opportunities. The following facts are universal:

1. *Fish can be one of the profitable products of pond waters.*

2. *Flood water and erosion are adverse to fish production.* Fish culture is largely water management, and muddy water and flood-

waters are not as productive as is needed in cultured ponds. Excess flow often washes fish out of the impoundment. Silt fills the basin and displaces the water resource. The soil conservation technology to control runoff and reduce silting is well developed in America.

3. *Waterweeds are troublesome the world around,* whether they be submerged, floating, or shallow-water emergents. The techniques of prevention and control are to deepen the pond edges, fertilize the water, graze and mow around the pond edges, and spray with chemicals. Chemicals, however, are expensive, their effects temporary, and their use dangerous in many situations. We must, therefore, use preventive management wherever possible.

4. *The natural fertility of water is seldom great enough for productive fish culture.* In all parts of the world, profitable farm-fish production demands high yields per acre. Water fertilization is the most efficient means of increasing production, and its importance is recognized in all countries where pond-fish culture is practiced. Commercial fertilizers are efficient and, in the Southeastern United States, will produce fish cheaper than will manures, hay and other organic materials. In many parts of the world, however, chemical fertilizers are much more expensive than in America, and animal manures are more plentiful. While the chemistry and practice of pond fertilization are well known in the Southeastern United States, they are not developed well enough for cold waters nor for waters of high alkalinity.

5. *The total carrying capacity of water is measured in weight of fish per unit of surface area*—in pounds per acre in the United States. The weight per unit of surface area is a common denominator of great significance. For fish cultural purposes we need to know, for each kind of water, the comparative yields of the natural water and fertilized water. We also need to know the cost, analysis, and yield of supplementary feeding. Total carrying capacity must be measured for each species of fish of interest and it must be related to the sizes produced (by weight).

6. *The size of fish to be produced is an important consideration.* Small sizes can be grown in greater yields and at less cost than larger sizes. The sizes in demand are, therefore, significant to both the producer and the scientist, particularly in the United States, where we demand large fish for food and sport.

7. *The average daily growth of fish per surface unit is a significant measure of production for intensive fish farming.* This is measured in pounds per acre per day in the United States and is similar to measuring livestock production. This yardstick has not been used commonly

in American fisheries research because it has been thought that such intensive production was not in demand. By measuring the daily production in weight, we can follow the rate of growth attained by fish of different sizes and in various population concentrations. A periodic check of the growth rate and daily yield is a fish farmer's check against overcrowding.

In Israel, Jamaica, Haiti, Indonesia, Europe, and elsewhere, studies have shown that the greatest growth per acre occurs while the weight of the fish is approximately 50 to 70 per cent of the water's carrying capacity. When their weight exceeds 70 per cent of the carrying capacity, the growth rate and production are reduced. The crop then should be harvested or thinned to reduce the number of fish. Our encouragement of ''heavy fishing'' in the self-perpetuating pond accomplishes a thinning of the population.

8. *Methods of harvesting the fish influence the annual yields.* Fishing is recognized generally as a harvesting method of low efficiency. In natural waters, it will annually harvest 10 to 15 per cent of the crop. Fishing becomes more efficient, taking 25 to 50 per cent, in well-managed waters of high fertility. Trapping is still more efficient, seining is better, while removal by drainage makes the harvest 100 per cent.

9. *Wildfish invasion of agricultural waters, and natural spawning in fish-farmed waters, are problems everywhere.* Draining, poisoning and filtering are some of the methods used to avoid these invasions of fish which compete for the food supply. We also use predator species to consume the unwanted fry and fingerlings, but this technique is not well developed except in the bass-and-bluegill culture.

10. *Stocking rates for farm ponds in the United States* have been the subject of much discussion and divergent opinion. This stems from two facts: (1) Our state and federal hatcheries produce the kinds of fish which are to remain in ponds more or less permanently, and (2) some people have not distinguished between non-cultured and cultured waters. To advocate one stocking rate for all waters is to assume that no one will practice the known techniques of modern fish culture. As an agricultural practice stocking rates must be related to water productivity.

11. *The species of fish for agricultural waters must be available, manageable and profitable.* Much work has been done in the United States toward providing fish for ponds, but most of it has been limited to the self-perpetuating warm-water game species and the cold-water trouts. Private fish hatcheries will be needed for some of the required cultures.

The farm pond research of the Agricultural Experiment Station, Auburn, Alabama, (H. S. Swingle et al.) continues its extensive research with bluegills, largemouth bass, shell crackers, speckled-bullhead catfish, and the gold-fish, fathead and golden shiner minnows. Swingle and his colleagues also studied to a point of temporary or permanent rejection in southern waters the following species: crappie, green sunfish, redbreast, long-eared sunfish, smallmouth bass, warmouth, round flyers, gizzard shad, Mississippi threadfin shad, yellow bullheads and chain pickerel. Fisheries research in northern agricultural waters is still too limited to be conclusive.

Limited studies of food-fish cultures are under way in Alabama and a few other fisheries research stations. They include bigmouth and smallmouth buffaloes, common and mirror carp, flathead and channel catfish, and speckled bullheads. Unfortunately, this research is not going ahead at full speed, because of lack of adequate financial support for agricultural fisheries.

CONCLUSION

We are confronted with a nationwide challenge to expand agricultural economy and to produce food fish and fishing from the waters of the tens of thousands of farm and ranch ponds and reservoirs in the United States. We need several fish cultures if we are to make profitable fisheries use of the varied types of these agricultural impoundments.

DISCUSSION

DISCUSSION LEADER CORNELL: Thank you, Verne. Agricultural waters usually are thought of as being those on farms where bass is the crop. Too much of our discussion in the past has been lost in minor details of fertilization procedures, stocking ratios, and so on. The problems presented by agricultural waters go far beyond those concepts, and Mr. Davison's paper has outlined the challenge that lies before biologists concerned with the management of these waters.

CHAIRMAN HARKNESS: Thank you, Mr. Davison. That concludes the papers specifically on fish. You will note, I think, that the papers on fish have dealt, for the most part, with cold-water fish and the ecology of cold-water fish, the things affecting the trout from different parts of Canada, and adjacent New York State, and it is certainly very pleasant to have a paper dealing with the problems of pond fishes and fishes in general in the southern part of the States where the warmer climate makes that type of fish culture a little more the rule of the day than it is in Canada, although certainly in the northern states there has been a great development in pond culture. It is coming into parts of Canada very rapidly.

SOME EVIDENCES OF HOME RANGE IN WATERFOWL

ALEX DZUBIN

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This paper has two objectives: (1) to present evidences of home range for breeding waterfowl in the pothole habitat of southwestern Manitoba; and, (2) to compile and clarify some of the conflicting data on territorialism in waterfowl.

Leopold (1933) has explained that the fundamental unit in management is the seasonal mobility or cruising radius of the species. In general, the geographic ranges of waterfowl are governed by the location of the breeding grounds, wintering areas, and migration pathways. In fact our entire flyway management schemes are based on the movement within and between these three units. There is a specific and individual variation in mobility within each unit. This affects our ability to census, harvest, or otherwise manage the ducks. The present paper is concerned with the mobility and "home range" of pairs on the breeding grounds from the time of initial settling, through prenesting and nesting, to the completion of incubation.

The home-range concept has not been studied extensively by ornithologists, although mammal workers have used it for some time. Seton (1909) has used "home range," and explains it as follows: "No wild animal roams at random over the country; each has a home region, even if it has not an actual home"; and, "In the idea of a home region is the germ of territorial thought." Burt (1943) defines home range as "that area, traversed by the individual in its normal activities of food gathering, mating. . . . Territory is the protected part of the home range, be it the entire home range or only the nest."

LITERATURE: HOME RANGE AND TERRITORY IN WATERFOWL

Sowls (1951) applied home range of waterfowl to "The area in which a bird spends its period of isolation between the breakup of spring gregariousness following spring arrival, and the reformation of fall gregariousness." He further reported that several favored areas may be used in any home range and that drakes "defend" more than one area. Defended areas did not always have definite boundaries. Similar observations were made in the present study, wherein males might show aggressive behavior at a loafing pothole, feeding area or waiting site without regard to definite boundaries. Nord, Evans, and Mann (1953) working in the pothole region of South Dakota have used the term home range to describe the number of water areas used by a pair. Evans and Black (1955) have more recently discussed the application of home range to waterfowl breeding on a study area at Waubay, South Dakota. Their work with marked pairs led them to plot minimum home ranges of blue-winged teal and to compute the mean radius for each pair. Their data, included in an appendix, indicate a wide use of certain fixed water areas by individual pairs. I believe that the concept of home range in waterfowl helps better to interpret observed behavior. Territories in waterfowl might be thought of as only the defended portion of the home range as similarly shown by Burt (*loc. cit.*), for mammals.

The concept of territoriality in mammals and birds has received much attention in the past and is receiving more consideration at the present time. Hochbaum (1944) has emphasized the role of territory in the breeding cycle of waterfowl. He has shown how drakes of one species protect a definite area from other pairs and certain drakes of their own species. Hochbaum (*ibid.*) and other workers (Stoudt 1952) have postulated a hypothesis of a carrying capacity on some of the waterfowl breeding grounds, on the basis of drake intolerance and water-area availability. Noble (1939) has proposed the simplest and most universally applicable definition of territory as "any defended area." Territorialism in some species of waterfowl has been questioned by other workers. For example, Munro (1943), working on the mallard (*Anas platyrhynchos*) in central British Columbia, stated that "no behavior that might be interpreted as territory defense has been observed." However Girard (1941) working in western Montana discussed how mallard drakes on "sentinel duty" kept other drakes away from mate and nest, and referred to their waiting areas as a "domain." Hochbaum (1944) described the territorial pursuits of the mallard on the Delta Marsh.

Trautman (1947) has reported that little competition was evident between males or pairs of black ducks (*Anas rubripes*) in an island-nesting situation, but that in a marsh where a larger nesting population was present males frequently pursued females. Wright (1947) found that "the territorial defense in the black duck in the Northeast takes the form of defense of the area the drake is in at the time, rather than the defense of a single area for the whole time he is with the nesting female. . . ." He later showed that "paired males will tolerate other pairs, but the approach of an unpaired male is the signal for violent reaction" (Wright 1954). Mendall (1949) believed that black duck territorialism was far less pronounced in the Northeast than it was for species in the heavily populated western marshes as indicated by Hochbaum (*ibid.*). However he later concluded that territorialism in the black duck might be a more important factor in underpopulated marshes than previously realized.

Harris (1954) working in the potholes of eastern Washington reported that mallard, gadwall (*Anas strepera*) and shoveller (*Anas clypeata*) defended territories most ardently, but blue-winged teal (*Anas discors*), cinnamon teal (*Anas cyanoptera*) and green-winged teal (*Anas crecca*) were the most tolerant of their own species. He further noted that territorial pairs of the same species mixed in feeding areas with no apparent friction between them. Johnsgard (1955) working in the same area, noted territorial behavior in the following species: blue-winged teal, baldpate (*Anas americana*), gadwall, shoveller, redhead (*Aythya americana*), and ruddy duck (*Oxyura jamaicensis*). Bennett (1936) reports territorial defenses in the blue-winged teal in Iowa and speaks of "nesting territory," "waiting territories," and "waiting sites." Hochbaum (*ibid.*) has described territories for the blue-winged teal in the Delta Marsh, Manitoba. In the Cariboo region of British Columbia, Munro (1944) noted that there did not seem to be much competition for territories in the pintail (*Anas acuta*) but a breeding pair of bufflehead (*Bucephala albeola*) established a definite territory which the male rigorously defended from encroachment by other bufflehead males (Munro 1942). He had stated earlier that the Barrow's golden-eye (*Bucephala islandica*) also has a territory (Munro 1939).

Populations of mallards and pintails in the short grass region of Saskatchewan and Alberta do not seem to exhibit the same territorial aggressiveness as reported elsewhere. Both Gollop (1954) and J. Lynch, (communication, Dec. 1954) have observed gregarious habits of pairs through the nesting season. Gollop (*loc. cit.*) reports that paired birds loafed in flocks, fed, and nested in groups during the period of prenesting, nesting and incubation. Webb (communication Aug. 1954) has noted similar behavior in pintails and some mallards in southern Alberta. Aggressiveness in those species was not very evident during the nesting season. W. Leitch, (letter Feb. 1955) noted friction between pairs of blue-winged teal and shovellers on the Caron Pothole Area in southern Saskatchewan. He also noted that intensity of mallard and pintail defense may have fallen off by the time his counts were taken on the area late in May, for aggressiveness was not shown by these two species at this time. A. Smith, (letter Feb. 1955), writing of the prairie pothole areas in Alberta, believes that "the pintail is nearly colonial in its breeding habits and the mallard somewhat so, though its population is usually lower in each area than is that of the pintail." His observations show that the blue-winged teal was the most territorial duck, but even within this species territorialism appears spasmodic. Other species showing territoriality to some degree, were the gadwall, baldpate, canvasback and ruddy duck. Further north in the parklands of Alberta, mallards, which made up the largest number of any one species, appeared as noncompetitive here as were pintails on the prairies to the south.

Up to the present time no intensive study of the territorial behavior of one species has been made, by a single observer, throughout the entire breeding range. Until this is done, the differences reported in the aggressiveness of any one species may only be differences of interpretation.

STUDY AREA AND WATERFOWL SPECIES

The area on which breeding home range of waterfowl was studied is a typical block of agricultural land, 1½ square miles in extent, some 9 miles south of Minnedosa, Manitoba. The topography is of a knob-and-kettle hole type. Upland fields of cereal grain crops are interspersed with small water areas or "potholes." It is a part of the characteristic pothole country in southern Manitoba, an area which stretches in a wide rectangular block for 4,000 square miles from Minnedosa and the Riding Mountains to the Saskatchewan border.

One hundred and ninety-five potholes of all types were found on the study area, 45 permanent, 115 semipermanent and 35 of a transient nature (modified after Bach's 1951 classification.) The emergent vegetation of potholes is varied: the main dominants are white-top (*Scolochloa festucacea*), sedge (*Carex* spp.), cattail (*Typha latifolia*) and bulrush (*Scirpus acutus, S. validus, S. paludosus*). Thirty-three per cent of the permanent areas were less than one acre in size and 75 per cent less than 2.5 acres, while 95 per cent of the semi-permanent potholes measured less than one acre and all were less than 2.5 acres. About 60 per cent of the land is under cultivation, 20 per cent is made up of potholes or wet areas, and the remainder is in permanent pasture, road allowances, fencerows and aspen-oak bluffs. For a more complete description, see Evans *et al.* (1952) and Dzubin (1954).

The total pairs of breeding ducks per square mile was 97.8 in 1954. Over a three-year period (1952-1954) mallards averaged approximately 33 per cent of the population, blue-winged teal 23 per cent, and canvasback 10 per cent. Other species breeding here in decreasing order of abundance were baldpate, pintail, redhead, ruddy duck, gadwall, green-winged teal and lesser scaup (*Aythya affinis*).

HOME RANGES OBSERVED

Twenty-three pairs of ducks were marked during the 1954 season, with the aid of boom-traps of the type described by Black and Evans (1953). Pairs were neck banded with the plastic Fabrolite. (This material was obtained at the T. Eaton Co., Winnipeg, Manitoba.) Subsequent observations were recorded on maps to show extent of home ranges. The following description of the home ranges of three breeding pairs of waterfowl, canvasback (*Aythya valisineria*), mallard, and blue-winged teal is given as an example to show the variation in size. They are presented because they were studied most thoroughly. Comments on other marked and unmarked pairs are included.

Canvasback. The minimum breeding range of one pair of neck-banded canvasback is shown in Figure 1. The maximum distance between drake observations or "range length" is 3,900 yards whereas the maximum distance between pair observations was 3,480 yards. The area shown in the map was also utilized by from 8 to 15 pairs of canvasback for feeding, loafing, waiting and nesting areas. All these ranges overlapped with the one shown and also overlapped with each other. The approximate area covered by the drake's home range was in excess of 1,300 acres. Previous observations of unmarked pairs, which were identified by certain plumage peculiarities, indicated that the area utilized by the drake was about 4 sections or 2,560 acres. During the prenesting period the pair ranged widely. This may be due in part to the wandering of the female in search of a nest site. Movements of the pair during the nesting or laying periods, and the incubation period were somewhat limited, chiefly to feeding and loafing areas. A few observations show that during incubation the drake's cruising radius increases.

The water area in the left central portion of the range (Figure 1), in which most of the observations were made has been called the "primary waiting area." At least four more such waiting areas were located immediately outside of the study block. The waiting area used by the marked drake was also utilized by three other canvasback pairs and an unmated drake during the breeding season. All drakes using this area appeared to wait here for their laying and incubating females. This same pothole was utilized as a feeding, resting and loafing area by the drakes and their hens. The five drakes fed together without friction until the females returned. Each drake of a pair would defend an area of about 16 feet about his hen. There was no clear-cut evidence that a drake protected an entire pothole or piece of shoreline in the absence of a hen. However, on several occasions, drakes were

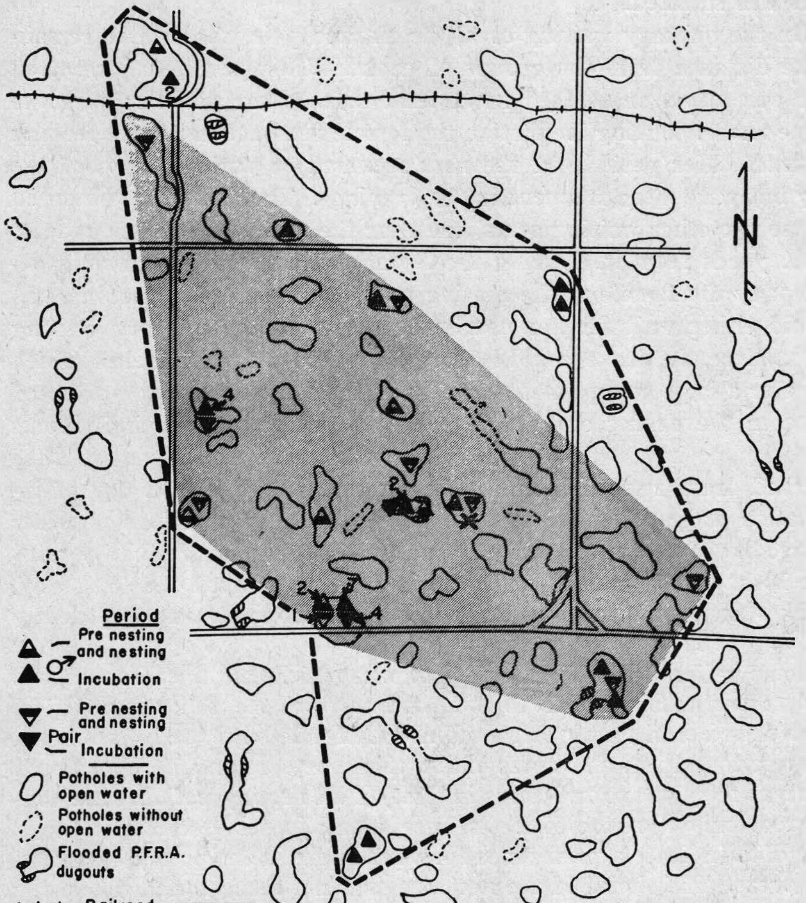

FIGURE I: MINIMUM BREEDING HOME RANGE OF ONE PAIR OF CANVASBACK

Explanatory note: The cross-hatched portion shows the area in which the pair was seen from prenesting through incubation, while the broken line gives the entire area in which all pair and drake observations were made. This latter area is the home range. Any observation of the drake during the prenesting or nesting period is shown as a half-darkened regular triangle. Subsequent observations during incubation are shown as full-darkened triangles. Pair observations are similarly shown except that triangles are inverted. Where more than one observation was made on a pothole a numeral associated with the triangle represents numbers of times the pair or drake was noted. The prenesting period is regarded as that period of one to two weeks prior to the time the first egg is laid, while the nesting period is confined to the egg-laying stage.

seen to attack a pair that had flown into the pothole on which the nest was situated.

On the primary waiting area, the marked pair was seen to remain some distance from the groups of drakes. This pair fed here often but was also seen to feed in at least three other potholes over the home range. During incubation the female used at least three feeding potholes. Her periods off the nest occurred at various times of the day but were more frequent in the evenings. The drake was observed to use two other waiting areas, one near the nest site and one at least 1,300 yards from it. During the laying period, the drake would accompany the female to the nesting pothole in the early morning and would either wait for her nearby or return to the primary waiting area. Here, the drake would loaf, feed or rest with other drakes, which were in a loose group. Although little aggressive behavior was noted between the drakes on this primary waiting area, some individuals appeared to have priority in the use of favored loafing spots. The marked drake remained on the home range until the 16th day of incubation after which he was not seen again. Observations of other pairs showed that drakes often remained in the nesting vicinity until the 10th day of incubation and less commonly until the 18th day. Drakes then gathered into large groups of 10 to 20 and apparently left for the molting grounds.

Some indication of the sizes of potholes which contained canvasback nests during 1952 and 1953 is apparent from the following. Of 51 nests found on the study area and in a half-mile buffer strip around it, 21 were found in water areas of less than one acre and 13 were found in areas from one to two acres in size. If all nesting potholes are considered, only 9 of the 51 nests were found in semipermanent areas. The data indicate a preference for small, permanent potholes as nesting areas. The choice of the nest site does not seem to depend on the pothole, *per se,* but is influenced by the presence of certain plant types, *e.g.* cattail and also by the suitability of other potholes in the region. A large pothole, *i.e.* above 2.0 acres in size, near the nest site was usually used by the drake for a waiting area. Utilization of potholes by the pair, both for feeding and loafing, appeared to be restricted to a number of permanent areas above one acre in size, but as shown, females did use smaller areas for nesting.

In summary, the home range of a breeding pair of canvasback included many potholes of which only a few were utilized to any extent. Certain potholes were used for feeding, others for nesting and still others as waiting areas. There is, then, a community relationship of potholes, each having one or more functions in the breeding requirement of a pair. No one pothole seemed to fill all breeding requirements completely. The home range of one breeding pair is also utilized successfully by other pairs, although the same potholes may not be used for similar daily requirements. On the study area, which includes many canvasback home ranges, there seems to be no general lack of nesting, feeding, loafing, or waiting sites, and the habitat could possibly support more pairs. However the total number of pairs has not changed materially from 1952 through 1954.

Drake aggression was confined to a small area about the female and moved with the pair. No one water area or portion of a water area was "defended" when the female was not present. However, as previously noted, the drake might show aggressive behavior toward a pair of canvasback which had flown onto the nesting pothole. This behavior did not prevent more than one canvasback female from nesting in water areas over 2.0 acres in size.

Mallard. The minimum breeding home range of one pair of mallards is shown in Figure 2. The range length is shorter and the land

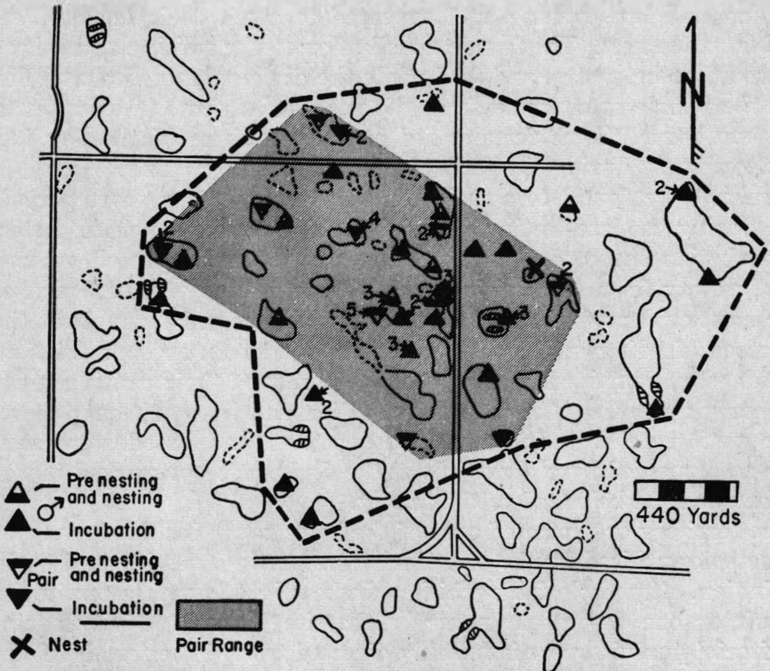

FIGURE II: MINIMUM BREEDING HOME RANGE OF ONE PAIR OF MALLARDS surface covered by the pair is less extensive than in the canvasback. The maximum length between drake observations was 2,590 yards and only 1,870 yards for the pair. The area covered by the drake alone was in excess of 700 acres. At least four water areas within 800 yards of the nest were commonly used by the drake, and these again have been termed waiting areas. Two of them were used more often than the other two. The range of the pair and drake during prenesting and nesting periods was somewhat variable, but was smaller than the range of the drake alone. The drake's range appeared to increase in size as soon as incubation had begun.

The feeding areas were a number of different potholes within the range. The pair or drake might utilize one area for 3 to 5 days and then feed in another pothole some distance away. The nest site logically must bear some relationship to the water areas most commonly used by the pair either for feeding or loafing but was not necessarily close to these areas. The pair fed and rested together during the afternoon, after the female had returned from the nest.

During incubation drakes visited their waiting areas in the early morning and late evening and appeared to await their females' arrival from the nest. The female usually flew to the waiting area where she would bathe, preen, and feed while the drake remained alert. The time the female spent away from the nest varied from 20 minutes to over two hours. Some females fed at various times of the day but most commonly during the early morning and late evening.

Drakes were found to associate with each other for short periods even during the late-nesting period but this was not common. Later, as incubation began, drakes started to congregate into small groups, usually away from the waiting area. These groups of 2 to 5 drakes were most evident during the late morning hours and throughout the afternoon, feeding and loafing together, with no apparent friction. Drakes often remained in the general vicinity of their waiting areas commonly up to the 14th day of incubation and less commonly thereafter until hatching. In several cases a hen and brood were seen to be accompanied by a drake but as the drake was not marked it was not known if he belonged to the family.

The territory or protected area of the home range included many of the potholes utilized by the pair during prenesting, nesting and early incubation. A pothole from which a drake might launch a territorial pursuit on one day might never be the site of such activity again. Territorial pursuits were invariably directed toward the female of the transgressing pair. The size of the territory might parallel the size of the home range during the prenesting and nesting period. Subsequently the range was greater and the size of the protected area became smaller. The drake, on his territory, regularly drove off pairs in territorial pursuit, but even the marked pair might be chased off some of the water areas which they used regularly. The home range was utilized by many other mallard pairs which nested in the general vicinity, as was the case with the canvasback. The waiting site was the pothole or potholes from which the drake consistently showed aggressive behavior and launched territorial pursuits. However, even these sites were used by other pairs when the drake was absent.

Blue-winged Teal. The data on blue-winged teal are more complete, and this spcies will be treated in greater detail. The minimum breeding home-range of one pair of blue-winged teal is shown in Figure 3. This range was much smaller than either the canvasback or mallard range. The maximum length of the drake's range was 1,560 yards, while it was only 1,100 yards for the pair. The drake was seen to utilize an area in excess of 250 acres. The bluewing range was also utilized by other pairs as part of their home range. This is also characteristic of the other two species. Pairs use potholes on their range which are not at the time occupied by another pair of the same species, or they may even occupy the same pothole as another pair for feeding or resting. During the prenesting and nesting periods

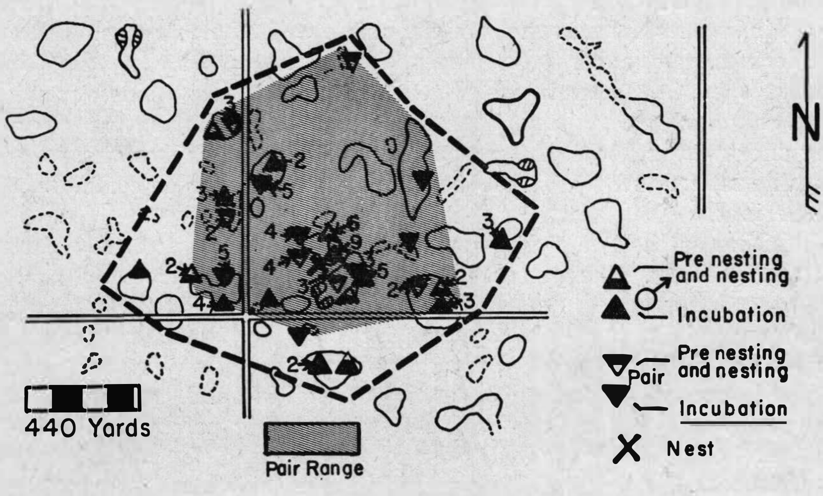

FIGURE III: MINIMUM BREEDING HOME RANGE OF ONE PAIR OF BLUE-WINGED TEAL the pair range was not extensive. Eggs were laid in the morning hours, but some females were noted on nests even into the late afternoon. During this time, the drake awaited the female on some one or more areas close to the nest. During incubation, at least five potholes within 500 yards of the nest were utilized by the drake during some portion of the day. The resting spots, which the drake used consistently, included muskrat houses, floating vegetation, exposed shore lines and mud flats. The primary waiting area, where most drake and pair observations were made, was the nearest pothole to the nest. However, observations on other marked pairs indicated that females or pairs may be seen up to one mile from the nesting site. Certain areas within the home range seem to be more preferred as feeding, loafing or preening areas than other potholes of a similar appearing nature. The feeding potholes were scattered through the home range but each might be favored for only 5 or 6 days.

Drakes were commonly observed in the vicinity of the nest site up to the 16th day of incubation and less commonly thereafter. Small groups of drakes were seen only during late-incubation and during the brood season. Drakes were observed with broods on occasion but, as with the mallards, the drake and the female with the brood may not have been a mated pair.

During the nesting and incubation periods groups of blue-winged teal pairs seem to cluster about several water areas to form a "community of pairs" or a small "breeding colony." This behavior, in part, influences the movement of the pair over its home range. These gatherings may be due to the presence of certain preferred feeding or loafing spots in the landscape. Groups of pairs utilize a single water area only during certain portions of the day: chiefly during the morning and evening hours. Intraspecific strife between drakes and pairs is quite intense but this strife does not seem to discourage community gatherings.

Waiting areas close to the nest are the consistently protected part of the territory. The presence of the female on the protected area enhances the aggressive response of the male. The head "pumping" (Lorenz 1941) of the female appears further to initiate aggressiveness in her drake. However, when several pairs utilize one water area, each drake is not always invincible in his own waiting site. Again, as in the mallard, it is the female of the transgressing pair which is attacked. Here, however, the drake of the attacked female attempts to fight with the territorial drake in mid air: this occurs only rarely in the mallard. Blue-winged teal drakes readily attack lone drakes of their own species if they fly nearby. Females in the presence of their drakes are aggressive to other pairs which swim into the vicinity. Much peeping and head pumping is associated with pair aggression in the potholes.

The actual protected area of one drake in a group of pairs which utilize a pothole might be very small, as little as 40 square feet. The size of this area may vary with the time of day and somewhat from day to day. On areas of less than 0.2 acres, one pair of teal is usually intolerant of another pair settling there. The pothole chosen by blue-winged teal may vary from year to year. One 1.2-acre pothole which supported three territorial pairs in 1952 did not have any in 1953. A larger area that supported one pair in 1952 had six pairs congregated there in 1953. The presence of one or two pairs on an area appears to draw other pairs into the vicinity. Darling (1938, 1952), working on gulls, has suggested that colonial nesting may have important stimulating value. Even though pair counts of blue-winged teal on this area have not fluctuated widely from 1952 to 1954, there appears to be more suitable habitat available than is utilized. This is merely speculation and not a known fact. In the bluewing the territory or protected area, in a rigid topographical sense, is confined to the primary waiting area or areas. This is protected against intrusion by other drakes, females or pairs even when the female is not present. Aggression is also centered around the female, and it appears as though she is defended from other drakes. In this sense, a "moving territory" might be applicable to the blue-winged teal. The waiting area of the male is the most constantly protected site. Blue-winged teal are more prone to aggressive fighting than either the mallard or canvasback and seem to be more intolerant of surrounding pairs than either of these species. The drake's territory closely parallels the size of the home range even during the early incubation period. In view of the fact that their range and territory are more restricted than the other mentioned species, their continued fighting seems most remarkable.

Table 1 summarizes the variation in the size of range lengths and area covered by the home range, for all three examples. The drake

TABLE 1. RANGE LENGTH AND AREA OF THE HOME RANGE IN THREE WATERFOWL PAIRS.

	Drake Range Length[1] (yards)	Pair Range Length (yards)	Approximate Area of Home Range (acres)
Canvasback	3,900	3,480	1,300+
Mallard	2,590	1,870	700+
Blue-winged Teal	1,560	1,100	250+

[1]Maximum distance between observations.

range lengths are larger than pair range lengths among the species studied.

Pintail and Redhead. Preliminary observations of pintail and redhead ranges have been made on some marked and many unmarked individuals. These observations are not as extensive as the ones on the previous three species and merely indicate that further variations in home-range size may occur. Pintail ranges may well extend over five miles in any one direction from a central point. One marked drake was seen four miles west and one mile north of his original banding site. Small groups of pintail drakes were seen even during the egg-laying period, but it is not known if these were mated birds. Smith (*loc. cit.*) noted, in southern Alberta, that seven pintail drakes used the same 0.75-acre pothole for a waiting area. No apparent conflict was noted between the individual drakes even when each of their hens returned from the nest to feed in the pothole. On my own study area aggressiveness between males seems to be rare in this species although chasing of females by drakes is evident early in the breeding season. Redhead ranges cover at least five square miles and closely parallel the ranges of some canvasback. Drakes do not appear to defend any one pothole or portion thereof but merely show aggressive response to other redheads that swim within some three feet of the pair. Single males and mated birds which are separated from their females tolerate each other's presence and may be found in small, loose groups through the breeding season until late incubation. Subsequently they leave the pothole country and apparently retire to the moulting lakes.

OBSERVED TERRITORIAL BEHAVIOR

The concept of a moving territory in Canada geese (*Branta canadensis*) has been criticized by Conder (1949) who felt that territory should be limited to an area of topographical reference. The term "individual distance" was used to describe the defense outside of a specific territory. More recently Koskimies and Routamo (1953) have used this term to describe the aggressive behavior of the white-winged scoter (*Melanitta fusca*) in Finland, as the "mated-female distance." These terms have not been utilized to describe the "moving territory" of the previously described prairie-nesting species. Because of the preliminary nature of this study the term "moving territory" will be applied to aggression shown by the male off his regular territory. This aggression is usually concerned with the "defense" of the female.

Any reference to protected area or territory in waterfowl implies some sort of aggressiveness on the part of a drake or his female to other members of the same species. Hochbaum (1944) has described pursuit flights and territorial defense in many North American waterfowl. However, aggressiveness or intolerance may not be the only explanation for these territorial pursuits. Heinroth (1910), Geyr von Schweppenburg (1924, 1929, 1930, 1953) and Christoleit (1929) have pointed out, for the mallard, the differences between true territorial pursuits, courtship parties, nuptial courtship flights, and harrying of nesting females by groups of drakes in attempted "rape." Even territorial pursuits, wherein a paired male flies at the female of an intruding pair and chases her a way while her mate follows some distance behind, might be explained as attempted rape on the part of the pursuing drake and not necessarily as a show of aggressiveness.

Mallard. The female of the mallard pair is the most consistently pursued individual in territorial encounters. Of the several hundred pursuits that I have observed, involving one or more paired drakes attacking a pair, only six were noted which showed friction between the drakes. The length of the territorial chase may vary. For instance, in one case a marked drake chased a pair for 150 yards while on another occasion the same drake chased another pair for 1,500 yards. The female during territorial pursuit, usually emits one of two calls, the call accompanying the "gesture of repulsion" or, less commonly, she may give the "inciting call" (of Lorenz, *loc. cit.;* see also Geyr von Schweppenburg, 1930). More than one drake may join a territorial pursuit for I have seen up to four drakes join a single drake in the pursuit of a mated hen. Most of my observations of the territorial pursuits in the mallard have been inconclusive since I could not follow many of the pursuits to completion. On many occasions drakes returned to their waiting area after a short chase only. Apparently successful "rape" has been seen once in the mallard and four times in the blue-winged teal. Webb, R. (*loc. cit.*) has noted it several times in pintails in Alberta. These observations show that, at least on occasion, chases are motivated by the "drive to rape." Further observations are needed to determine the frequency of this type of chase.

Contrasted to the territorial pursuit, courtship or sexual flights do also occur, as indicated by Hochbaum (*loc. cit.*). They are evident when more than 4 drakes are pursuing a female. The female in this case is usually silent and often hovers in the air for short periods. A marked drake that had a female in early incubation was seen to join such a party for a short time only. In fact many drakes which already appear to be mated join such courting groups and follow the hen for some distance. This is not to be confused with harrying or attempted rape of lone hens by large groups of drakes (See Geyr von Schweppenburg (1953) "courtship pursuit flights"). I have seen as many as 28 mallard drakes following a lone female in apparent courtship flight.

Territorial drakes do arise to meet lone drakes, but aerial fighting is very rare between mallards. A drake which flies into another drake's waiting area is usually chased off by a minor show of aggressiveness on the part of the owner. On the area which I studied, a mated drake mallard will not usually allow another mated pair, female or drake to utilize the same small water area or portion of marsh shoreline as himself, especially during the prenesting, nesting, and early incubation periods. The spacing of pairs on the breeding grounds could be explained on the basis of intolerance, through aggressive responses by the drake or perhaps by attempted rape. Pairs may have a definite space requirement in order to breed successfully and produce a maximum number of broods.

Blue-winged Teal. Territorial pursuits in the blue-winged teal closely resemble those described for the mallard, except that aerial fighting between drakes usually occurs. The male of the attacked pair does not merely follow the territorial drake which is chasing his female but fights with him in mid-air. Courting parties or nuptial flights seem to be rare in this species on the breeding grounds, although I have seen as many as 10 drakes harrying a marked, incubating female. The territory in this species is composed of some protected waiting area or areas. Drakes also show aggressiveness to other bluewings which swim close to the female whenever the pair is off the waiting area. This last ameboid area might be described as a "moving territory" in order to differentiate between "defense of definite topographical areas" and "defense of the female."

Canvasback. Pursuit flights are not evident, although small groups of drakes do chase females early in the breeding season. These may be courting flights as described by Hochbaum (1944). Nuptial courtship is also evident in this species (Hochbaum, *ibid.*). Aggressiveness, as reported previously, is evident when a pair, drake or female swims near a mated pair or occasionally when one pair flies into another pair's nesting pothole. Drakes, then, do not appear to defend any one pothole but only show aggression to other canvasbacks that swim near the female.

DISCUSSION

From these preliminary observations in the pothole habitat, I would like to suggest the following definitions which should be useful in the future discussion or understanding of home ranges and territorial behavior in waterfowl. They will stand the test of being used on other areas only if we realize that territories are not as rigid as previously thought. Each species of waterfowl will adapt to the conditions at hand, thus altering its daily pattern to conform to the characteristics of each habitat. In any event, these general definitions, which hold for the pothole region in southern Manitoba, will have to be subjected to a number of clarifying conditions for each species and then be tested for each habitat (*i.e.* marshes, rivers, lakes and especially in the short-grass region where some pairs breed in close proximity).

Home range is the area in which the pair is most active during the breeding season (*i.e.* during the prenesting, nesting and incubation periods). This area must include the nest, feeding, and loafing sites, and also the territory (including a waiting area). It is the sum total of land and water areas utilized by the pair, including such areas as the male and female may use individually from the time of initial settling on an area or prenesting period to the time the drake leaves for the molting grounds and the female hatches her brood. The last two events may not be simultaneous. The home range of pairs on the breeding grounds is distinguished clearly from the brood range and the range of the birds when they leave for the molting grounds. In cases of renesting, a new home range may be established, again beginning with the initial settling of the pair with intent to nest. This home range on the breeding grounds include some areas of constant activity and other areas of secondary importance. Most activity occurs near the center of the home range and peripheral areas are visited less frequently. The home range of the breeding pair has no special form but is dependent on the location of the first-mentioned requisites. This unit can and does overlap with similar ranges of other pairs of the same species and also with the home ranges of other species.

Territory is the defended portion of the home range from which a drake attacks another pair, drake or female, of his own species. Such attacks or territorial pursuits take place from a number of different potholes over the home range. The size of the territory might be said to vary from day to day depending upon from which areas the drake pursued other ducks. It could best be described as ameboid. It may parallel the size of the home range during early periods of nesting. Such boundaries as do exist may not be defended with any degree of intensity, for time of day and chronology of the breeding season ultimately influence aggressiveness. Drakes may make repeated attacks during one portion of the day and not show any aggressiveness during other periods. In addition to these defended areas a "moving territory'' is evident in some species. This may be associated with the defense of the female and not necessarily with the defense of a water area or portion thereof. Territorialism is most evident from the time of the prenesting period to early incubation. Since a defended area is the criterion for a territory, then it does overlap with the territory of other pairs of the same species and most certainly with territories of other species, for little interspecific intolerance exists. The size of this area varies between species, during different periods of the day for any one species, and during the breeding season itself. The difference between the territory and home range is often not clear cut. An area from which a territorial pursuit has taken place may not be utilized for subsequent pursuits. It is then a matter of interpretation and definition. Home range is associated with the whole portion of the land and water area utilized by the pair including the area from which the drake shows aggressiveness. It may thus include the nesting site if it is off the region regularly inhabited by the pair, part of the area over which the drake roams after incubation has started, and certain water areas from which the pair is regularly chased by another drake. The territory is not a clearly defined area with rigid boundaries. If it is not evident in some species on some habitats then, these individuals may have only a home range.

Waiting area is an integral part of the territory, comprising the site of intensive activity by the drake. It may be one pothole, a series of potholes or a portion of a marsh where the drake awaits the female during egg laying and the early incubation periods. It is persistently protected but, as in the case of the territory, may not be protected with the same intensity during all periods of a day or at all times during the breeding season. During incubation drakes use these areas most consistently in the morning and evening hours. Although it is in the home range, it may not be near the nesting site. The waiting area used by a drake may be utilized by another drake if the original one is absent.

ACKNOWLEDGMENTS

This paper is, in part, a presentation of the hypothesis and ideas of a great many people. It could not have been written without their aid.

I would like to thank the members of the Fish and Wildlife Service of Region 3 for their continued interest in the project, especially C. D. Evans, K. E. Black, and A. S. Hawkins who freely gave information from the Waubay Study and who discussed with me many of the ideas contained here. To other waterfowl workers (who gave unpublished data from their respective regions), J. H. Stoudt, A. G. Smith,

W. F. Crissey and John Lynch of the Fish and Wildlife Service, W. G. Leitch of Ducks Unlimited, J. B. Gollop of the Canadian Wildlife Service and to Ray Murdy of the South Dakota Department of Fish, Game and Parks, I also give my sincere thanks. H. A. Hochbaum and D. F. McKinney of the Delta Waterfowl Research Station gave invaluable aid in criticizing the manuscript. Dr. I. McT. Cowan, University of British Columbia, helped to initiate the study in 1952. Special thanks is due to R. A. McCabe, J. J. Hickey, S. T. Dillon, and R. S. Cook of the University of Wisconsin for advice and criticism of the paper. Murray Evans, Oberlin College, aided in the boom-trapping operations during 1954. Lyle K. Sowls, Arizona Cooperative Research Unit, gave many valuable suggestions. Much of the work on home range is an outgrowth of his past studies.

My thanks also, to the National Research Council of Canada for summer equipment; to the Wildlife Management Institute for funds to continue the study, and, to the Manitoba Game and Fisheries Branch for living quarters.

SUMMARY

1. Twenty-three pairs of ducks were neck-banded and followed to determine the extent of their home ranges. Three examples of home ranges are given. The maximum distance between drake observations was 3,900 yards for canvasback, 2,590 yards for the mallard, and 1,560 yards for the blue-winged teal. The approximate area of the home ranges of these three species was 1,300+, 700+, and 250+ acres, respectively. The drake's range length was larger than the range length of the pair.

2. Home range has been defined as the area in which the pair is most active during the prenesting, nesting and incubation periods. Territory is the defended portion of the home range. Waiting areas are part of the territory used, and defended, most consistently by the drake.

3. The "territory" in the canvasback was a defended area around the female, about 6 feet in diameter. The territory moved with pair activity; *i.e.* "moving territory." Mallard and blue-winged teal territories were ameboid in shape: usually including one or more waiting areas and an area of indefinite size, similar to that defended around the canvasback female. This area, in the blue-winged teal is defended by the drake wherever the pair are together. The drake, however, may defend an entire pothole when the female is on the nest. Blue-winged teal drakes fought more over territories than either canvasback or mallard drakes, apparently because of their re-

stricted home range. Territorial pursuits by mallards and blue-winged teal are directed against the female of the transgressing pair. During pursuits blue-winged teal drakes fight in the air whereas mallard drakes rarely do. Courtship flights occur in the mallard, blue-winged teal and canvasback especially early in the breeding season. Groups of mallard, blue-winged teal drakes chase lone hens during the breeding season. This behavior may be associated with attempted rape or harrying by the drakes.

4. A review of the literature on the presence of territories in waterfowl presents a confusing picture. In the pothole region I found territorial behavior well developed. According to other observers the intensity of territorial behavior varies between species and in different breeding habitats. Further detailed study is needed to clarify the situation.

5. I believe the concept of home range helps better to interpret observed behavior in waterfowl. Further intensive study of home ranges in each habitat should lead to a clearer understanding of the biology and ecology of each species of duck.

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DISCUSSION

DISCUSSION LEADER CORNELL: Thank you, Mr. Dzubin. I am sure this paper on the definition of terms is of considerable value, particularly in future studies in this field, and to the ecologists who will describe the habitats concerned. It is impossible to think constructively without exact definition of terms used, and in this paper we appear to have an excellent summary of exact definitions.

Are there any questions to be asked of Mr. Dzubin at this time?

Mr. Dzubin, is it your belief that this range concept limits the duck population density on the nesting area?

MR. DZUBIN: That is a very difficult question to answer at the present time. The birds in the short-grass prairie do not seem to be territorial and almost seem to breed in colonies; whereas the birds in my own study area seem to space themselves; that is, you never find two pairs of mallards in the same pothole. I am in no position to answer that question at the present time. I am sorry.

DISCUSSION LEADER CORNELL: Are there any questions from the floor? I have one more, then.

Is there enough information, from your observations, to make recommendations concerning the management of breeding areas, either in the creation of potholes, limiting their shape or size or vegetation in them?

MR. DZUBIN: A study of this sort would lead us to a better appreciation of what birds need to successfully breed. However, I feel that more studies of this sort would have to be done in each habitat to find out what birds need in other habitats, and all this compared in some sort of summary, and this might be given to those states interested in creating a new waterfowl habitat.

FLUOROSCOPIC MEASURES OF HUNTING PRESSURE IN EUROPE AND NORTH AMERICA

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The human population in the United States has doubled in the last 50 years and will double again in much less than 50 years. With the increase in people and the decrease in living space for wild things we are urgently in need of means of measuring the significance of hunting in limiting wild populations. The need for such measures is particularly great for migratory game because no state feels the same responsibility for birds reared elsewhere that it does for its resident birds. This is clearly shown by the fact that no state asks for a reduction in the 120 days of legal harvest that our waterfowl sustain each year in their flight to the wintering grounds.

Many efforts are being made to measure hunting pressure in waterfowl. Means of interpreting "duck stamp" sales are being studied, but such indices cannot measure hunting pressure sustained by the same ducks when outside the United States. Rates of band recoveries have been made available through the work of Hickey (1952), Cartwright and Law (1952) and others. The use of a fluoroscope to detect the percentages of live ducks and geese carrying shot pellets in their flesh has been explored by Whitlock and Miller (1947), Elder (1950) and Bellrose (1953).

The fluoroscopy of waterfowl provides an immediate result whereas banding requires several months or years to provide useful data. Thus

[1]Contribution from the Missouri Cooperative Wildlife Research Unit: U. S. Fish and Wildlife Service, Wildlife Management Institute, Missouri Conservation Commission, Edward K. Love Foundation, and University of Missouri cooperating.

Work in Europe was done while on sabbatical leave from the University of Missouri and was made possible through the financial assistance of the Wildlife Management Institute and the American Museum of Natural History.

a random sample of geese on a wintering refuge can be examined with a fluoroscope immediately after the close of the hunting period and a measure of the total hunting pressure on that population secured directly. Further, the fluoroscope can be used to measure hunting pressure in resident species such as pheasants, showing the amount of hunting pressure being sustained by legally protected hens, and discerning regional differences in pressure on cocks within a state. It may also be used to detect shot in populations alleged to be unshot.

It is the purpose of this paper to report observations made with the fluoroscope of birds in North America and Europe, to discuss species differences and their possible causes, yearly trends in key species, flyway differences, and the significance of target size as a factor influencing the results of fluoroscopy and band recovery rates and to explore the relationships among the various measures of hunting pressure.

APPARATUS AND METHODS

The equipment now in use is a simplified form of that which I described in 1950. It was reduced in weight for transport to Europe by two principal improvements: 1) The heavier type generator was replaced by a "Homelite, Model 23-A," which weighs only 125 pounds, and is compact in design and simple to operate. It was especially valued when operated on the fire escape of the fourth floor of a large London poulterer's establishment. 2) The lead-lined box which encloses the X-ray machine, supports the fluorescent screen and provides a table top for movement of the boxed ducks was redesigned as shown in Figure 1.

This new design permits examination of geese wrapped in sacking when the table top is inserted at the lower level with the accessory door removed; it provides minimum clearance for a duck-restraining box beneath the fluorescent screen when the top is inserted in the upper position with the accessory door in place. In this way the bird to be examined is kept as close to the screen as possible, thus increasing clarity of the image. The top can be quickly removed and slipped into the plywood box in vertical position for carrying or shipment.

The push-button switch controlling production of X-rays is mounted on the side of the box for convenience in hand operation and to keep it off wet ground.

Procedure in operation is exactly as previously reported. Great familiarity with the safety routine has permitted complete abandonment of leaded gloves and other protective clothing. If the push-button is never touched until the duck is in position and the other hand

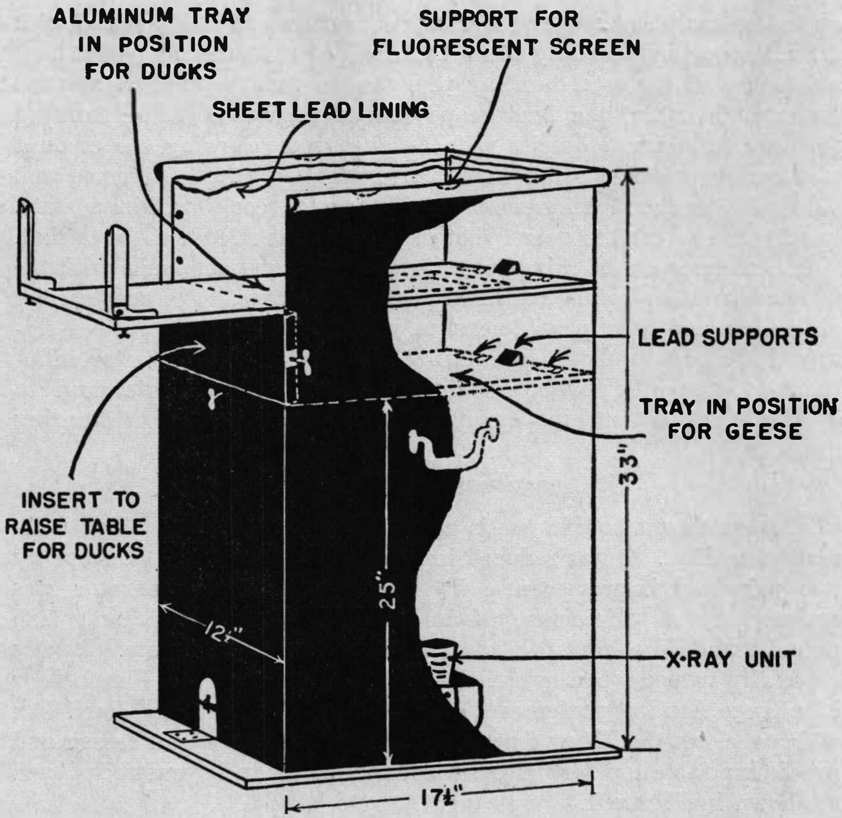

Figure 1. Fluoroscope with one side cut away to show details of construction. The aluminum covered tray can be quickly put in position for ducks or geese or removed and slipped vertically in the top for carrying.

dropped below the protective lead-lined wall of the box, no hazard results. Great care is needed in fluoroscoping geese for it is frequently necessary to roll them or move them about to see a part which has fallen outside the illuminated field of the fluorescent screen. Even though outside the beam, the hand should not be held nearby in readiness for it will receive constant secondary rays ricocheting from the bird and the screen. Best practice is always to put the left arm behind the back before pushing the switch.

SOURCES OF BIRDS FLUOROSCOPED

Birds of all species have been sought whenever and wherever an adequate sample promised to be available. Chief sources were:

1. Bait traps operated for banding purposes—at the Delta Waterfowl Research Station; Tule Lake Refuge, California; Lake Andes and Coxy's Lake, South Dakota.
2. Corral traps into which flightless birds were driven for banding by Fish and Wildlife Service crews in Manitoba and Saskatchewan.
3. Decoys into which birds were enticed with a tolling dog—Delta Waterfowl Research Station; Severn Wildfowl Trust, Gloucestershire, England; and in Holland.
4. Geese captured with cannon net traps in Missouri and rocket nets in England and Scotland.
5. Natural catastrophes such as botulism die-offs in Manitoba, winter kills of pheasants in South Dakota, and drownings of old squaw ducks in fishermen's nets in Lake Michigan.
6. English and Dutch "poulterers" dealing in decoy-trapped ducks from Holland.

FINDINGS

More than 21,000 birds have been examined with the fluoroscope during the six years of this study. Although this report includes only those species examined in large enough numbers to permit drawing at least tentative conclusions, the many other species handled in lesser numbers frequently proved of much interest. The two sandhill cranes found dead during a botulism epizootic contained lead shot in their flesh. Three of seven live Ross geese trapped at Perry River by Peter Scott also carried body shot. A large sample of a "totally protected" species would indeed be interesting to examine. Any natural catastrophe providing a sample of dead swans for instance, would thus show how well the closed season is being observed.

Yearly variation in the incidence of body shot. In the early years of this study it was hoped that year-to-year trends in hunting pressure might be detected. Only four species were examined in sufficient numbers each year to provide data for this comparison. However, it is clear from examination of Table 1 that the year-to-year differences were small, seldom exceeding 5 per cent. The only one which proved statistically significant at the 95 per cent level was that for drake pintails. Here the 1950 birds were found to have been shot less heavily than in the preceding or following years.

To detect year-to-year changes of 5 per cent in shooting pressure a sample of approximately 700 birds of the same age, sex and species must be obtained each year. For seven years Bellrose (1953:344) examined Illinois mallards for body shot, but large samples were handled only in 1949 and 1950; the percentage adult drakes with shot was nearly

TABLE 1. YEARLY VARIATION IN THE INCIDENCE OF BODY SHOT IN SUMMER POPULATIONS OF DUCKS IN MANITOBA AND SASKATCHEWAN, 1948-1952.

Species	Year	Males: Number Examined	Males: Per cent with shot	Females: Number Examined	Females: Per cent with shot
Mallard	1948	83	31.3	88	25.0
	1949	972	28.3	286	20.0
	1950	1,210	30.0	360	24.7
	1951	875	27.0	382	23.6
	1952	237	27.4	124	20.2
Pintail	1949	648	21.0	264	15.5
	1950	1,286	16.2	116	6.9
	1951	223	23.3	45	11.1
Blue-winged teal	1949	649	6.6		
	1950	844	7.7		
	1951	701	8.7		
	1952	173	8.1		
Green-winged teal	1949	119	7.6		
	1950	242	7.0		
	1951	48	8.3		

identical in these two years (36.0 and 36.9). It would seem that the carry-over from one year to the next of an unknown population segment bearing body shot probably masks year-to-year differences due to variations in shooting pressure. Hence, trends within species may only be detected with very large samples or when major changes in shooting pressure take place. Perhaps every tenth year a large sample of adult drake mallards should be examined in order to follow long-time trends and measure the effects of greatly altered bag limits or a closed season, such as that established for Canada geese in the Mississippi flyway in 1946.

A pooled sample of mallards and black ducks examined in Michigan (Miller, 1943) after close of the hunting season showed that, among 864 birds of both sexes, 26 per cent carried body shot; this is identical with my mallard sample from the Prairie Provinces when data from both sexes are lumped (Table 4). It is of interest to recall that the Michigan sample was obtained in 1941-42 or five years before my sample was gathered.

Species and sex variations in the incidence of body shot. The data from all years were pooled to give an average frequency of occurrence of body shot in males and females separately for the 13 species shown in Table 2. Mallards, black ducks and pintails lead the list and the entire sequence of apparent hunting pressures confirms the earlier conclusions (Elder, 1950) and fits well with Bellrose's (1953) figures for the mallard and blue-winged teal. His lower figure for the pintail (12.1 per cent) I believe is due to the very small sample and the fact that it includes an unknown proportion of females, which in most species collect fewer shot than do males. Our data for female mallards are similar (23 vs. 21.4 per cent), Bellrose's figure being somewhat depressed by an unknown percentage of juveniles which could not be separated from adults in the fall. All my data were gathered after the close of hunting in winter or in summer when all birds were adult and had been through one full shooting season.

No means for distinguishing sex in live coots has been found, hence all data for this species both from band recoveries and from fluoroscopy represent average figures for both sexes. The very small percentage found carrying shot is thought to be due to four factors: they migrate at night, raft on open water during the day, move south just ahead of the waterfowl hunting season (Bellrose, 1944; Ward, 1953) and like teal, are probably more likely to come down when hit by one shot than are larger birds such as mallards (Bellrose, 1944, 1953). That this vulnerability to hunting is not shown by band recovery rates (Mann, *et al.*, 1947; Van den Akker and Wilson, 1949; Cartwright and Law, 1952) is perhaps attributable to the fact that many hunters do not bother to pick up coots they have killed.

The very late arrival of old squaw ducks in this country, plus their habit of staying offshore on the open water, undoubtedly accounts for the fact that less than 2 per cent of adults and but one of the 414 juveniles examined carried shot.

Comparison of migratory and resident species. Aldo Leopold (1933: 215) pointed out the fact that states do not permit their resident game birds to be hunted for more than a fraction of the period that their migratory birds are pursued each year. This raises the question as to whether ducks are really hunted harder than upland game birds. While in search of an answer to this question I accepted the invitation

TABLE 2. INCIDENCE OF BODY SHOT IN BIRDS FLUOROSCOPED IN NORTH AMERICA, 1948-1954.

Species	Males			Females		
	Number Examined	Number with shot	Per cent with shot	Number Examined	Number with shot	Per cent with shot
Mallard	3,934	1,170	29.7	1,324	304	23.0
Pintail	2,229	421	18.9	485	66	13.6
Blue-winged teal	2,370	183	7.7	238	11	4.6
Green-winged teal	411	32	7.8	84	2	2.4
Shoveller	159	13	8.2	39	0	
Baldpate	140	15	10.7	38	4	10.5
Gadwall	62	10	16.1	27	0	
Black duck	62	17	27.4			
Old squaw	345	5		276	3	1.1
Redhead	11	2	18.2	51	11	21.6
Canada goose[1]	402	189	47.0	333	144	43.3
Pheasant	301	82	27.3	445	23	5.3
Coot (male and female)	340	10	2.9			

[1]Data for this species supplied by C. E. Shanks, Missouri Conservation Commission.

of the South Dakota Department of Game, Fish and Parks to fluoroscope a sample of 746 pheasants winter-killed by ice storms between January 15 and February 18, 1953. The results (Table 2) show that, during the shooting season of 20 days in the heart of America's pheasant range, cocks sustain about the same shooting pressure as do mallard drakes during their entire migratory flight through four months of shooting.

South Dakota hunters are not shooting indiscriminatingly at any pheasant that rises, for only 1/5 as many females carried shot as did males, but when we correct for the difference in the size of the two sexes (cocks are 25 per cent larger; see Table 5 and discussion below) we find that females are subject to careless or illegal gunning equal to one-fourth the hunting pressure sustained by the cocks. A good many hunters should see the sex of their bird more clearly before pulling the trigger.

Comparison with European ducks and geese. In Europe wildfowling has been practiced centuries longer than it has in North America, and the fact that waterfowl have persisted in the face of comparatively dense human populations for so long makes it seem particularly important that we evaluate the effects of current hunting practices and the laws affecting these practices. European waterfowl are not protected by international agreements like our Migratory Bird Treaty Act of 40 years standing, nor by uniform laws over large areas such as the United States. Hence, any population of ducks or geese is shot under many different sets of regulations from Iceland and Finland to Spain and Italy. In many countries there are no legal restrictions on hours of shooting, size of daily bag or means by which waterfowl may be taken, and game wardens are unknown. They are trapped in decoys in Holland, netted in France, pursued with punt guns in Britain and they are widely sold in poultry markets.

Does this general freedom from legislative restriction cause the European gunner to pursue his waterfowl more vigorously or effectively than we do ours in North America? It was in the hope of answering this question that I examined geese in Great Britain and ducks in Holland last year.

While working at the Severn Wildfowl Trust, I made two expeditions with Peter Scott to the principal river estuaries in England and Scotland where geese gather in large concentrations to winter. A detailed account will appear elsewhere (Elder, in press,-a) but the results of fluoroscoping 1,216 pink-footed geese and 260 grey lag-geese are shown in Table 3.

TABLE 3. THE INCIDENCE OF BODY SHOT IN EUROPEAN DUCKS AND GEESE.

	Adults				Juveniles	
	Males		Females		Both Sexes	
Species	No. exam.	% with body shot	No. exam.	% with body shot	No. exam.	% with body shot
Pink-footed goose	407	44.0	418	38.5	391	3.8
Grey lag-goose	82	36.6	79	38.0	99	3.0
Teal	149	6.0	87	4.6	655	1.4
Mallard—Oct.	61	18.0	26	23.0	314	10.0
Dec.	89	18.0	41	25.0	259	10.4
Oct.						
Frozen birds that could not be age-classified.	264	18.5	139	10.8		

The large numbers of pinkfeet already carrying bands showed that these birds came directly from Iceland where Mr. Scott had banded them in previous summers. The percentage of adult males carrying shot (44 per cent) shows that this species is shot as heavily in Iceland and Great Britain alone as are Canada geese coming to Missouri (Table 2, Shanks' data).

The very high incidence of shot in geese on both continents is explained in part by their large size, comparatively great longevity and the intensity with which they are sought as a sporting trophy. On their wintering ground in Great Britain they are pursued for four months, without bag limit or restriction on hours of shooting. In North America despite our many restrictive regulations our geese are subject to the same high pressure because our hunters have better transportation, more lethal armament (pump guns and automatics, outlawed by tradition in England) and are provided artificial concentrations by public shooting grounds in connection with refuges—a practice unheard of abroad.

Duck traps in Britain are few and catches last winter were small. Consequently, I depended upon Dutch-caught ducks taken in large decoy traps by means of trained decoy dogs. Their ducks are killed and sold for market in Holland and shipped frozen to London. Hugh Boyd (of the Severn Trust) and I were admitted to one large cold storage plant in London for one day's work. We fluoroscoped 1,226 frozen ducks from Holland. The European teal were very similar to our greenwings; we could age and sex them by their plumage. In the mallards, sex alone could be distinguished, for neither the penis nor the bursa could be examined while the birds were frozen.

The other 1,062 ducks were examined at various poulterers in Holland—in Amsterdam, Leeuwarden and The Hague. The results are shown in Table 3, but I have not included samples of other species examined (shoveller, pintail, European widgeon and garganey) for their numbers were too few to warrant reporting percentage occurrence of body shot.

The percentage of European teal found with shot was not significantly different from that found in bluewings in North America where from 6.6-8.7 per cent of adult males (Table 1) carry such pellets in their flesh.

The mallard is the key species in most places in Europe, just as it is in North America. The consistency with which 18 per cent of males were found with shot indicates the probable reliability of this figure. It is significantly lower than the percentage found by me in adult male mallards in America during five years (27-31 per cent) and by Bellrose during seven years (29-41 per cent). The 10 per cent of females found with shot is less than half the frequency found in hen mallards in America. This strongly suggests that our mallards in the Mississippi fiyway are being more heavily shot than are the mallards wintering in the Netherlands. In view of our many restrictions and complete international agreements, this is an interesting and somewhat surprising finding.

Final evaluation of these fluoroscopic data as a measure of comparative shooting pressures under European and American systems of management should await comparison with some other measure of shooting pressure such as mortality rates computed from band recoveries in Europe. So far the only such study available is that by Höhn (1948) which indicates that English mallards have higher mortality rates than do ours (Hickey, 1952). Analyses of decoy-trapped and banded birds in Holland are much needed and will soon be completed by J. A. Eygenraam of the Institute for Applied Biological Field Research.

Relation between incidence of body shot and band recovery rates. If fluoroscopy and rates of band recovery are both valid measures of hunting pressure, there should be a constant relationship between cumulative band recovery rates for each species and the average percentage of each species found carrying lead pellets during the same series of years. To explore this relationship the average percentage of each species carrying shot was determined by lumping data from the two sexes and for all years in order to plot them against cumulative average recovery rates from Fish and Wildlife Service files for early years (Hickey, personal communication), as shown in Table 4. These are shown graphically in Figure 2. A more discriminating comparison may be had by comparing the sexes separately with their respective band recovery rates. Hence Table 4 also gives the direct, or first-year recovery rates, from Ducks Unlimited banding (Cart-

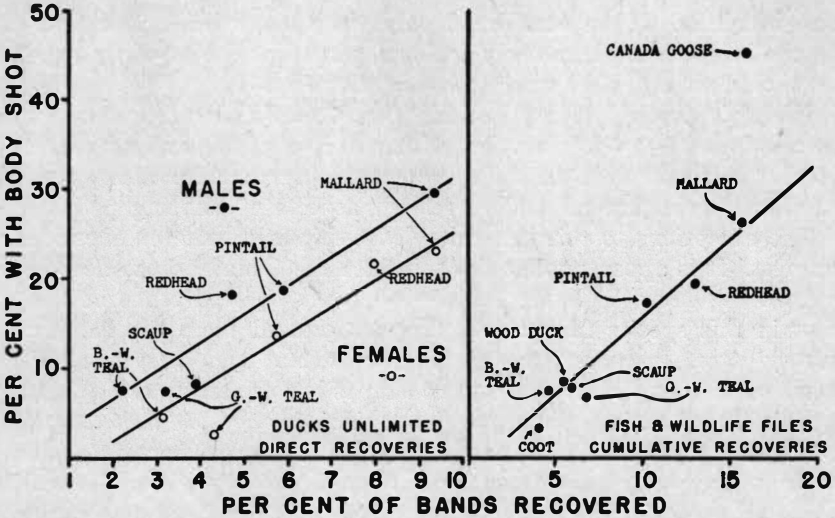

Figure 2. The relationship between band recovery rates and the percent of birds carrying body shot. Cumulative recoveries (sexes lumped) are from Hickey (personal communication) and D.U. figures are from Cartwright and Law, 1952.

wright and Law, 1952). These are plotted against sex-discriminatory fluoroscopy data (as given in Table 2) for graphic presentation in Figure 2.

Examination of this figure makes it clear that there is a very real and almost constant relationship between the two measures of hunting pressure—as band recovery rates increase the incidence of body shot

TABLE 4. BAND RECOVERY RATES OF NORTH AMERICAN WATERFOWL, AVERAGE TARGET SIZES AND PER CENT CARRYING BODY SHOT (SEXES LUMPED.)

	Ducks Unlimited Direct Recoveries[1]						U. S. Fish & Wildlife Service	
	Males		Females				Cumulative Recoveries[2]	
Species	Number banded	Per cent recovered	Number banded	Per cent recovered	Target size	Per cent with shot	Number banded	Per cent recovered
Mallard	4,273	9.4	4,398	9.2	1.91	26.1	259,986	15.6
Black duck					1.88		96,354	12.8
Pintail	1,126	6.0	2,546	5.8	1.55	17.5	172,900	10.2
Blue-winged teal	2,152	2.3	1,781	3.1	.94	7.4	46,357	4.6
Green-winged teal	377	3.2			.85	6.9	41,329	6.7
Shoveller					1.22	6.6	4,841	10.4
Baldpate	334	4.2	203	3.5	1.50	10.7	16,163	12.7
Gadwall					1.58	8.9	5,445	17.4
Wood duck					1.28	8.4[3]	9,277	5.5
Redhead	471	4.7	400	8.0	1.76	19.7	12,987	12.8
Lesser scaup	738	3.9	522	3.9	1.48	8.0[3]	40,770	5.7
Canada goose					4.00	45.3	15,076	15.7
Coot					1.25	3.4	3,291	4.1[4]

[1]Cartwright and Law, 1952.
[2]Hickey, personal communication.
[3]Bellrose, 1953, and personal communication.
[4]Mann, Thompson and Jedlicka, 1947.

also increases. If we considered only the three species represented by the largest sample, namely mallard, pintail and blue-winged teal, we should see a perfect straightline relationship.

Hickey's figures do not include the Ducks Unlimited data and correlations of my information with these two independent sources of data make it seem all the more convincing that fluoroscopy and band recoveries are measuring related aspects of hunting pressure.

The exceptions to this general relationship now take on increased interest. It is apparent that the point for the Canada goose does not fall on the curve with the ducks—frequency of occurrence of body shot in this species far exceeds that anticipated from the band recovery rates. In other words, geese get hit, live, and carry away the token of their experience without coming to bag as readily as do ducks.

Relation of waterfowl target size to incidence of body shot. It is reasonable to assume that the larger a bird the greater its chance of acquiring body shot when fired upon from any given distance. Since all birds are fired at from all conceivable distances and angles, it follows that larger species should be more frequently found with body shot and should be carrying a greater average number of shot per bird. To test this hypothesis I computed the average target size considered as a silhouette presented to the gun by the formula $A = W^{2/3}$, A being the relative area of the silhouette and W the average weight for the sex and species. The assumption is that within a closely related group of birds, such as waterfowl, the specific gravity and shapes are so similar that the relative silhouette areas can be accurately computed from average weights. The results are given in Table 5.

Examination of the last column in this table shows that males exceed females in size from 6-17 per cent among the various species of waterfowl and by 25 per cent in pheasants. This percentage difference completely accounts for the greater frequency of occurrence of body shot in males of such species as Canada goose and grey lag (shown in Table 2) and explains half of the difference between the sexes of pink-footed goose, mallard, and blue-winged teal, and part of the sex difference in pintail and green-winged teal. The difference between the sexes which is not explained by target size I believe is due to the greater carry-over of males from year to year, for Bellrose and Chase (1950) and Hickey (1952) have shown that mallard drakes live longer than hens, even though they are more vulnerable to shooting. They believe that the high mortality of hens during nesting more than compensates for their lesser vulnerability to the gun. This was postulated much earlier by Mayr (1939), and I

TABLE 5. WEIGHT AND COMPUTED SILHOUETTE TARGET SIZES OF WATERFOWL AND PHEASANT. TARGET SIZE IS COMPUTED BY THE FORMULA: $A = W^{2/3}$, when A is relative Area of Silhouette, and W is Average Weight.

Species	Males: Number weighed	Males: Average weight	Males: Target size	Females: Number weighed	Females: Average weight	Females: Target size	Per cent ♂♂>♀♀
Canada goose[1]	216	8.38	4.32	184	7.05	3.68	17.4
Grey lag-goose[2]	81	7.52	3.84	80	6.80	3.59	7.0
Pink-footed goose[2]	231	6.09	3.33	232	5.49	3.11	7.1
Mallard[3]	2,998	2.80	1.99	1,794	2.50	1.84	8.2
Black duck[4]	506	2.83	2.00	447	2.34	1.76	13.6
Pintail[4]	188	2.09	1.64	143	1.76	1.46	12.3
Blue-winged teal[4]	496	.96	.97	233	.86	.91	6.6
Green-winged teal[3]	63	.85	.90	52	.72	.80	12.5
Shoveller[5]	90	1.40	1.25	71	1.30	1.20	4.2
Gadwall[3]	30	2.10	1.64	37	1.90	1.53	7.2
Baldpate[3]	47	1.90	1.53	35	1.80	1.48	3.4
Old squaw[6]	141	1.90	1.53	85	1.62	1.38	10.9
Redhead[5]	82	2.50	1.84	40	2.20	1.69	8.9
Wood duck[5]	248	1.50	1.31	163	1.40	1.25	4.8
Pheasant[7]		2.81	1.99		2.00	1.59	25.2
Coot[4] (male & female)	719	1.39	1.25				

Average weights for the above species were derived from the following sources:
[1]Elder, 1946.
[2]Elder, In press—b.
[3]Various sources, including Hawkins and Bellrose, 1939.
[4]Mann, Thompson, and Jedlicka, 1947.
[5]Nelson and Martin, 1953.
[6]Ellarson, Personal communication.
[7]Smith, Personal communication.

believe it explains why more male ducks of several species are found with body shot than are females.

The importance of relative target size (Table 5) to the frequency of occurrence of body shot (Table 2) is shown by Figure 3. It seems that for most ducks there is a nearly straight-line relationship between the target size and the incidence of body shot in any given sex and species. In all ducks, excepting the old squaw, for which there are adequate samples (drakes of 8 species, hens of 5) there is a very close agreement between their size and the percentage carrying shot. This implies that in comparing species of ducks alone, during the brief period of this study, target size can account for most of the observed differences in incidence of body shot. This seems equally true for the cock pheasant.

The exceptions are again the most revealing. It is apparent that old squaws, coots, hen pheasants and all three species of geese are entirely different from the main group of ducks and are not being hit by shot as frequently as would be anticipated from their size alone. The reasons for this lesser vulnerability to the gun have already been discussed for the old squaw, coot and hen pheasant. Among geese I believe the fact that they are not carrying shot as frequently as expected is due to the greater proportion of out-of range shooting which is done with these big birds.

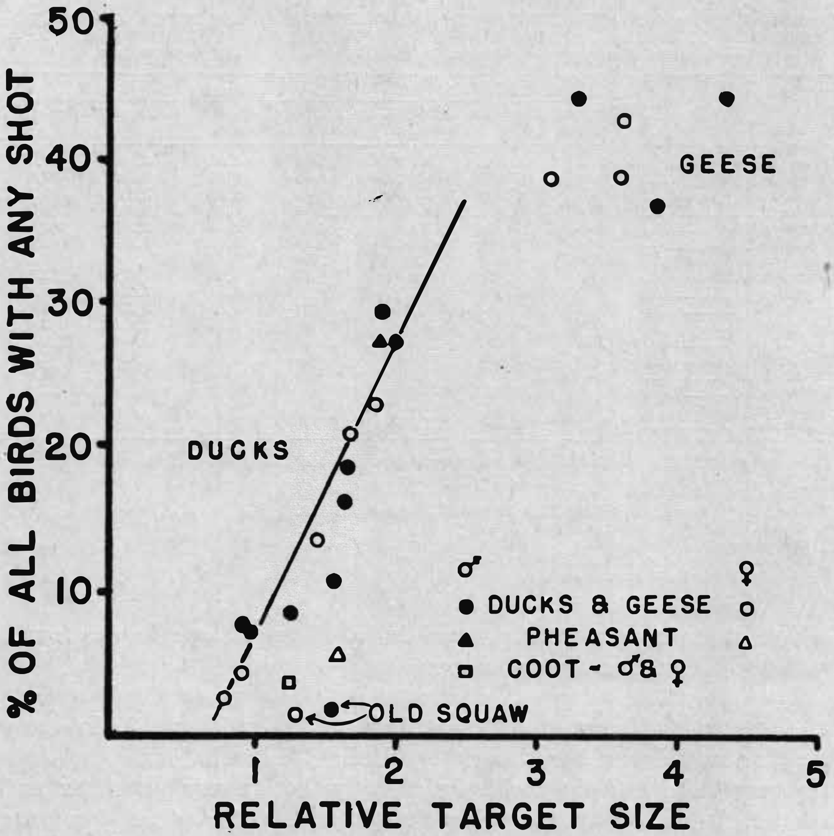

Figure 3. The relationship between relative target size and the incidence of body shot.

If target size controls the frequency of occurrence of birds carrying shot it might also be expected to influence the number of shot which an individual bird acquires. Therefore the number of lead shot found in each sex of each species was tabulated and graphed. Those for three difference size classes—teal, mallard, and goose—are shown in Figure 4. That for the mallard is very similar to Bellrose's figure (1953). Examination of these suggests that the larger the bird the more likely it is to have many shot and the less likely it is to have a high proportion with only one shot. Two measures were made for all species: The percentage of all birds carrying shot which had only one shot and the average number of shots found per bird (Table 6).

To discover the meaning of this distribution the percentage of birds

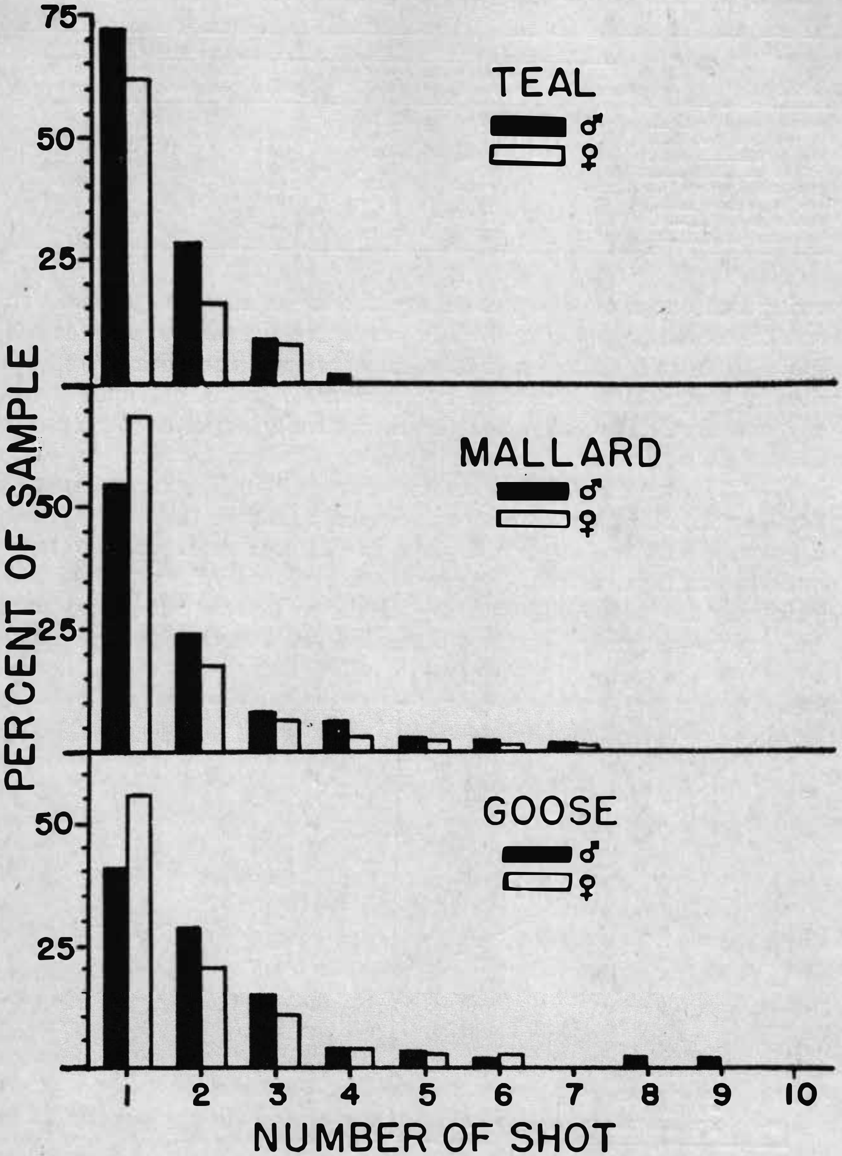

Figure 4. Frequency distribution of various numbers of shot found per bird in three species of waterfowl of different sizes.

TABLE 6. NUMBER OF BIRDS FOUND CARRYING SHOT IN FLESH, AVERAGE NUMBER OF SHOT FOUND PER BIRD AND PER CENT CARRYING ONLY ONE SHOT.

	Males			Females		
Species	Number	Per cent with only one shot	Average no. shot	Number	Per cent with only one shot	Average no. shot
Green-winged teal	33	82	1.24			
Blue-winged teal	144	72	1.46	14		1.43
Pintail	358	65	1.57	50	78	1.78
Mallard	686	55	1.95	186	68	1.59
Pink-footed goose	195	44	2.23	145	56	2.06
Grey lag-goose	30	43	2.50	30	42	2.00
Canada goose	195	41	2.86			
Pheasant	81	58	1.90	22	68	1.36

having but one shot was plotted against the relative target size for each of the sexes in figure 5. The curve for the observed data was drawn by inspection and a theoretical curve was then drawn to find whether target size controlled the percentage of birds found with only one shot. (The curve for the theoretically expected percentage is based on a relationship derived from the Poisson distribution, $y = xe^{-x}/1\text{-}e^{-x}$.) For the females the two curves coincide, for the males they do not. This means that for males, especially in the three species of geese, birds are found with only one shot more frequently than would be anticipated from their relative target size. The explanation again may lie in the difference in the distance from which they are fired upon—not only are they often too far away to get hit hard

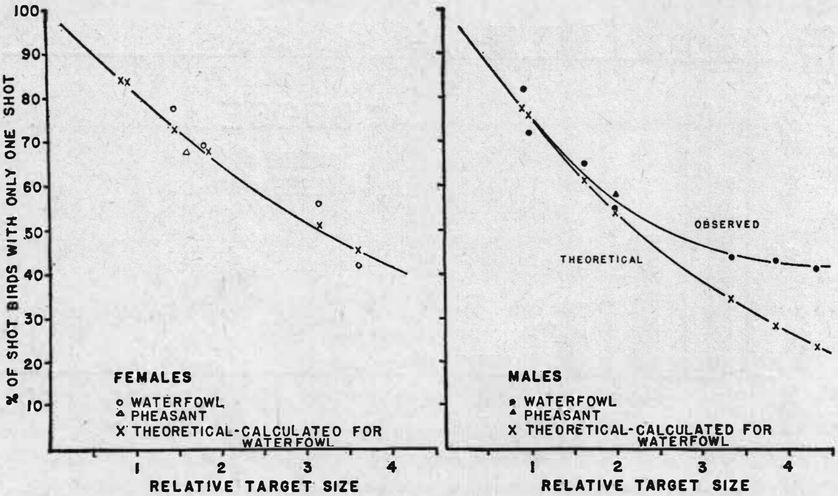

Figure 5. The relationship between relative target size and percent of shot-bearing birds of various species observed to have but one shot and the theoretical percentage expected to have but one.

enough for shot to penetrate, as mentioned above, but are also more likely to be hit by just one stray shot, than are ducks. I have no explanation for the failure of this to be revealed by the data for females.

Relative target size was then plotted against the average number of pellets being carried by birds that showed one or more pellets (Figure 6). Again, there is a clear-cut relationship: The larger the bird the

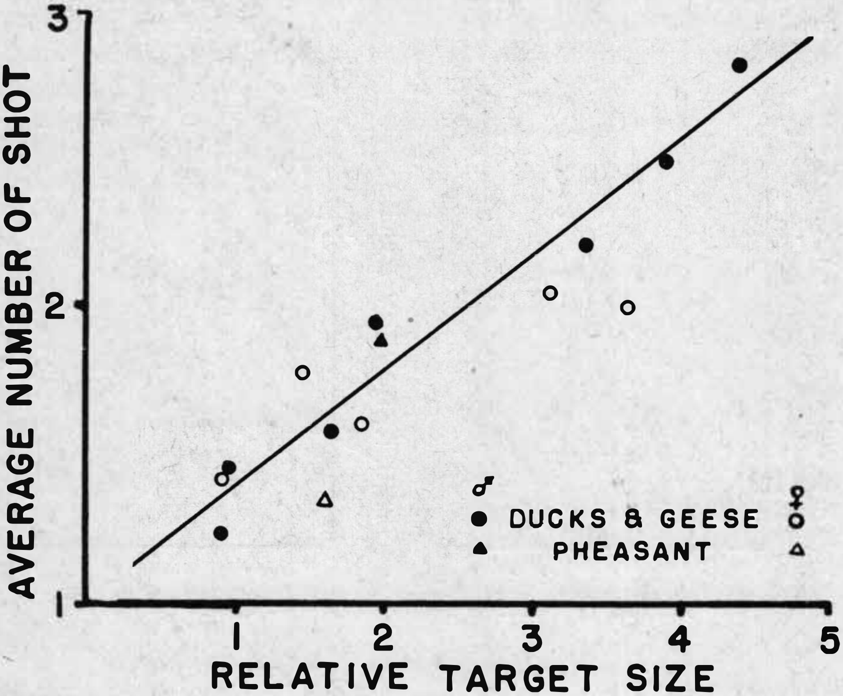

Figure 6. The relationship between relative target size and the average number of body shots carried by various species.

more likely it is to have a higher average number of shots embedded in its flesh.

Relationship between waterfowl target size and band recovery rates.—Since it was shown above that the percentage of birds carrying body shot, at least among most ducks, was directly proportional to the target size of the particular sex and species, the possibility that band recovery rates may also be an expression of target size is at once suggested. To analyse this relationship, the relative target sizes averaged for both sexes (Table 4) were plotted against cumulative recovery rates for both sexes in Figure 7. This produced the surprising discovery that at least for coots and the ten species of ducks for which sufficient data were available, target size influences the likelihood of a banded bird being recovered. This applies solely to interspecies comparisons and not to geographic or time specific comparisons within species .

To my knowledge this has never before been pointed out in analyses of band recoveries; it again shows that fluoroscopy and band recovery rates are probably related by their mutual dependency upon the variable of duck target size. The fact that goose data if plotted would

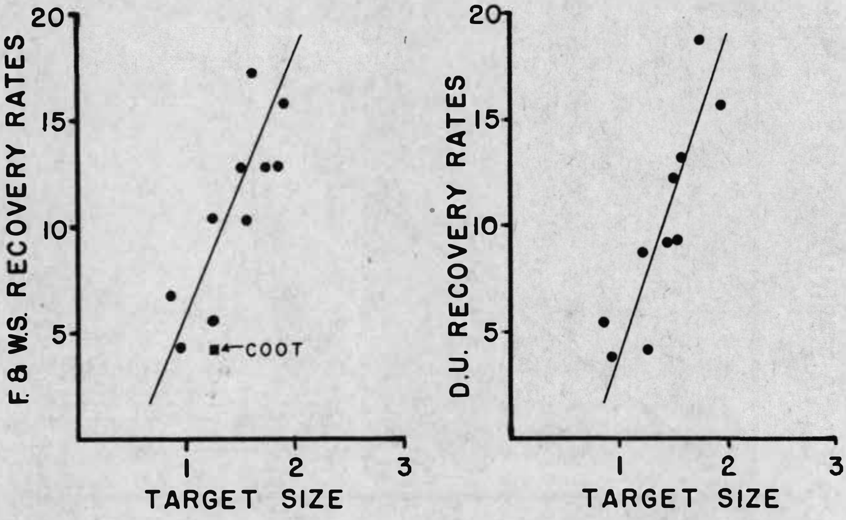

Figure 7. The relationship between relative target size and the average cumulative band recovery rates of coots and 10 species of ducks (sexes lumped).

fall so far from the curve that they would not be shown on the figure again suggests that these larger birds are in a different category of influences affecting rates of band recovery.

INTERPRETATION

The analyses in this paper bring us to the inescapable conclusion that fluoroscopy and band recovery rates have the same short-comings as measures of hunting pressure—they both correlate so closely with size of the bird that the simplest explanation seems to be that they are really but indirect measures of relative target size; at least this holds true for ten of the species of ducks examined. Interpretations of data from either source should be made in the light of the knowledge that size of bird may determine incidence of body shot and band recovery rate. Such an approach points up the significant deviations and shows that fluoroscopy figures for geese, old squaws, and perhaps coots, as well as female pheasants, are significantly different from the anticipated values. None is collecting shot as frequently as would be expected from examination of the curve representing the ten species aforementioned.

Despite this limitation imposed by the influence of target size, several important uses for fluoroscopy remain. One of these is the discovery of species or populations that do not fit the curve of anticipated values mentioned above. This procedure will point out the need for learning the causes of these deviations, whether they be biological or the results of shooting regulations and management practices.

Fluoroscopy is an effective means for testing the validity of contentions that there are unharvested segments of duck populations. This was the purpose of our examination of late-arriving mallards wintering in South Dakota. More than 1,000 mallards were examined at each wintering area (Lake Andes and Coxy's Lake) ; the frequency of body shot (males 29 per cent, females 22 per cent, Murdy, 1952) was not significantly different from that found elsewhere (Table 2).

It seems that fluoroscopy, like band recovery rates, can be used to measure broad trends in shooting pressure from decade to decade in the same species, and the effects of major changes in shooting regulations, such as a closed season in a flyway.

Whenever possible, fluoroscopy should be used to determine the effectiveness of closed seasons in protecting such species as whistling swans, Ross geese and sandhill cranes, for band recoveries can be of little value here.

Perhaps the greatest use of the fluoroscope is in detecting regional differences in shooting pressure both within states (upland game) and between major flyways (migratory species). One attempt at such a comparison is made in Table 7. Here the incidence of body shot in two species banded in greatest numbers during the same summer in widely separated areas is compared. In each case the sample from the Pacific flyway carried a significantly higher frequency of shot than did birds from the Prairie Provinces. The *chi* square method, using a 2-by-2 contingency table, was applied to test the significance of the differences between the populations and in each case showed the California sample to be significantly higher at the 99 per cent level of confidence.

This finding is of some interest when it is recalled that ducks from

TABLE 7. REGIONAL COMPARISON OF INCIDENCE OF BODY SHOT FOUND IN DUCKS IN THE SUMMER OF 1950.

		Males		Females	
Species	Source	Number Examined	Per cent with body shot	Number Examined	Per cent with body shot
Mallard	California	557	36.8	98	18.4
	Manitoba and Saskatchewan	463	29.8	101	33.7
Pintail	California	63	34.9		
	Manitoba and Saskatchewan	1,286	16.2		

the Prairie Provinces acquired their shot in the Mississippi and Central flyways while those in California in summer certainly were from the Pacific flyway. Some would question whether this indicates justification for the more liberal shooting seasons permitted in the Pacific flyway. I believe that more and larger samples should be taken and that to be conclusive, a coordinated effort by several of the states in each flyway must be made. In addition to the Michigan and Illinois studies already discussed, work with the fluoroscope is also underway in Utah, Colorado and New York (Heuer, 1952; Foley and Taber, 1952; Rielley, 1953; and Figge, 1951, 1953).

If an adequate sample (ca. 700 adult drakes) can be examined in each flyway each year we may soon have a much better idea of the comparative shooting pressures existing.

An extension of the use of fluoroscopy for examining regional differences is the comparison of species of similar size in America and Europe. Among three species of geese and three species of teal on the two continents there was found in each group a similar percentage of birds carrying shot; this is evidence for the balanced effects of social and mechanical limitations in Europe equalling the legal restrictions here. A sample of mallards is needed from Britain and elsewhere to prove that the Dutch sample I examined was representative of Europe. If so, European gunning on this key species is half as great as in North America. However, it is very likely that the thousands of mallards hatched in Dutch decoy ponds diluted my sample; for, although they were free-fliers, many had probably never left Holland and therefore had not acquired as many shot as the truly migratory mallards.

The rates with which young birds acquire lead pellets may prove especially useful in pinpointing the region in which most of them acquire their shot. This information can be used for immediate appraisal of new regulations or local management practices—such as opening part of a refuge to public shooting. Among fall mallards one half as many juveniles carried shot as did adults both in America (Bellrose, 1953) and in Holland (Table 3). Fluoroscopies performed south of these points in the same year would permit further localization of specific regional hunting pressures. Among teal the differences were greater than in mallards: one-fourth as many juvenile Dutch birds carried shot as did their parents and in Illinois only one-tenth as many. Perhaps that means that most of the hunting pressure on teal is applied south of these points, as suggested for this continent by Inigo's findings (1951) that nearly half the waterfowl bagged in Puerto Rico were teal.

SUMMARY

1. The field fluoroscopy equipment described in 1950 was simplified and reduced in weight; it proved especially useful during work in Europe the past year. It is adapted to handle both small ducks and large geese.

2. More than 21,000 birds have been provided for examination by cooperation with men catching waterfowl for banding by use of bait traps, corrals for driving flightless ducks, cannon- and rocket-propelled goose nets, and decoys with tolling dogs; by poulterers in London and Holland; and by death of large numbers of birds through natural catastrophes such as ice storms, drowning in fishermen's nets and botulism epizootics.

3. Yearly variations in percentage of birds carrying shot are seldom statistically significant, partly because of the large sample needed to detect differences of magnitudes so far observed and partly because yearly variations in shooting pressure are masked by carry-over of an unknown segment of the population already bearing shot.

4. Comparison of the species of ducks shows the big and popular mallards and black ducks leading in frequency of birds found carrying body shot, with teal and coots falling at the bottom of the list. However, this sequence has little importance because it is mostly explained by differences in target size, as summarized below. The old squaw ducks showed less than 2 per cent carrying shot; this does indicate a very small harvest by Indians and little shooting pressure from white man.

5. Fluoroscopic examination of winter-killed pheasants in South Dakota showed that cocks had been shot as hard in the 20-day open season as were Mississippi valley mallards during the four-month waterfowl season. After correction for size difference in the two sexes, we conclude that pheasant hens are sustaining one-fourth as

much gunning pressure as cocks, even though protected by a completely closed season every year.

6. Among Britain's two most abundant wintering species of geese—the grey lag and pinkfoot—the percentage of adult males found with body shot (38 and 44) was approximately the same as among Canada geese in Missouri (47.0). Their open season is as long as ours in the Mississippi flyway. Their punt gunning and lack of bag limit or restriction on hours of shooting seems balanced here by our heavier armament and more mobile shooters, supplied with public shooting grounds adjacent to refuge concentrations.

7. Ducks from Holland's many commercial decoys showed that among European teal the incidence of body shot was the same as in North American teal. The large sample of mallards examined from Dutch decoys indicated that they had been subjected to about one-half the gun pressure that this species sustains in the Mississippi flyway. This difference may be the result of dilution of wild migratory birds with a large proportion of mallards reared in baskets on decoy ponds.

8. Cumulative band recovery rates for each species were plotted against the average percentage carrying lead pellets and a nearly straight-line relationship was found—as band recovery rates increased the incidence of body shot also increased. This indicates that the two methods measure similar aspects of hunting pressure. Canada geese are an exception—their incidence of body shot far exceeds the expectation from recovery rates, indicating that they can fly off with a wound that would bring down a duck.

9. Relative size of target presented to the gunner was computed for each sex of each species. Males exceed females in target size from 6-17 per cent and this accounts for most of the sex differences in the percentage of birds carrying shot. The remaining difference is probably accounted for by the greater average longevity of males giving them more years in which to acquire their pellets.

10. When the relative target sizes were plotted against the frequency of occurrence of body shot in the drakes of eight species of ducks and hens of five, as well as for the cock pheasant, a nearly straight-line relationship was found. This implies that for strict species comparisons among ducks their target sizes account for most of the observed difference in incidence of body shot.

11. Exceptions to this size rule are species sustaining truly different shooting pressures, not strictly proportional to size: coots, old squaw ducks, and hen pheasants carry shot much less frequently than anticipated from their sizes alone.

12. Two further measures were also found to have a high correlation with body size, namely the percentage of shot birds having but one body shot and the average number of shots found per bird. Males of three species of geese had more than the anticipated percentage with but one shot; this was attributed to the great ranges at which geese are fired upon.

13. Finally, band recovery rates were also plotted against relative target size and again it was found that size of bird is of great significance—the larger a species the greater the likelihood of its showing a high band recovery rate. Geese do not fit this relationship and are in an entirely different category.

14. Although coots are never aged nor sexed, cumulative figures for this species show that both the percentage carrying shot and the band recovery rates are lower than among ducks and that the percentage with body shot is lower than anticipated from the target size.

15. Among geese the percentage carrying shot exceeds that anticipated from recovery rates because these big birds don't come down when hit by shot as readily as do ducks. The percentage with body shot is lower than anticipated from their target size due to the great ranges from which they are fired upon. The open patterns of shot from guns at these distances account for the high percentage of shot-bearing geese that have but one pellet.

16. Fluoroscopy has proven a rapid method for determining relative shooting pressures on two or more populations of a species. Clamor for a special season on late-arriving or wintering mallards subsides when they are found to have lead pellets in their flesh as frequently as other populations of the species. It can be equally useful in appraising effectiveness of closed seasons on particular species such as wood ducks, swans, or Ross geese.

17. Perhaps the most interesting use of fluoroscopy is the possibility it provides for making flyway comparisons as a check on recovery rates and other measures of shooting pressures in these major administrative areas. First tests show that California mallards and pintails have been shot harder than Central and Mississippi flyway populations.

ACKNOWLEDGMENTS

I have worked with many people and have been aided by many agencies in the gathering of these data. I am especially indebted to H. Albert Hochbaum, Director of the Delta Waterfowl Research Station of Delta, Manitoba for continued advice and encouragement and for use of the Station fluoroscope. To John Lynch, Floyd Thompson, Art Hawkins and other members of the Fish and Wildlife Service banding crews, I am particularly grateful. In Wisconsin Bill Kiehl made available coots from his study, and Bob Ellarson permitted me to examine the old squaw ducks he had salvaged from Lake Michigan fishermen and also contributed weights for this species. In South Dakota Eldon Smith made all arrangements for the examination of their winter killed pheasants, and Ray Murdy provided the winter trapped mallards—both by permission of the South Dakota Department of Game, Fish and Parks. Ducks from California banding traps at Tule Lake Refuge were made available by A. W. Miller. Data for the Canada goose are from the files of C. E. Shanks, of the Missouri Conservation Commission. For statistical advice I am indebted to Edward Novitski of the University of Missouri Department of Zoology.

The work abroad would not have been undertaken without the help and encouragement of C. R. Gutermuth. For arrangements in Europe I am grateful to Richard Pough of the American Museum of Natural History and to Miss Phyllis Barclay-Smith of the Wildfowl Research Institute. For the opportunity to accompany the goose netting trips of the Severn Wildfowl Trust I am happy to thank Peter Scott and his able assistant, Hugh Boyd. All work in Holland was arranged and aided by J. A. Eygenraam of the Institute for Applied Biological Field Research.

Last and not least, I am grateful to my graduate students who have served so well as assistants: Glen Sanderson, George Brakhage, Milton Weller, George Dellinger, and Daniel McKinley.

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DISCUSSION

DISCUSSION LEADER CORNELL: Thank you, Dr. Elder. You have presented a most interesting thought there. If your fluoroscopic findings are related to target size, they are obviously related to band recovery rates; so, then, must we assume that band recovery rates are also related to target size. Are there any comments or questions?

DR. R. B. MILLER [Alberta]: I have two questions. Is it possible to infer from the percentage of birds carrying size what the percentage of successful kills was out of the population?

DR. ELDER: This matter was discussed by Frank Bellrose in his 1953 paper at these meetings, and I have nothing which would contribute any further interpretative data.

DR. MILLER: You found the same percentage of pheasants carrying shot as you did mallards in this area—27 per cent, I think. Would you be able to infer from that that there was the same proportion of the pheasants killed as there was of the mallards, or would you have to take into account the differences in the characteristics of the shooting of the birds?

DR. ELDER: I think there are undoubtedly differences in the way the birds are hunted and the ability of birds of different sizes to fly off with shot. If I could have shown the rest of my data, I would have shown you that the percentage of birds carrying only one shot is much higher, when they are shot at great distances, they are much more likely to be flying through an open pattern, and therefore would acquire only one shot at one time.

I think this would certainly be relevant to, and would probably control the proportion of birds which would be downed by any particular gunning pressure.

MR. J. V. K. WAGAR [Colorado]: I was wondering what thought you had given to hunter psychology. In our country many of our waterfowl hunters will pass up teal whereas they will shoot at a mallard rather eagerly. But when it comes to a goose, they will pour down a barrage which is really something. So I suspect you would get a distortion there based upon the desirability within a particular conutry. I wondered how much thought you had given to that in your analysis.

DR. ELDER: I think that is an important question, and I am sure that in the West where hunters have a chance to be selective, maybe it is more important than in the Mississippi Flyway. At least, in my experience, it seems that a hunter shoots at anything he gets a chance to.

In order to try to evaluate the significance of abundance of a bird, comparing the total figures as gathered from the Upper Mississippi Refuge with the data from fluoroscopy, we did not find that the two sets of figures fitted. Some species, yes; others not. In other words, birds are not being shot and killed and found in the bag in proportion to the per cent that we find through fluoroscopy at that particular point.

MR. DAVID MUNRO [Canadian Wildlife Service]: I wonder what could be considered the desirable method for selecting birds to be fluoroscoped?

DR. ELDER: A preliminary analysis of the size necessary indicated that to detect differences of 5 per cent, approximately 700 birds of the same age and sex would be needed. I believe this should be what we aim for.

MR. MUNRO: How about geographical distribution in selecting your samples? You mentioned 750 birds per flyway. Would they be selected by random distribution throughout the areas, or how?

DR. ELDER: The possibility of getting a random sample is, as you know, always remote. However, since we know that there is little homing of drakes to ancestral territories, as there is in the female, the opportunity to get random samples among males seems to be greater than it would be among hens. Consequently, we concentrated on adult drakes. I believe in that way we had a much better chance of getting a random sample.

DISCUSSION LEADER CORNELL: Were you thinking, in your question, of the possibility of a continued accumulation of shot as they pursued their course southward? Is that the idea?

MR. MUNRO: That was not the point. I was really thinking of the differences in hunting pressure as compared to birds in different parts of the flyway.

MR. BEN GLADING [California]: You mentioned that the indicated number of shot was heavier in your California sample—was that in pintail?

DR. ELDER: Yes.

MR. GLADING: The band returns on pintail indicate that they are very likely taken in the neighborhood of 7 or 8 per cent versus mallards in our flyway of about 15 per cent. How does that compare with other flyways as far as pintail are concerned on band recovery versus shot return? Is there any discrepancy there?

DR. ELDER: I am afraid I don't have any figures on band recovery for the two flyways at my fingertips. However, my findings held true for the mallard as well as the pintail.

DISCUSSION LEADER CORNELL: Are there any further questions?

If not, it might be of general interest if Dr. Elder would tell us just a little bit about the details of the tolling dog.

DR. ELDER: I suppose the oldest method of capturing ducks is that of a decoy. It is a large trap, a funnel really, which curves slightly so that the birds can be lured into it by means of a tolling dog which appears and disappears around a series of wickets. This little dog attracts the ducks over the decoy flock which is always on the pool. The semi-domesticated mallards follow the decoy; the wild ducks follow them. The man runs back behind the wicket and shows himself. The domestic ducks sit still; the others flush down toward the end. He pulls a long string and drops a gate behind the ducks.

TECHNICAL SESSIONS

Tuesday Morning—March 15

Chairman: Oliver H. Hewitt

Associate Professor, Department of Conservation, Cornell University, Ithaca, New York

Discussion Leader: Trusten H. Holder

Federal Aid Coordinator, Game and Fish Commission, Little Rock, Arkansas

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UPLAND GAME RESOURCES

IS THE HEN PHEASANT A SACRED COW?

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The biologist who must teach game management principles to show the necessity for harvesting doe deer may have his pupils respond with the embarrassing question, "Then why can't we kill hen pheasants?" He may even ask himself, "Why do management principles governing the hen pheasant conflict with those that apply to all other game species?" Quail, grouse, ducks and all other game birds are hunted without regard to sex and they continue to thrive and produce a good kill, yet the hen pheasant is given almost complete protection in all states and Canadian provinces at the present time.

Hen pheasants were hunted in a number of states prior to 1940 and were harvested in large numbers from the Dakotas during the period of pheasant abundance. Although many still believe that these liberal hen kills caused the pheasant decline, the technicians (Kimball 1947) who studied the problem found no indication that hunting was responsible. Hens are still taken annually on Pelee Island in Ontario (Stokes 1954), and they can be legally killed in some of the poorer pheasant range in the State of California. The Pelee Island hen season is maintained to regulate the pheasant population in order to prevent damage to agricultural crops. The season in California is justified on the grounds that the poorest pheasant habitat in the southern part of the state must be maintained almost wholly by liberated birds, and low natural production does not warrant the protection of hens.

Biologists do not all agree on why the hen pheasant should require this type of complete protection from hunting. Some have suggested that all hens are needed for breeding stock and that there is no surplus produced. Others feel that there may be a surplus of hens only during peak years; thus it is only at rare intervals that hens could be killed. It has also been suggested that the hen is more susceptible to predation losses and for this reason cannot stand a hunting season. There are also those who feel that there may be a surplus of hen pheasants, but the complexity or difficulty of securing adequate control of the kill does not justify opening the season.

To check the validity of these various opinions it will be necessary to analyze the principles that control all animal populations. The number that can be harvested from any game population is determined by the surplus that is produced annually. A surplus of females is present only when a species is at the point commonly known as "carrying capacity." If the habitat bucket for pheasants is full, there should be enough hens and cocks produced for both to be harvested. Therefore, it is very important to know when pheasant populations are at carrying capacity. To discuss this question intelligently it will be necessary to determine what carrying capacity is. Allen (1953) states that "a particular land unit has a well-defined and limited carrying capacity for each species." Leopold (1933) defines carrying capacity as "the maximum density of wild game that a particular range is capable of carrying." Under this definition there may be different capacities for every month of the year as seasonal changes occur in food and living conditions. There are also different carrying capacities for different years, as no two are identical.

For this paper I would like to qualify this definition further by stating that "carrying capacity is the maximum density of game that a particular range is capable of carrying to the next breeding season." The greatest number that can survive to the breeding season is the carrying capacity of a given range for that particular year. This is the important capacity in that it is the end result after all of the environmental pressures have taken their toll. This final or breeding season abundance also is the most important factor in determining the production of young for the following year.

Carrying capacity is a quality of the range or area in which a particular species of bird or animal lives. We often refer to it as habitat carrying capacity, but in so doing we consider more than just the food, cover and water that exist in the area. All of the environmental pressures, such as weather, disease, predation, and accidents, have a part in determining the habitat capacity. The presence or absence of disease organisms in the soil can markedly influence the population levels that animals maintain in those areas. The reduction of predators or even the control of illegal hunting might be classed as habitat improvement under such a broad definition. For this paper I will consider that habitat includes all environmental pressures affecting the abundance of animals, except legal hunting. As hunting is a readily controllable management tool, it should be considered separately from the other factors that limit populations.

We might assume that a newly introduced species of bird or animal would increase rapidly with a "sigmoid" type of growth curve, levelling off at a more or less constant carrying capacity. If this were true, it should be comparatively easy to recognize capacity populations as those in which the broodstocks remain relatively constant from year to year. However, this condition seldom occurs because environmental pressures or habitat conditions are constantly changing. This greatly complicates the problem of determining when a population is at carrying capacity. To establish some type of rule or set of conditions for determining when capacity is reached, I would like to propose the following tests. These are generalizations which should apply to any unhunted population.

1. A population that maintains a constant level of breeding season abundance is at carrying capacity, if density-dependent factors are involved in any of its losses. Density-dependent losses are those which increase in pressure as the population rises and decrease as numbers diminish.
2. The breeding season population may increase and still be at carrying capacity, if density-dependent losses are involved, and if environmental pressures are reduced to allow such an increase. In other words, the carrying capacity may increase and population pressures fill the enlarged living capacity.
3. A breeding population that goes down in numbers due to losses that are at least in part density-dependent must still be at carrying capacity even though their numbers are much reduced.

Density-dependent losses cause the "break" in the increase curve in any newly established population, and they set limits for all animal abundance. They are the great population stabilizers that regulate all living things. A stable or declining population that involves these losses must be at capacity. An increasing population with such losses may still be growing and only approaching capacity, or the increase may be merely the result of a capacity change.

Even with all of these tests it is still very difficult to determine whether a population is at capacity, because we cannot accurately classify losses to determine which are density-dependent. Thus, the best indication of capacity is secured from an analysis of what must be true if animals are not at capacity.

If the above statements are correct, we may also conclude that *a population that is below carrying capacity and still does not increase must have all of its reproduction harvested by density-independent losses.* Such losses take a constant percentage of the population regardless of abundance. Smith (1935) proposed that density-independent factors alone could not control a population over a long period of time. This was again stressed by Ricker (1953) when he stated, "If only density-independent causes of mortality existed, the stock can vary without limit and must eventually by chance decrease to zero." This rule can best be demonstrated with the diagrammatic population balance shown on Figure 1. From this diagram it is apparent that a population that is not at carrying capacity and still

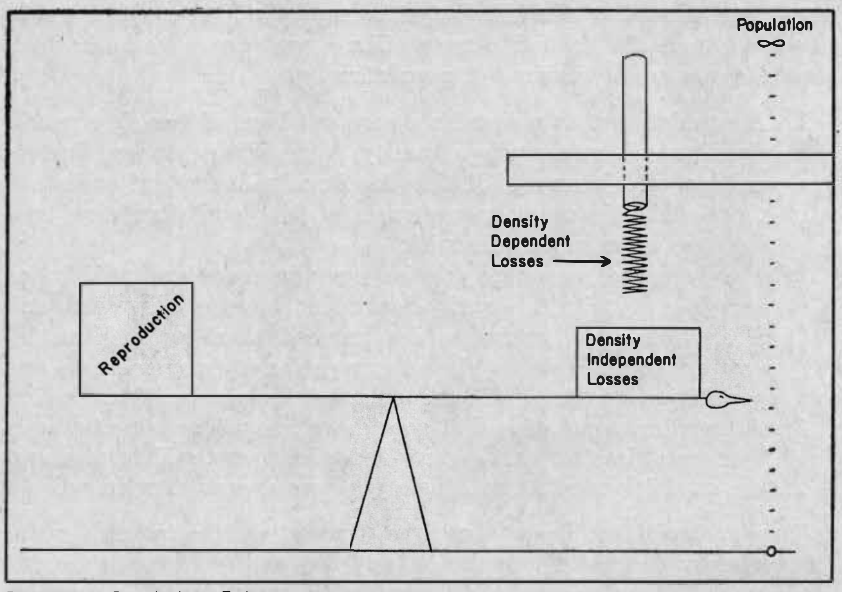

Figure 1 — Population Balance

does not increase must have density-independent losses which by chance exactly equal the annual reproduction. It should be obvious that such chance coincidence cannot continue to occur over a period of years. Thus the population level must either drop to zero or move up to the density-dependent spring which causes carrying capacity. Obviously, no population can increase forever, with the result that most animal numbers remain at least relatively constant. They move up and down to a degree, but they fluctuate within the range of density-dependent losses. A population that cannot maintain its numbers at carrying capacity has no margin of safety to avoid being eliminated entirely by some minor catastrophe.

How do these principles generally relate to the problem of killing hen pheasants? It would seem that the pheasant, which has been established on the North American continent for over 50 years, must be at carrying capacity on most of its ranges most of the time, or they would have been completely eliminated at some time during this period. The suggestion that pheasants are at carrying capacity only on years of peak abundance would imply that the entire pheasant population is in a very precarious condition. However, if they are at capacity most or all of the time, it seems reasonable to assume that a surplus of both hens and cocks would be available for harvest. Allen, in his booklet *Pheasants Afield,* concludes that there undoubtedly is a surplus of hens which theoretically could be harvested, but he stresses the dangers of overharvest. Shick (1952), in his publication on the Prairie Farm in Michigan, concluded that "13 to 15 birds per 100 acres represent the normal spring carrying capacity of the Prairie Farm." However, he believed that a larger number of pheasants can be carried in an emergency. Stokes, working on Pelee Island pheasants, was unable to detect any definite carrying capacity, but he did find that brood survival was lower on Pelee Island than on other areas where pheasants were less abundant. This might indicate that a density-dependent loss factor was coming to bear on the population and that they were approaching capacity even with the heavy harvest of hens.

The status of the pheasant population, with regard to carrying capacity, has an important bearing on management practices that may be designed to increase that population. For example, the fall planting of hen pheasants would have no value if the existing population is at carrying capacity. Merely to throw more game into a full habitat bucket is to waste it. However, if the populations are not at capacity, the number present in the spring would be in direct proportion to the number available in the fall; thus the fall planting of hens would be a good management tool in that it would increase the breeding population. If a capacity bottleneck is reached during the rearing season, rather than during the winter, the fall planting of hens would be of no value, because such a population would already have a surplus of breeding birds. Most pheasant studies have failed to show any real benefits from liberating more hens into an already established pheasant population. This would seem to be further proof that such populations are already at carrying capacity.

Another generally accepted management practice for increasing the supply of game birds is habitat improvement. Many states are working on such programs at the present time, and there are numerous examples in the West where such work has resulted in the production of more pheasants. Probably one of the best examples is found in the Columbia Basin area of eastern Washington where new irrigation projects have created vast new areas of pheasant habitat. The existing pheasant populations in most areas are increasing just as rapidly as the habitat is being improved. However, if the pheasants generally were not at carrying capacity, any type of habitat improvement should be of little or no avail. There would be little value in making the habitat bucket larger if pheasant reproduction could not keep the present bucket full.

Much confusion regarding carrying capacity is due to the fact that we do not understand how it operates on a pheasant population. Losses from starvation, malnutrition, parasites and disease are the obviously density-dependent factors that control the abundance of most animals. We might assume that, if pheasants are at carrying capacity, they too must be dying of starvation or disease. However, most studies have shown that food shortages and disease are almost non-existent among wild pheasants. Their food, which is largely seeds, is highly nutritious so that birds that are able to secure an adequate supply are generally in good condition. The pheasant is also a strong, mobile bird that can forage great distances.

Most research studies have concluded that predation and accidents are the principal known causes of pheasant loss. Latham (1951) classes predation as a density-dependent loss that helps to regulate most animal populations. However, the relationship between predator and prey is very complex, and it is my opinion that there can be both density-dependent and density-independent predation. If a predator, such as a red fox, specifically hunts for pheasants, the resultant losses are density-dependent, because a reduction in the pheasant population would undoubtedly force the fox to give up pheasant hunting and pursue other sources of food. There is also the possibility that a high pheasant population might attract predators to an area and thus increase loss pressure. However, there seems to be little indication that such predator concentration occurs. Winged predators which are most mobile could gather in large numbers where pheasant abundance is high, but this does not seem to be true on Pelee Island where Stokes reports high pheasant densities with a very moderate predator population.

Some predation occurs at all levels of pheasant abundance and undoubtedly must be independent of density. The predator that forages generally, hunting rabbits, pheasants, mice and song birds, has a density-independent effect on pheasants. Even though they may become very scarce, they will still be taken incidentally to the hunting of other game and buffer species. Thus a predator has a density-dependent effect on pheasants only when it is specifically hunting them.

Accidents are another source of game loss that are generally considered to be density-independent. If lightning strikes one per cent of the pheasants in the course of a year, this loss will remain one per cent whether pheasants are abundant or scarce. However, there is a relationship between food-and-cover and accidents-and-predation that is density-dependent and this causes some accidents to be density-dependent. Both accidents and predation are the result of hazards and pheasants avoid danger by remaining in or near protective cover. Thus the proximity of food to cover is very important in reducing hazard. MacMullan (1954) implied this when he indicated that "standing corn close to cover assures winter survival." The danger experienced by most birds or animals is that which occurs while securing food and is a hazard of *time* and *distance*. The time that the bird must be away from cover in search of food and the distance that must be travelled from protective cover determine the amount of exposure to predation and accidents. A small number of birds in a given unit of cover could secure food close at hand while a large population in the same habitat would be forced to forage much farther and for longer periods of time. There are countless examples of excellent cover that are not utilized because food is not readily available. Birds could live in such cover and secure food at greater distances, but their absence is proof that the hazards are too great.

Accidents as well as predation are involved in these losses. For example, the bird that must go back and forth across a highway every morning and evening in search of food is subjected to great danger. This would be unnecessary if adequate food were available closer to cover. This hazard involved in securing food should probably be classed as food shortage even though it involves only that food properly located in relation to good cover. Undoubtedly, this is one of the most important density-dependent loss factors affecting pheasant abundance.

Another source of confusion comes as a result of the variations in pheasant abundance which seem to occur with little or no environmental change. These fluctuations are very similar to the grouse cycles which are still largely unexplained. The experiment of grouse hunting in Michigan (Laycock 1953) seems to indicate that their fluctuations are changes in carrying capacity, since harvest continued through the low of the cycle apparently did not delay the recovery, or lower the subsequent peak of abundance. Thus, it is quite possible that pheasant peaks and depressions are also the result of capacity changes. These highs and lows of pheasant populations may be the result of environmental changes which alter the carrying capacity and thus change the abundance of breeding birds. Such changes in carrying capacity might be the result of severe winter conditions which make less food and cover available, thus reducing the number that live through to the breeding period. However, it is also possible to have extreme variations in pheasant abundance due to changes in breeding success without any actual change in carrying capacity. This is indicated by Shick's figures for Prairie Farm pheasants, as shown in Table 1.

It will be noted in this table that the breeding population (carrying capacity) remained relatively constant at approximately 12 birds per 100 acres for the years 1940-41-42, but the kill varied widely during this same period, going from 12.6 to 20.6 cocks per 100 acres. Actually, the year having the lowest breeding population produced the largest kill. Apparently, most ranges will support only limited amounts of "seed" stock, but even a small crop or a poor nesting season is adequate to "get the seed back." Thus a bumper crop or a small crop can make a tremendous difference in birds available for harvest; yet they may not represent any change in, or any departure from the carrying capacity.

There is also some indication that marginal pheasant ranges are more inclined to be cyclic in production than are the better ones.

TABLE 1. PRAIRIE FARM BREEDING POPULATION & KILL (SHICK 1952)

Year	No. of Hens Per 100 Acres (May)	Cocks Killed Per 100 Acres
1940	12.7	12.6
1941	11.4	20.6
1942	13.9	16.9

Many of the good pheasant areas of the West are irrigated lands which seem to produce a fairly consistent crop of birds from year to year. The climate here is generally warm and dry during the nesting and rearing season, with ample water provided through irrigation. This produces a somewhat stable type of climate resulting in a constant bird production. Many of the poor ranges seem to be less consistent in their production.

Table 2 compares the Prairie Farm kills, as shown by Allen, with computed kill figures for the entire state of Michigan. This indicates that although the Prairie Farm production has varied widely from year to year, the general trend has not coincided with that for the entire state. It would seem that the Prairie Farm, which represents the best habitat, was not responsible for the much higher production in the years of 1943 and 1944. This may imply that average or poor habitat cause the peak.

Some of this increased kill from low populations may give an

TABLE 2. MICHIGAN PHEASANT KILLS, 1938-1944

Year	Prairie Farm Kill Per 100 Acres	Computed Total State Kill
1938	14.8	921,018
1939	15.6	907,848
1940	12.6	990,419
1941	20.6	1,254,725
1942	16.9	1,200,595
1943	12.6	1,368,039
1944	17.6	1,401,076

exaggerated picture of the true population increase. MacMullan has shown that it is virtually impossible to harvest more birds when they are down to two cocks or less per hundred acres. Thus, a range that increased its production from two to four cocks per hundred acres actually has increased its harvestable surplus from nothing a year to two birds a year. Similarly, a 50 per cent increase from four to six cocks per 100 acres may result in a 100 per cent increase in kill. Undoubtedly, these low density populations contribute much to the harvest in peak pheasant years.

Since the Chinese pheasant is an exotic bird, still reasonably new to many of its present ranges, there is the possibility that some birds may be outside of their proper environment. They may be temporarily established in areas where they cannot continue to survive naturally. High reproductive pressures on peak years may also maintain such temporary populations in otherwise sub-marginal habitat. Any density-independent loss which removes all or more than the annual increase might be classed a catastrophe loss. A healthy game population can weather such losses now and then, but if they occur often, we may conclude that the environment is not proper.

Juvenile losses, due to inclement weather, are density-independent and could be of catastrophic nature. It is quite apparent that poor nesting seasons, or heavy losses of young, cause poor hunting seasons, but there is little data to show that these reductions are actually reflected in the subsequent breeding populations. It was shown in Table 1 that poor production did not appreciably change breeding populations on the Prairie Farm during the years studied by Shick. Studies of weather and pheasant populations in Iowa (Kozicky, et al., 1955) show a correlation between June temperatures and fall pheasant numbers, but in only one year (1947) of the 17 studied did the reduction appear to be in the nature of a catastrophe. Possibly during this one season reproduction did not keep the carrying capacity filled, and there was no surplus of hens. Apparently persistent renesting has prevented any real reproductive failures.

Those who contend that losses to hen populations are so heavy that they cannot support a kill apparently do not understand population dynamics. Hens must die as rapidly as they are reproduced or they would eventually cover the earth. Losses can never be prevented, but they can be channelled into productive uses. Hunter harvest can replace much of the natural loss of hen pheasants. Theoretically, hunting kill could replace all of the density-dependent losses and part of the density-independent losses. Obviously, the hen pheasants cannot support a removal of 70 per cent to 90 per cent of their number annually, as is the case in some cock seasons, but it is possible that a hen harvest might approach the cock kill in numbers. Stokes reports a Pelee Island kill of 14,578 cocks and 12,581 hens in 1950. However, it seems logical that hen harvest should start with a minimum kill and build up gradually to permit study of the results. Possibly a harvest of one hen for every two cocks killed may be the maximum that could safely be taken. This would still result in a 50 per cent increase in the total pheasant harvest.

There are several methods available for controlling the kill of hens during an open season. Special tags could be issued with a limit of one per hunter per season. Hunting of hens could also be confined to a very short period such as one day, or even one-half day. Areas having a long pheasant season could allow one hen in the daily bag on the last day of the season, and if this results in an inadequate kill, the number of days could be increased gradually. With these and other possibilities there should be no real problem in controlling the harvest of hens or in limiting the take to any desired number.

CONCLUSIONS

If the pheasant were a monogamous species, a cock mating with only one hen, the present management policy would preclude any hunting season whatsoever. The same reasoning that justifies the present hen closure would make it necessary to close all pheasant hunting. Many states have had experience with game populations that were given complete protection for years, assuming that they would increase in numbers. When it finally became apparent that this policy was fruitless, it was generally found that a harvest that included both sexes did not reduce the numbers. Undoubtedly, pheasant populations would react in the same manner.

Since pheasants do not "overgraze" their range, and because they seldom are numerous enough to damage farm crops seriously, there is a general feeling that there is no need to kill hens. It's the same old problem encountered in setting doe seasons: "If they aren't causing damage, why kill them?" This type of reasoning does not recognize the fact that intensive management should include a full utilization of all surpluses.

In the past it has been assumed that the killing of hen pheasants will reduce the population the following year. This is based on the belief that the birds are not at carrying capacity. However, most available evidence indicates that pheasants as well as all other forms of wildlife maintain their numbers at capacity most of the time. There must be a surplus of hens available almost every year which could be taken without any effect upon populations of subsequent years. They can add materially to the pheasant harvest. It is my opinion that there is no good reason for continuing to maintain the hen pheasant as the "sacred cow" of North American game birds.

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DISCUSSION

DISCUSSION LEADER HOLDER: Thank you, Mr. Lauckhart. Now, if there happens to be a real Southern gentleman in the crowd, now is the time to rush forward and defend the female sex. Mr. Lauckhart has suggested that at least some, and perhaps up to 50 per cent, of the female pheasant population ought to be harvested along with the male population. In doing so, he has prepared a very fine review of some of the principles of game population.

DR. JOSEPH J. HICKEY [Wisconsin]: There is one other principle of harvesting which the speaker did not mention, and that is the possibility that a breeding population can be depressed in a manner which will increase the rate of reproduction of game. This is the second principle at stake in harvesting populations. The principle was expounded at length by Errington in 1945. It's been experimentally demonstrated by Glading and Saarni in 1944.

Now, as far as density independent factors are concerned, we have always had pheasant populations at the periphery of the pheasant range which are controlled by density independent factors. The question now comes up, if you have density independent factors controlling the marginal populations, should you improve the habitat?

You see, our speaker has said that you shouldn't. I think he left that inference; but if the density independent factors can be controlled, habitat restoration is in order.

Now, I would say that California quail probably are controlled by a density independent factor, in terms of moisture, and in so far as the pheasant range the limiting factor is probably a complete lack of food. Whether this is a density dependent factor or not, I don't know.

In some cases we certainly could give pheasants cover which would give them a better chance to withstand some of these blizzards which we get in the northern part of the country.

DISCUSSION LEADER HOLDER: One of the points which Mr. Lauckhart did not cover in his paper from the floor, he did cover in the written text, which I happen to have seen, and I'm going to call on him to answer that question.

I would like to point this out, though, that it's in this area where you have just about two pheasant cocks per hundred acres and you have an increase in the population. There, the increase in the population has been hunted and the harvest has been built up to a great extent.

MR. LAUCKHART: It seems there were two or three questions involved there. One point that we are bringing up is that the low populations don't have to worry about hunting pressure, because of the law of diminishing returns. That is, the harvest is always poorest where population is lowest.

Regarding whether density factors are dependent or independent, I consider food and water both density dependent.

I have discussed it in the paper. It is the matters of time and distance; that is, water may be available and food may be available, but is it close enough to the other requirements of the species? For example, the pheasant may be able to go from cover to the food, but it may have to go such a long way and spend such a long time hunting for food that it is exposed to so much hazard that it will not survive; and that is a density dependent loss.

I presume that wouldn't apply with water. They wouldn't drink all the water close at hand, and have to go farther, but that might be true with food.

ROBERT MCCABE [Wisconsin]: I don't want you to think that Wisconsin is ganging up on you, but I have two questions, and I think these are quite basic to your problem.

On a theoretical basis, I'm inclined to agree with you, but I'd like to ask you specifically, since you state somewhat categorically that habitat improvement works—I think this can be shown for some species, but do you have any information that would tie this in with pheasants? Do you know of any place where X amount of habitat improvement has increased the bag of pheasants in any specific amount?

MR. LAUCKHART: Yes, I think in the State of Washington we probably have one of the best examples of habitat improvement in the Columbia Basin, where they are turning sagebrush land into good farming land and bird land. The birds keep up with the farmers planting their crops, and the population builds up just as fast as the land develops for them. I think the Reclamation Service is doing habitat improvement service for us in Washington, and they are building wonderful pheasant populations without any help from anybody else.

DR. MCCABE: Is that habitat creation or improvement? I would like to make that distinction.

MR. LAUCKHART: Well, it's all a matter of degree, whether you improve the habitat a little, or you create a new habitat. The pheasants were there. They didn't have to migrate from anywhere else. They were all through that sagebrush country, but the population was too low to make it productive to hunt.

MR. MCCABE: Well, No. 2, you said that you knew that stocking did not increase your population, and you said it somewhat categorically. This is another basic question: How do you know this?

MR. LAUCKHART: I was just presenting a general conclusion from other studies in the literature.

I could give you one example that we have in the State of Washington. We put in what we call a field-rearing project where we rear pheasants on the ground, and leave them there so we don't have to transplant them and put them in the new habitat.

We raised 1,500 pheasants right on top of one of our regular census plots that had been counted for the last six years. We didn't get one bird increase the following year from having liberated 1,500.

DR. ARTHER S. EINARSON [Corvallis, Oregon]: I think we are all agreed now that we can all go out and shoot pheasant hens. I think one thing that we have neglected in accepting hypothesis is perhaps the difference in the value of the hen and the cock bird.

In the survival studies on Eliza Island, we found that our winter losses of hens are very great, running from 40 to 70 per cent. We see the situation of the hen in the field before the guns and find that she is taken far more readily than the rooster.

We have been collecting birds on a study this winter. We have no trouble collecting the hens. We do have trouble collecting the roosters. The density there is about 4,000 birds on 2,000 acres.

We have the everyday situation of the traffic kills on the highways. In the two years' time we have been observing it, we have had merely a handful of cocks killed by cars, but in a one-mile stretch approximately 190 hens were taken in the same period.

For that reason, we aren't looking at the thing realistically when we merely analyze it from the standpoint of a theoretical problem. Further than that, we probably should be worried, too, about our over-all national picture on pheasant production. Are we condemned to provide hunters 40 per habitat acre? We find that the hunter can't tell the difference between 25 per habitat acre and 40 per habitat acre, but he says he can't find any of 5 per habitat acre.

I think we should go back to the situation of covering our entire United States winter range of the pheasant and decide: Can we, in some areas, utilize the limited habitat we have, the low-density areas, a little better?

The higher density areas, of course, will be the places where the hunter goes, and if we do not do this, we, as biologists, will be up against the situation that if we have not measured and watched our densities to permit people to know what the habitat will carry, released our information and followed it, we will some day be in the position where the hunter tells us that our population is extremely low in places, long before we see it ourselves.

MR. LAUCKHART: Thanks, Art. Art and I have argued this many times. There's one thing regarding the idea that it's easier to trap hens. It we killed off 80 to 90 per cent of the dumb hens, as we do the roosters, it would be much more difficult to kill hens. By the end of the season, roosters are pretty well educated. No wonder they won't go near traps!

Regarding this matter of losses of hen pheasants, the principle is that the hens have to die as fast as they are born. The more rapidly they are born, the more rapidly they must die. That's a general principle.

If they don't die as rapidly as they were produced, they would eventually cover the earth; losses can never be prevented. We talk about that, and we might be able to delay them a little bit, but it's temporary, like stopping the Mississippi from flowing by building a dam.

The only thing we can do is divert these losses into productive use, allow the hunter to utilize them.

DISCUSSION LEADER HOLDER: The last paper which was given in the other hall yesterday afternoon brought out, incidentally, that perhaps some of the hunters in some parts of the country are already herding the hen pheasant population into usage, even though they aren't doing it legally, because it was stated that by fluoroscopic examination they found that 27 of the cocks were carrying shot and about one-quarter of that number of hens were carrying shot.

ACORN YIELDS AND WILDLIFE USAGE IN MISSOURI[1]

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The great dependence of many game species upon acorns points up the need for information on yields of acorns, factors affecting these yields, and utilization of acorns. Management of forest game sometimes has been handicapped through the lack of such knowledge. In Missouri, acorns constitute an extremely important food for wildlife, for within the state there are 15,000,000 acres of forest land, mostly of the oak-hickory type. About 20 species of oaks, in addition to some 10 forms and variations and 16 recognized hybrids, grow within Missouri's boundaries (Palmer and Steyermark, 1935).

The purposes of this paper are threefold: to show the productive potential and factors affecting yields in certain oaks; to evaluate the importance and availability of acorns as food for various species of wildlife; and to report some effects of acorn-feeding upon wildlife. Although some new data are presented in this paper, much of the information from Missouri has been published in various forms having limited distribution. Since many of these works contain valuable acorn data incidental to the major subjects discussed, it is the objective of this paper to ferret out and draw together these findings pertinent to acorns in Missouri.

ACORN YIELDS

An adaptation of the open-quadrat and trap systems of sampling and classifying seed described by Downs and McQuilken (1944) was used in this study. Acorns were collected in traps, each being 3.3 feet square (¼ mil-acre) by 12 inches deep, and constructed of wood frames covered with ¼-inch hardware cloth on sides and bottom, poultry netting of 1-inch diameter on the top. The traps were designed to permit acorns to fall through the top into the trap, but to exclude deer, squirrels, and birds. It is assumed that the trapped acorns were accessible to mice. Traps were placed under a representative part of the crown, usually midway between the bole and the periphery of the crown. All acorns removed from the traps were classified by inspection and counted into two major groups, the im-

[1]A contribution from Pittman-Robertson Project 13-R (Missouri).
Data pertaining to the University Forest were furnished by the Department of Forestry, University of Missouri. The authors wish to acknowledge the assistance of Dr. Paul Y. Burns, Louisiana State University, who compiled and analyzed statistically the acorn yield data. Special thanks are due Dr. T. S. Baskett, U.S. Fish and Wildlife Service; Dr. R. S. Campbell, University of Missouri; and Messrs. B. T. Crawford and W. O. Nagel, Missouri Conservation Commission, for editorial assistance.

mature and the mature acorns. The mature group was sub-divided into three categories: 1) sound acorns; 2) acorns having insect damage; and 3) acorns showing signs of wildlife usage. Acorns were collected from units established in three different localities of the Missouri Ozarks—the Indian Trail State Forest and Wildlife Refuge in Dent County, the University Forest in Butler County, and the Drury Wildlife Refuge in Taney County. A more detailed description of sampling methods, location, and related findings was reported by Burns, Christisen, and Nichols (1954).

There were selected for study 163 oaks, representing five major species: (1947-1952): black oak (*Quercus velutina* Lam.); black jack oak (*Quercus marilandica* Muench); scarlet oak (*Quercus coccinea* Muench; white oak (*Quercus alba* L.); and post oak (*Quercus stellata* Wang).

The annual yields by species and localities are summarized in Table 1.

Averages for the localities are included, except for the Drury Wildlife Refuge, where data were gathered for only two years.

No clearly defined cycles of seed production were evident. It appeared that heavy acorn crops were produced at intervals shorter than five years. Complete failures did not occur in the Indian Trail Forest, as each species, except scarlet oak, produced some acorns each year. In the University Forest all three species had failure or near-failure in 1949. A good seed year for one species tended to be a good year for other species. The years 1948 and 1952 were generally good seed years, and 1949 was generally poor.

Annual yields of acorns varied considerably except in the case of

TABLE 1. AVERAGE NUMBER OF MATURE ACORNS PER TREE IN THREE EXPERIMENTAL FOREST STANDS—1947-1952

Forest	Year	Black Oak	Black Jack Oak	Scarlet Oak	Post Oak	White Oak
Indian Trail	1947	600	1,100	0	300	500
	1948	2,200	1,000	2,300	200	900
	1949	300	300	0	200	600
	1950	200	300	0	300	1,900
	1951	300	200	100	200	900
	1952	1,900	1,500	—	300	1,800
	Average	900	700	500	200	1,100
	Standard Error	200	100	300	100	400
University	1948	2,000		4,400		900
	1949	100		0		0
	1950	2,900		600		700
	1951	100		0		1,800
	1952	2,500		6.800		100
	Average	1,500		2,400		700
	Standard Error	600		1,200		300
Drury	1947	400	200		300	1,500
	1948	1,500	1,100		100	400

post oaks. Although lower in production, post oak produced more stable annual yields than did other species. The yearly variation was most pronounced in the scarlet oaks. This species was not sampled in Indian Trail Refuge in 1952, a year of heavy production. Had the 1952 yield been obtained, a higher computed average for the species would have resulted. Differences between species in the University and Indian Trail averages showed no significance when tested statistically at the 5 per cent probability level, except for post oak.

Variability in acorn production among individual trees was great. For example, in the Indian Trail Forest stand, the most prolific white oaks produced an average of 7,700 acorns per year, while the least prolific yielded only 100. Park (1942), in a study of yields and persistence of wildlife food plants, noted that in the percentage of plants bearing fruit the oaks as a group were lowest of all studied. Generally, in Missouri the good producers in a given year were also the good producers in other years; poor producers were consistent also. This is in agreement with Cypert (1951), Downs (1949), Smith (1929), and Wood (1934), who found some oak trees to be inherently good seeders and others inherently poor.

Simple correlation coefficients were computed between number of mature acorns produced and the d.b.h., the height, the crown length, the crown diameter, the crown surface area, and the radial wood growth. The crown surface area was computed from crown diameter and crown height, by assuming that the crown had the shape of a paraboloid. Most computed coefficients were small and statistically insignificant.

Crown diameter had the highest degree of correlation with acorn yield; wide-crowned black, black jack, and scarlet oaks tended to be more prolific seeders than trees with smaller crowns. Similar results were reported for post oak in Texas by Petrides *et al.* (1953). Trees with large crown surface areas generally yielded more acorns than trees with small crowns. The relationship between acorn production and d.b.h., total height, and crown length were weak, but large trees did show a tendency to be better producers. Apparently no relationship existed between rate of radial growth and acorn yields. Cypert (1951) believed that vigor, exemplified by radial growth rate rather than size of the tree, was important in attaining a heavy acorn yield. Moody (1953), in Louisiana, noted that large-diameter trees and oaks with dominant and co-dominant crown characteristics were better producers than others studied.

The value of acorn mast varies not only according to the size of the crop, but also according to the proportion of mature acorns.

TABLE 2. PERCENTAGES OF MATURE ACORNS FOUND, 1947-1952

Forest	Black Oak	Black Jack Oak	Scarlett Oak	Post Oak	White Oak
Indian Trail	62	54	69	44	73
University	90	—	77	—	94
Drury	75	69	—	49	83

Consequently, data bearing on the latter point were collected. They are presented in Table 2.

Less than half of the post oak seed matured; other species exhibited averages ranging from 54 to 94 per cent, with white oak having the highest percentage of mature acorns. The proportion of ripe acorns was somewhat higher in good seed years than in poor production years.

The proportion of ripe acorns infested by insects was of considerable magnitude, as is shown in Table 3.

Post oak acorns particularly were affected, with more than 80 per cent being wormy; other species averaged over 50 per cent infestation. Insect damage was less severe in years of good seed yields. Some data are available in the literature for comparison. In 1937, about 67 per cent of the acorns analyzed by Kautz and Liming (1939) from these same five species in the Clark National Forest of the Missouri Ozarks were infested with insects. In the Appalachians, Downs and McQuilken (1944) reported only 30 per cent of the mature acorns attacked by insects.

The species of insects causing the damage were not identified. Downs and McQuilken (1944) reported that nut weevils appeared to cause the greatest amount of insect damage. Korstian (1927) found three major group of insects attacking acorns in North Carolina—nut weevils, moth larvae, and gall-forming cynipids.

Apparently, the numbers of sound acorns for wildlife and for reforestation could often be doubled if acorns were free of insect attacks. This would be of particular benefit in years of poor acorn yields.

Mature acorns are of prime importance in regeneration of oak stands. Table 4 shows what proportion of the mature acorns were classified as sound during the collection period. Acorn soundness

TABLE 3. PERCENTAGES OF MATURE ACORNS DAMAGED BY INSECTS, 1947-1952

Forest	Black Oak	Black Jack Oak	Scarlet Oak	Post Oak	White Oak
Indian Trail	72	71	70	87	72
University	46	—	48	—	59
Drury	61	60	—	80	57

TABLE 4. PERCENTAGE OF MATURE ACORNS CLASSIFIED AS SOUND, 1947-1952

Forest	Black Oak	Black Jack Oak	Scarlet Oak	Post Oak	White Oak
Indian Trail	16	14	14	5	14
University	41	—	41	—	19
Drury	32	21	—	12	32

varied by species from 5 to 41 per cent. If the examination of acorns had been continued through the late winter and the spring, instead of terminating each January, an even lower percentage of sound acorns would have been found, as insects and wildlife continued to deplete the supply. The proportion of sound acorns was somewhat higher than average in years of good seed yields, particularly in the black oak group.

To calculate the acorn mast food supply available to wildlife, weights of acorns were taken. During the storage period of 2-3 weeks before weighing, acorns lost some moisture; hence the number of acorns per pound is greater than in fresh acorns. However, the results presented in Table 5 indicate the approximate number of acorns of each species equivalent to one pound.

Similar results were obtained by Dalke (1953) on acorns collected from the Indian Trail Forest, with one exception: He counted fewer scarlet oak acorns than white oak acorns per pound. Even closer agreement of weight equivalents from Indian Trail with Downs' (1944) Appalachian study was evident. The average number of acorns per pound recorded for oaks in the Appalachians was as follows: black oak, 241; scarlet oak, 194; and white oak, 166 acorns.

Assuming that all mature acorns not damaged by insects are available to wildlife, the average number of sound acorns produced per tree, by species, in the Indian Trail stand was as follows: black oak, 261; black jack oak, 189; scarlet oak, 146; post oak, 29; and white oak, 306. Actually, these figures are conservative. Acorns which were eaten in the entirety or carried away were not included; nor were some of the insect-damaged acorns which were of some benefit as food. However, Dixon (1934) found wormy acorns were rejected by mule deer in California and, according to Korstian (1927), un-

TABLE 5. AVERAGE NUMBER OF SOUND ACORNS PER POUND, 1947-1948

Species	Indian Trail Forest	Drury Forest
Black Oak	240	278
Black Jack Oak	422	357
Scarlet Oak	202	—
Post Oak	—	441
White Oak	169	104

sound acorns are rarely taken by rodents. Ritter (1938) concluded that wormy acorns were disregarded by the California woodpecker.

In conclusion, the production of mature sound acorns in this region of the Missouri Ozarks may be summarized by consideration of a hypothetical stand, consisting of 20 seed-bearing oaks per acre, evenly distributed among the five species studied. The average number of acorns available for food from this stand would be about 4,000 (3,720) per acre per year, representing a weight of 20 pounds. This yield is at variance with the findings of Downs (1949), who reported yields of 100 to 150 pounds of acorns per acre in southern Appalachian forests. Computations based on data presented by Dalke (1953) show that in 1938 the Indian Trail Forest produced approximately 5,600 mature acorns per acre suitable for food. The occasional poor seed year, however, is critical for mast-consuming wildlife species. The poorest year in the Indian Trail Forest was 1949; the assumed stand in this case would have a crop of only 2,200 acorns per acre available for food, or a weight equivalent of 11 pounds.

WILDLIFE USAGE

The importance of oaks, particularly the seed produced, for wildlife is demonstrated by the number of different animals using this plant. Van Dersal (1940) reported from a review of literature that 186 different kinds of birds and mammals use oaks and that, with the possible exception of *Rubus,* this number exceeded that recorded for any other genus of woody plants. Martin, Zim, and Nelson (1951) noted 96 kinds of animals in the United States which used oaks (mostly acorns) to an appreciable extent for food. This fact has not gone unnoticed by foresters who have viewed this utilization of acorns by wildlife with some degree of apprehension.

It was logical to attempt to measure the extent of acorn usage by wildlife in conjunction with the acorn yield study conducted in the Ozarks. The findings are conservative for three reasons, namely: 1) the study was not carried through the late winter and the spring, and thus total utilization was not measured; 2) some acorns were removed or consumed in their entirety by wildlife, so this fraction was not measurable; 3) the identifying fragments of usage, the acorn tips which were counted, did not account for all the used acorns. No attempt was made to classify usage by various wildlife species, although acorn fragments bear distinguishable marks made by different wildlife species.

The percentage of mature, trapped acorns, as recognized by fragments, used by wildlife ranged from 7 per cent to 22 per cent (Table

6). It was evident from the data that there was no consistent preference for any one (or several) species of acorn. A similar conclusion was reached by Downs and McQuilken (1944) in the analyses of usage of acorns by wildlife in the Appalachians. These observations are especially interesting in view of proposals which suggest differences in palatability of different species of acorns: acorns of the white oak group are reportedly ''sweeter'' than those of the black oak group. This observation is based presumably upon the findings that the black oak acorns differ chemically from acorns of other species in that they contain a much higher percentage of fat and somewhat more tannin than do white oak seeds (Korstian, 1927; King and McClure, 1944).

The figures shown in Table 6 are conservative, because the acorns were partially protected from wildlife by the traps. Mice could have gained access to acorns after climbing up the trap sides and through the wire cover. Acorn usage in the unprotected quadrats was about twice as heavy as in the traps, although these figures were considered

TABLE 6. PERCENTAGE OF MATURE ACORNS USED BY WILDLIFE, 1947-1952

Forest	Black Oak	Black Jack Oak	Scarlet Oak	Post Oak	White Oak
Indian Trail	12	15	16	8	14
University	13	—	10	—	22
Drury	7	19	—	8	11

even more conservative than the trap data for two reasons, namely: 1) acorns were exposed to ground feeders only for a short time interval, from dissemination until collection in January; and 2) the number of acorns taken in their entirety by ground feeders was unknown.

The main acorn feeders in the trees were birds and squirrels. Acorns and fragments dropped by these animals and caught in the traps were essentially a measurement of acorn utilization occurring in trees. Acorns collected from the open quadrats had been exposed to both the arboreal feeders and the ground eaters of acorns. Assuming that arboreal utilization of acorns over the trap and over the open quadrats were comparable, the difference between the open quadrat and the trap percentages would represent usage of acorns by animals feeding on the ground. These findings are presented in Table 7.

Using data secured from the Indian Trail Forest during 1947 through 1951, it was found that the average annual usage of acorns in trees (*i.e.*, traps) amounted to 13.6 per cent of the mature acorns produced. Terrestrial feeders accounted for 18.2 per cent of the

TABLE 7. PER CENT OF MATURE ACORNS UTILIZED BY ARBOREAL AND TERRESTRIAL WILDLIFE ON INDIAN TRIAL FOREST

Species	Usage	1947	1948	1949	1950	1951	Average
Post Oak	Arboreal	7.4	3.2	18.6	13.0	2.5	8.9
	Terrestrial	27.9	4.9	53.3	36.8	29.1	21.5
White Oak	Arboreal	11.0	5.7	30.2	10.4	10.8	13.6
	Terrestrial	38.2	13.1	80.6	66.1	56.2	37.2
Black Oak	Arboreal	1.9	10.6	23.6	9.9	9.9	11.2
	Terrestrial	6.0	14.6	29.3	14.1	12.2	4.0
Black Jack Oak	Arboreal	2.9	12.1	36.1	8.2	11.4	14.1
	Terrestrial	35.5	31.5	36.5	24.4	37.0	18.9
All Four Oak Species	Arboreal	9.3	8.8	29.2	10.2	10.5	13.6
	Terrestrial	16.7	10.6	15.4	25.9	22.7	18.2

ripe acorns. Generally, terrestrial usage of acorns was equal to or greater than that occurring in the trees, with the exception of 1949, a poor acorn year. Apparently the acorn feeders in the trees consumed the bulk of the crop before the acorns reached the ground feeders. Tree users in that year fed on 29.2 per cent of the acorns; whereas ground feeders used only 15.4 per cent of the mature acorns.

Analyses of these data on the basis of four oak groups (post, white, black, and black jack oaks) revealed the acorn usage to be fairly constant in the trees for all species. However, terrestrial usage varied from 4 per cent in black jack oak to 37.2 per cent of the ripe acorns in the white oaks.

These utilization figures are very conservative since acorns removed in their entirety by animals could not be measured. Cypert and Webster (1948) estimated that about 13 per cent of acorns produced by water and willow oaks were removed in their entirety from the trees by birds and, with the percentage of empty acorns found, indicated 17-22 per cent of the acorns never reached the ground. These authors reported also that the number of acorns taken from the trees by bluejays and red-headed woodpeckers was more than 10 per cent of the crop. Bluejay feeding may benefit quail by the fragmented acorn meats the jays leave. This interesting aspect to the bluejay-bob-white quail relationship has been discussed by Lay and Siegler (1937).

Wildlife usage was proportionately greater in poor seed years than in good years. In abundant seed years, wildlife usage was below 7 per cent for all three species in the University Forest stand. Wildlife utilization of acorns in Indian Trail amounted to 30 per cent in 1949, a poor seed year; but only 9 per cent in 1948, a good acorn year.

Wildlife utilization of acorns and its effect upon oak regeneration never has been a matter of agreement for those concerned. Korstian (1927) concluded that 90-100 per cent of the acorn supply may be consumed by animals and, in view of this "damage," emphasis should not be placed on their beneficial relation to woodlands. Olmstead (1937), Hoffman (1923), Seton (1927), and others have commented on the beneficial aspects of acorn-feeding by squirrels. Klugh (1927) pointed out that there is an immense over-production of acorns, as compared with the number which fall in situations where they can germinate; therefore there is no evidence to show that squirrels interfere with forest production in any ways by their seed-eating habits.

Many data showing the importance of acorns in the diet of Missouri game species have been gathered through analyses of stomach or crop contents. Deer, turkeys, quail, ducks, and crows are all known to rely heavily on acorns in Missouri. Other animals such as squirrels, bluejays, woodpeckers, etc., undoubtedly consume large quantities of acorns, but little information has been collected about them. Stomach or crop content analyses of several species of Missouri wildlife follow.

Deer: Analyses of 440 deer stomach samples from the Missouri Ozarks region during the period August 1948 to May 1952 showed the importance of acorns to deer (Table 8).

The data show that acorns, first available in quantity during September, are heavily utilized by deer during the fall and winter, and comprise significant portions of the spring diet. Percentages of the total volume of food comprised by acorns each month ranged from 0.0 per cent (inadequate sample) in June to 62.4 per cent in October.

Based primarily upon late fall and early winter samples, acorns comprised 80.1 per cent of the food consumed by the deer examined during 1948; 7.3 per cent in 1949; 32.6 per cent in 1950; 44.3 per cent in 1951; and 53.9 per cent in 1952 (Korschgen, 1954). These results confirm the findings of other mast studies in Missouri, which showed heaviest acorn yields in 1948 and 1952, and poorest production in 1949. It appears that the deer stomach analyses provided a reasonably accurate key to acorn abundance, since comsumption of acorns by deer was in relative proportion to acorn abundance.

TABLE 8. ACORN USAGE BY 440 DEER IN MISSOURI, 1948-1953

Month	Number of Stomachs	Percentage of Total Diet
January	8	32.4
February	23	32.0
March	23	16.4
April	17	16.0
May	6	14.3
June	4	0.0
July	8	7.5
August	10	0.1
September	11	31.4
October	16	62.4
November	131	43.8
December	183	45.7

The importance of the various species of oaks as sources of deer food is shown in Table. 9.

These data show that four species of oaks—black, white, post-, and red—contributed the bulk of acorn foods. These are also the more important oak species in the primary deer range of Missouri. Acorns, when available, are the principal fall and winter food of deer living in this habitat. No particular preference for certain kinds of acorns by deer was noted. Availability seemed to be the prime factor in acorn usage.

That acorns are an important source of food for deer is demonstrated by other investigators including: Dalke (1947) and Dunkeson (1951) in Missouri; Forbes *et al.* (1941) in Pennsylvania; Halloran (1943), Sanders (1941), and Taylor (1944) in Texas; Dexter, *et al.*

TABLE 9. SPECIES COMPOSITION OF ACORNS IN STOMACHS OF 440 DEER, 1948-1952

Species	Percentage of Total Food
Black Oak (*Quercus velutina*)	13.2
White Oak (*Quercus alba*)	8.5
Post-Oak (*Quercus stellata*)	7.4
Red Oak (*Quercus rubra*)	6.9
Scarlet Oak (*Quercus coccinea*)	1.8
Black Jack Oak (*Quercus marilandica*)	1.4
Unclassified (*Quercus* spp.)	1.3
Pin-Oak (*Quercus palustris*)	0.8
Shumard Oak (*Quercus Shumardii*)	0.3
Southern Red Oak (*Quercus falcata*	0.1
Mossy-cup Oak (*Quercus macrocarpa*)	0.1
Chinquapin Oak (*Quercus muhlenbergii*)	trace

(1952) in Ohio; Pearson and Burnett (1940) and Pearson (1943) in Alabama.

Wild Turkey: A study of the food habits of the wild turkey in Missouri showed that acorns provided a large share of the food for these birds, especially during the late fall and winter months (Original data—Dalke, Clark, and Korschgen, 1942). This investigation was based upon an analysis of 3,639 droppings of wild turkey. Specific identification of acorn species was impossible but acorn remnants indicated that several varieties of acorns had been ingested.

Analysis of the droppings showing the percentage of acorns in total volume is presented in Table 10.

By volume, acorns ranked first among plant foods during January, February, and March; second, during November and December; third, during April; and of lesser importance during the remaining months. It was evident that the turkey relied heavily upon oak mast for food

TABLE 10. PERCENTAGE OF WILD TURKEY FOODS COMPRISED BY ACORNS

Month	Number of Samples	Per Cent of Total Volume
January	136	73.3
February	480	56.1
March	433	46.8
April	412	10.0
May	148	2.2
June	143	1.3
July	89	trace
August	60	2.1
September	266	0.5
October	1,032	2.5
November	306	34.0
December	154	43.0

during those months when grasses and their seeds were not easily obtained.

Studies of wild turkey foods in other states support the extensive use of acorns found in this study of Missouri wild turkey. In Virginia, Mosby and Handley (1943) reported that acorns accounted for 27.8 per cent of the crop and gizzard contents of more than 500 wild turkeys. Acorns were used throughout the year, except for the summer months. May, Martin and Clarke (1939), and Culbertson (1948) also reported heavy utilization of acorns by wild turkeys in Virginia. This item accounted for 14.8 and 44.7 per cent of the turkey diets in the two studies, respectively. Acorns were the leading food of wild turkeys in Alabama, 63.2 per cent of the winter diet, according to Webb. (1941). Good and Webb (1940) found that 40.1 per cent of the spring diet was oak mast. In New Mexico, Ligon (1946) listed acorns as a principal food of Merriam's wild turkey.

Waterfowl: The use of acorns by waterfowl in Missouri has already been described by Korschgen (1955). A brief resume of his observations follows.

Ducks of 10 species were found to have fed upon acorns during their fall migration through Missouri. Except for mallard, ring-necked and wood ducks, however, acorn consumption was not great. Pin-oaks provided the bulk of mast utilized by ducks, primarily because of their location in the wet-land habitats. Wood ducks also fed upon red and white oak acorns, the only other species identified in duck gizzards. Oak mast comprised 42.4 per cent of the fall diet of wood ducks, 9.7 per cent of the diet of mallards, and 9.6 per cent of the food consumed by ring-necked ducks. Other kinds of ducks consumed acorns in lesser amounts, as shown in Table 11.

Acorns, principally pin-oak, were found in 9.1 per cent of the samples, and accounted for 9.2 per cent of the total food consumed by 2,252 ducks. No acorns were found to have been used by gadwall,

TABLE 11. ACORN CONSUMPTION BY DUCKS IN MISSOURI, AS SHOWN BY GIZZARD ANALYSES, 1946-1953

Species	Number of Samples	Per Cent of Total Volume
Baldpate	30	trace
Black Duck	9	2.7
Green-winged Teal	318	0.3
Lesser Scaup	44	trace
Mallard	1,467	9.7
American Merganser	6	0.6
Pintail	148	0.9
Ring-neck	59	9.6
Shoveller	30	0.9
Wood Duck	38	42.4

redhead, blue-winged teal, canvasback, golden-eye, or ruddy ducks in Missouri. Absence of acorns in those species probably was influenced by early migration in the case of blue-winged teal, and by the small number of samples examined from other species.

Wood ducks were found likewise to be the principal users of acorns among seven species of ducks studied by Mabbott (1920). Stollberg (1950) found acorns heavily used by wood ducks, and to a lesser extent by other shoal-water ducks in Wisconsin. Greatest utilization of oak mast by waterfowl occurred in the central and southern states, as shown by Martin and Uhler (1939. Use of acorns by mallard, pintail, and wood ducks was reported by Martin, Zim, and Nelson (1951).

Quail: Food habits of the bobwhite quail in Missouri have been reported previously by Korschgen (1948; 1952). Missouri quail are found in many habitats which range from open prairies to dense woodlands. Analyses of 5,472 quail crops from all parts of the state during November and December showed that acorns served as the fifth most important food. Oak mast, whole or fragmented, was found in one-eighth of the crops examined, and accounted for 7.1 per cent of the total food volume.

Further study of quail foods by analyses of 1,358 droppings samples, collected state-wide from September through April, showed that acorns were utilized by quail each month of the study period. This food was found in 13.0 per cent of the droppings examined, but made up only 1.2 per cent of the eight-months food intake, by volume. The lesser importance of acorns in the quail diet, as shown by droppings, was attributed to the highly digestible nature of acorns which caused much of this food to be undetectable by the droppings analysis method.

Crops analyses revealed that quail often swallowed the small post-oak acorns whole, while larger acorns of other oaks usually were taken as fragments. Part of this feeding may have been as gleanings from the feeding of other birds and mammals.

The usage of acorns by quail has been noted by many who have studied the food habits of this game bird. Martin (1935) and Davison (1942) found that oak mast rated high as preferred quail food in the southeastern states. Gray (1940), Lett and Pearson (1942), Pearson and Howell (1943), Allen and Pearson (1945) all reported acorns as a principal food of quail in Alabama. Johnson (1940) found acorns to be important to quail in Mississippi, while Wilson and Vaughn (1944) showed the frequent utilization of acorns by quail in Maryland. Many additional references might be listed to show the importance of oak mast in the bobwhite diet.

Raccoon: The raccoon is an omnivorous feeder which was found to partake of acorns frequently as a staple food. Examination of 27 stomachs and 64 scats from this animal showed that acorns were eaten for 17.6 per cent of the meals. The percentage of total volume of food (4.1 per cent) probably is not truly representative because of the highly digestible nature of acorns. The data do serve to show that acorns are an important source of food for this popular furbearing animal, as also was reported by Baker, Newman. and Wilke (1945), Yeager and Elder (1945), and Llewellyn and Uhler (1952).

Crow: The crow, another omnivorous feeder, was found to take a diet of approximately two-thirds plant and one-third animal foods. Analyses of 156 crow stomachs and 35 pellets in Missouri showed that acorns occurred in 23.0 per cent of the samples; ranked seventh among 66 plant foods eaten; and made up 3.5 per cent of all food, by volume. Crow-feeding upon acorns was characterized by fragmentation of the fruits, rather than ingestion of whole acorns.

Other: Portions of the food of other animals were found to have consisted of acorns. Oaks of many species are known from field observation to be a principal source of food for squirrels, but food habits information from stomach analyses is very limited for squirrels in Missouri.

Examination of the stomachs of 352 gray foxes showed that these animals fed upon acorns occasionally. Acorns amounted to 0.6 per cent of the total foods.

Two occurrences of acorn in skunk scats indicate that these animals occasionally feed upon acorns, but this item is unimportant in the skunk diet. The presence of acorns in samples from great-horned owl, barred owl, channel catfish, and bullfrog may have been incidental or accidental.

EFFECTS OF ACORN YIELDS AND FEEDING UPON WILDLIFE

The abundance and quality of food which an animal has available influences its movements and activities, its general condition of health, and its ability to reproduce. Van Dersal (1940) comments: "The geographic distribution of a number of animals has been stated to coincide with or be dependent upon the range of oaks." Some animals mentioned included raccoon (Bailey, 1926), band-tailed pigeon (Bendire, 1892), California woodpecker (Ritter, 1939), and others. Seton (1928) noted for the gray squirrel that "habitat coincides with that of the oaks and hickories, especially the black oak, white oak, and bitternut hickory." Possibly no one kind of natural food influences as many species of wildlife as do acorns. Recent findings have given us some inkling of the importance of acorns to wildlife and the far-reaching effects of this food upon the general status of mast-consuming wildlife species.

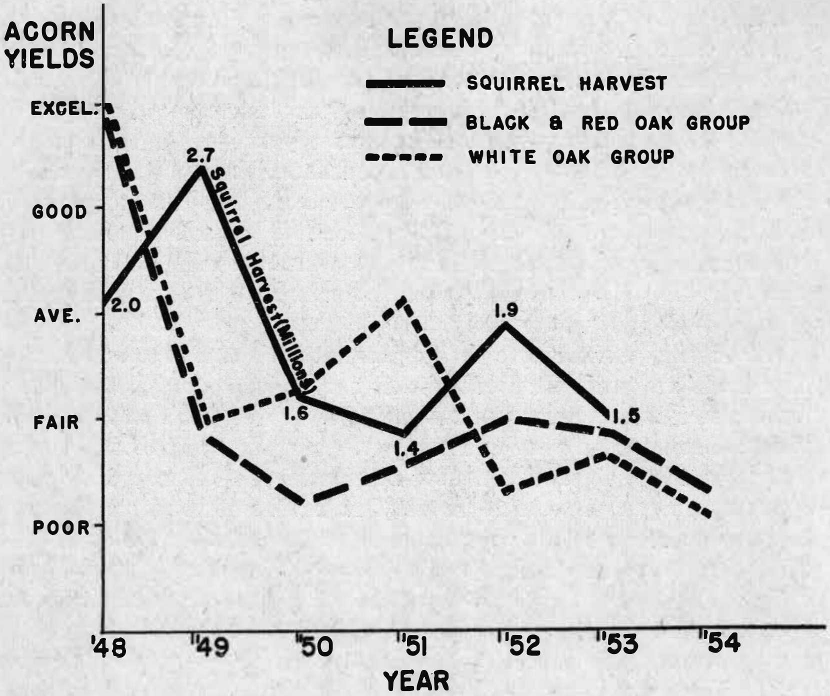
The primary factor responsible for the size of our annual squirrel crops in Missouri seems to be the abundance of acorns in the fall immediately preceding squirrel production. The relationship of squirrel abundance to acorn abundance is shown in the graph (Figure 1). The annual squirrel harvest data plotted against our annual mast survey data, obtained from our forest technicians, indicates a direct relationship of squirrels to acorn abundance.

The 1949 harvest of squirrels following the excellent acorn year of 1948 was nearly 2-¾ million squirrels. Acorn production was fair in 1949, and in 1950 the squirrel harvest was just over 1-½ million animals. Although some improvement in the white oak yields occurred in 1950, it was not sufficient to offset a further decline in yields by the black and red oaks. In 1951 the squirrel harvest declined to less than 1-½ million animals, but acorn yields showed marked improvement. Hunters bagged nearly 2 million squirrels in 1952. The white oaks' yield fell from average to poor; black and red oaks indicated a slight increase to fair yields in 1952. The following year, the total squirrel harvest declined to 1-½ million, and acorn yields showed little change, being below fair. The 1954 squirrel harvest figures are not available at this time, but it is anticipated that a slight increase occurred. Acorn yields were generally poor in 1954 and, if past evidences are indicative of trends, the squirrel harvest in 1955 is expected to be less than in 1954.

The abundance of other mast foods may modify the effects of acorn yield deficiencies in the sum total of food available to squirrels, but by sheer numerical superiority the acorns are more important than other nuts and are more widely distributed.

Figure I.

SQUIRREL HARVEST & ACORN YIELDS, 1948 - 1954



Studies in Illinois (Brown and Yeager, 1945), Indiana (J. Allen, 1952), Michigan (D. Allen, 1943), and Missouri (Crawford, 1950) have indicated that squirrel litter sizes vary with the abundance and quality of food available. Evidence at hand seems to indicate that this factor, coupled with numbers of litters per year, is a function of the food supply and governs the squirrel crop. Hunting success in terms of squirrels per gun-hour has fluctuated more widely in the south Missouri forest lands, where dependence upon acorns is more critical, than in the prairie type of western and northern Missouri. The degree of hunting success depends greatly on the number of young animals available to the hunter. Thus, hunting success is linked with acorn yields, as they influence squirrel production. Generally, hunters have better success and take a higher proportion of juveniles in the years following good seed crops; and, conversely, poorer success and a lower percentage of young subsequent to poor seed years (Burns, *et al.*, 1954). Reproductive success, so essential in sustaining the squirrel population, stems from the supply of acorns dropped the preceding fall. Truly the acorn is a barometer of squirrel reproductive success, hunting success, and total squirrel harvest in our state of Missouri.

The general importance of acorns in the diet of deer, particularly in south Missouri, was shown earlier. A specific point was of interest: 70 male deer of the 1-½ year age class taken in the Ozarks during the 1948 open season—an excellent mast year—averaged 4 pounds heavier, by dressed weight, than 71 bucks of the same age class for the same hunting period in 1949—a poor acorn year (Brohn and Robb, 1955). In view of the fact that December seasons allowed little opportunity for the effects of fall and winter feeding to become apparent in the weights of deer taken, at least this instance is suggestive of a food—weight relationship.

Availability of acorns to deer determines the extent to which other deer foods are used. Depredation on agricultural crops, pine plantations, and Eastern red cedar was more severe in years of light acorn yields. Browsing on pine was first noticed in the Indian Trail Refuge Forest in 1945, following a near-failure in the acorn crop. Heavy browsing on pine and cedar in the Ozarks did not occur again until 1949, when acorn yields were again light (Dunkeson, 1951). At that time, coralberry and sumac fruits, favored deer foods, were heavily browsed, and only 24 per cent of the deer stomachs examined contained acorns, which represented 8 per cent of the total volume. In the preceding year (1948), when oaks yielded heavily, these favored browse foods were used but lightly. An examination of deer stomachs revealed 97 per cent contained acorns, which comprised 83 per cent of the total volume of food.

Availability of food tends to influence the range and mobility of wildlife species. Dalke, *et al.* (1942) noted that wild turkey tended to drift into corn, cane, winter barley, and wheat fields in the Ozarks when acorn crops were short, but the birds remained in the oak timber when good yields occurred.

DISCUSSION OF MANAGEMENT IMPLICATIONS

In summarization, some of the findings relative to acorn yields and wildlife usage are reviewed with reference to management possibilities.

The occasional year of abundant seed production is of great importance in providing oak regeneration; in other years, few viable acorns are available because of insect damage and wildlife usage. Wildlife consumption is probably a limiting factor in regeneration of oaks in poor seed years; in good seed years, wildlife does not consume all the sound acorns, and squirrels may even aid regeneration by leaving buried acorns to germinate. Scarlet, black, and white oaks, because of their prolific yields of viable acorns, have an advantage over black jack and post oaks in regeneration. Post oak is especially weak in this respect. Post and white oak acorns germinate in the fall, and thus are less subject to animal usage than are the acorns of the black oak group, which germinate in the spring.

A good food species from the viewpoint of wildlife management is one which produces abundantly every year. None of the oak species studied met this qualification. Post oak was a consistent but low producer; the other species, although abundant producers in some years, were not consistent. Scarlet oak appeared to be especially erratic as a seed-producer.

Forest managers, desiring both oak regeneration and mast-consuming wildlife species, can best achieve this objective by leaving the good seed-producers when cuttings are made in oak forests. In the absence of production records or observations on seed yield by individuals trees, oaks with large crowns should be left to produce seed.

Wildlife managers should attempt to maintain a variety of oak species, so that complete failures will be unlikely. It is advisable to include oaks from the black group in the variety. Acorns from these oaks do not germinate until spring, and therefore are available to wildlife throughout the winter when other foods are scarce. A variety of oaks tends to stabilize the supply of food and likewise the populations of wildlife which are dependent upon it.

In conclusion, an effective method of evaluating the size of acorn crops annually in areas heavily forested with oaks furnishes the land manager with an effective tool with which to gauge wildlife activity and respond to the needs of the situation accordingly. The extent of deer browse on native plants and depredation on agricultural crops and pine plantations, the general outlook on squirrel production and hunting prospects, the advisability of food-patches and feeding stations to hold wildlife in particular areas, the success of baiting mast-consuming wildlife species into traps, the degree of use expected in flooded oak timber by ducks as opposed to the use of open marsh-lands . . . these are some of the useful facets of information available if acorn yields were known in advance.

There is little doubt that the acorn is an important resource in Missouri, considering that the total yield of seed from a very ordinary stand of oaks can average 20 pounds of mature, insect-free acorns per acre. If only 10,000,000 acres of Missouri's 15,000,000 acres of forest are productive, the yield is 200,000,000 pounds of insect-free acorns.

If these acorns possessed the economic value of corn (as they do in food value), this average yield in one year would bring 5 million dollars on today's market. Certainly the acorn is a natural resource vital to the creation of even greater resources—forests and wildlife.

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DISCUSSION

DISCUSSION LEADER HOLDER: Mr. Christisen, you gave the figures for the percentage of acorns in wildlife usage. Did that include acorns utilized by crows, jaybirds, and so on, or just game?

MR. CHRISTISEN: The figures which I gave on the terrestrial utilization and the figures which included the arboreal utilization did include the usage by woodpeckers and jays primarily, although some other birds do use acorns in trees.

The general figure which was given, which you may have had reference to, 7 to 22 per cent which were utilized by wildlife—did include usage by birds. It did not include the acorns which we had no record of, which were carried away.

MR. HANDLEY [Mississippi]: There is one question that I'd like to ask the gentleman. By the way, I think it was a very good paper and excellently presented.

I believe he mentioned that an average acre stand of hardwood would produce 20 pounds of insect-free food per year, and I wonder if anybody has any idea what he thinks would make up that average stand. I'm thinking particularly where it is a mixed hardwood-pine stand. Is there any idea as to how much hardwood should be left on the land?

MR. CHRISTISEN: The calculation was made on the basis of the Indian Trail Forest stand, which was, as I said, a very ordinary stand, second-growth hardwood, and it consisted of 20 trees per acre made up of five species of black, scarlet, and blackjack, in the black oak group, and white and post in the white oak group. Those five species comprised the bulk of the species found in the type of Missouri Upland which I happened to be sampling in the Indian Trail area.

I suspect 20 pounds is a very conservative figure, though, on the yield per acre, because it was a very poor stand of oak, it might be double the amount that I have given if we had a better stand.

Of course, the problem of calculating and computing what a stand will produce is very much a speculation, unless you do have figures. But if you have the species composition of an area and you have adequate sample of that area, you could calculate that.

Now, I haven't been of much help, maybe, in answering your question, but there are so many variables involved that it is rather difficult. Downs and McQuilken, I believe, did quote a higher figure on production in the Appalachian Mountains. Their figure, I believe, was something like 150 pounds per acre. However, I suspect that they had a better stand, and they may also have included some acorns which were not sound in our calculations. We cracked all of the acorns which we collected, so that we knew what per cent were sound and what were not.

DISCUSSION LEADER HOLDER: I want personally to thank both Mr. Christisen and Mr. Korschgen for this good review on acorn production. I believe it would be of considerable practical value in my own state, where eventually we will want to go into the harvest of timber on the game reservation areas which we have acquired. We will want to harvest that timber, of course, in such a way that the harvest will bring about the maximum wildlife benefit, and I feel quite definitely that their paper will assist us in that particular problem.

ANALYSIS OF PHEASANT AGE RATIOS[1]

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Ratios of juveniles per adult male obtained from hunter bag checks are often used, in conjunction with spring sex ratios, as a measure of pheasant productivity. A necessary assumption for use of such data is that the average age ratio in the bag represents the ratio in the population: However, Allen (1941), Stokes (1954), and others (see Stokes, 1954, page 77) have noted a decrease in age ratio as the hunting season progresses. A greater vulnerability to hunting on the part of juveniles would seem to provide a reasonable explanation of such a change in age ratio.

Data obtained in Michigan during the years 1950-1953 have shown a decline in age ratio as the hunting season progresses and have yielded average age ratios which do not appear compatible with spring sex ratios (only male pheasants are legally hunted in Michigan). Average age ratios in the bag have varied from 13 to as high as 18.5 juveniles per adult, while spring sex ratios have ranged from about three to four hens per cock in the four year period. This would require unreasonably high productivity per hen if the observed ratios correctly represent the population values

The purpose of this paper is to present some Michigan age ratio data in terms of an algebraic representation of a system which seems

[1]A contribution from Pittman-Robertson Project W-38-R, Game Division, Michigan Department of Conservation.

to fit the observed data. No effort will be made here to estimate actual age ratios in the population.

Age ratio records used here were obtained by mail collection of pheasant legs from sportsmen throughout Michigan's pheasant range (about 26,000 square miles in the southern third of the state). Age ratios for each day of the season were derived by classifying the collected legs as adult or juvenile on the basis of spur characteristics. The criteria used were shape, color, and texture, as described by Linduska (1943). Spur length was used only in preliminary separation. In order to minimize classification error all final aging was done by the same person during each of the four years. Spur length was not used as a final criterion since Petrides (1949) has demonstrated that the degree of error associated with aging by spur length alone is sufficient to distort derived age ratios seriously when they are as high as those expected in Michigan. Linduska (1943) indicated that gross appearance criteria were likely to be more accurate than length. It was found in the present study that age separation on this basis was quite distinct in most cases. Each year we have been able to classify all but four to ten of the three to four thousand specimens examined.

Records obtained from the four-year collection are given in Table 1 along with distribution of hunting effort for the given years (for explanation see below).

A mathematical model of a system which might produce records like those observed in Michigan and elsewhere may be written as follows:

Let

$X =$ Pre-season juvenile cock population.
$Y =$ Pre-season adult cock population.
$r_1 =$ Juvenile mortality rate from shooting.
$r_2 =$ Adult mortality rate from shooting.
(r_1 and r_2 are equivalent to the respective probabilities that a given juvenile or adult cock will be taken by one unit of effort).
$n =$ Total units of hunting effort (gun hours or some multiple of gun hours) up to and including some given time.
$k_1 =$ Kill of juveniles for a given unit of effort.
$k_2 =$ Kill of adults for a given unit of effort.

Then $k_1 = r_1 X (1\text{-}r_1)^{n-1}$

This equation may be interpreted as follows: the value $X (1\text{-}r_1)$ is equal to the number of juveniles surviving the first unit of effort; this number taken times $(1\text{-}r_1)$ equals the number surviving the second unit of effort; and so on, resulting in the number alive at the begin-

TABLE 1. MICHIGAN PHEASANT AGE RATIO RECORDS, 1950-1953

Day of Season	1950			1951			1952			1953			Cumulative Effort From Mail Survey (Gun Hours)
	Daily Age Ratio	Cumulative Effort (%)	No. of Specimens (W)	Daily Age Ratio	Cumulative Effort (%)	No. of Specimens (W)	Daily Age Ratio	Cumulative Effort (%)	No. of Specimens (W)	Daily Age Ratio	Cumulative Effort (%)	No. of Specimens (W)	
1	18.65	15.62	1336	17.56	21.74	1800	22.21	21.34	1323	24.82	26.36	1136	3823
2	14.54	29.93	746	19.09	30.27	442	31.60	31.09	326	18.35	37.79	329	5451
3	12.70	35.72	233	15.81	35.02	269	38.40	36.12	197	16.73	41.89	195	6311
4	14.75	38.45	189	16.54	39.90	193	36.00	41.23	185	16.75	45.36	142	7023
5	8.71	41.28	136	9.50	44.23	147	45.67	46.22	140	32.71	56.12	236	9443
6	13.33	45.84	129	7.59	56.20	421	12.75	55.00	275	15.08	62.88	193	10988
7	11.89	53.20	245	12.50	60.45	216	18.12	58.97	153	20.67	65.14	65	11420
8	12.65	57.75	232	12.40	62.49	67	8.00	61.62	36	27.00	67.48	28	11748
9	6.00	59.37	42	7.25	64.21	33	36.00	62.63	37	14.00	69.50	45	12048
10	8.17	61.95	55	4.00	65.62	30	54.00	65.61	55	26.00	70.91	54	12336
11	14.00	64.19	45	10.50	67.18	46	6.40	67.18	37	25.00	73.30	52	12653
12	6.60	66.81	38	9.50	69.35	42	11.33	68.70	37	6.00	79.94	98	13944
13	35.00	68.46	36	5.92	75.19	83	10.70	73.86	117	9.43	83.16	73	14847
14	20.40	73.59	107	10.75	77.94	47	7.50	75.84	68	4.67	84.24	17	14983
15	5.54	77.14	72	26.00	78.86	27	9.00	77.12	20	3.25	85.22	17	15131
16	31.00	78.38	32	11.00	80.44	24	4.75	78.37	23	5.75	86.38	27	15291
17	25.00	79.42	26	23.00	80.95	24	7.00	79.83	24	4.00	87.50	15	15432
18	10.50	81.05	23	13.50	81.91	29	6.00	80.71	14	3.50	88.66	9	15559
19	5.25	82.36	25	15.00	82.78	16	7.00	82.25	16	10.20	94.43	56	16267
20	....	82.92	....	7.28	87.00	78	12.50	88.56	54	4.75	97.01	23	16745
21	9.00	85.97	40	4.86	87.85	41	12.00	90.50	39	....	97.88	51	16925
22	9.67	87.65	64	9.67	88.99	32	12.00	92.75	26	16.00	100.00	....	17221

ning of the nth unit of effort (regarding units of effort as being consecutive) equaling $X\ (1\text{-}r)^{n-1}$. If this quantity is multiplied by r_1 we have the kill of juveniles for a particular unit of effort which is the result given above. This is an equation of the type treated by DeLury (1947, 1951) and is given on page 165 of his 1947 paper. DeLury gives a rather thorough development of theory applicable here, including the assumptions basic to use of the system described above.

From the above, an equation representing the age ratio in the kill resulting from a given unit of effort is:

$$\frac{k_1}{k_2} = \frac{r_1}{r_2}\,\frac{X}{Y}\left(\frac{1-r_1}{1-r_2}\right)^{n-1} \tag{1}$$

Since this is an exponential relationship it is equivalent to a logarithmic equation:

$$\log\frac{k_1}{k_2} = \log\left(\frac{r_1}{r_2}\,\frac{X}{Y}\right) + (n-1)\log\left(\frac{1-r_1}{1-r_2}\right) \tag{2}$$

which is, in turn, a linear expression with "Y intercept" equal to log $\left(\frac{r_1}{r_2}\,\frac{X}{Y}\right)$ and slope equal to log $\left(\frac{1-r_1}{1-r_2}\right)$.

This may be approximately fitted from sample data as a linear regression of logarithms of daily age ratios on cumulative effort. Graphic representation of the data from Table 1 is shown in Figures 1 and 2. It should be noted that weighted regressions were used with the weights ("W" column in Table 1) being the number of specimens on which the particular day's age ratio is based. Two classes of weights are shown in the figures by encircling dots representing the more heavily weighted values.

The daily age ratio is taken as an "instantaneous" value (*i.e.*, representing the age ratio of one unit of effort) and is plotted at the beginning of the day, whereas, as an average, it should properly be plotted at some intermediate point in the day's effort.

Distribution of hunting effort for the four years is available only from a survey conducted by a newspaper, the Detroit *Free Press*. These records have been adjusted to totals for each year on the basis of license sales, taking 1953 as a base. Since response to this survey is purely voluntary and necessarily restricted to readers of the *Free Press*, biases of considerable magnitude may well exist in these data.

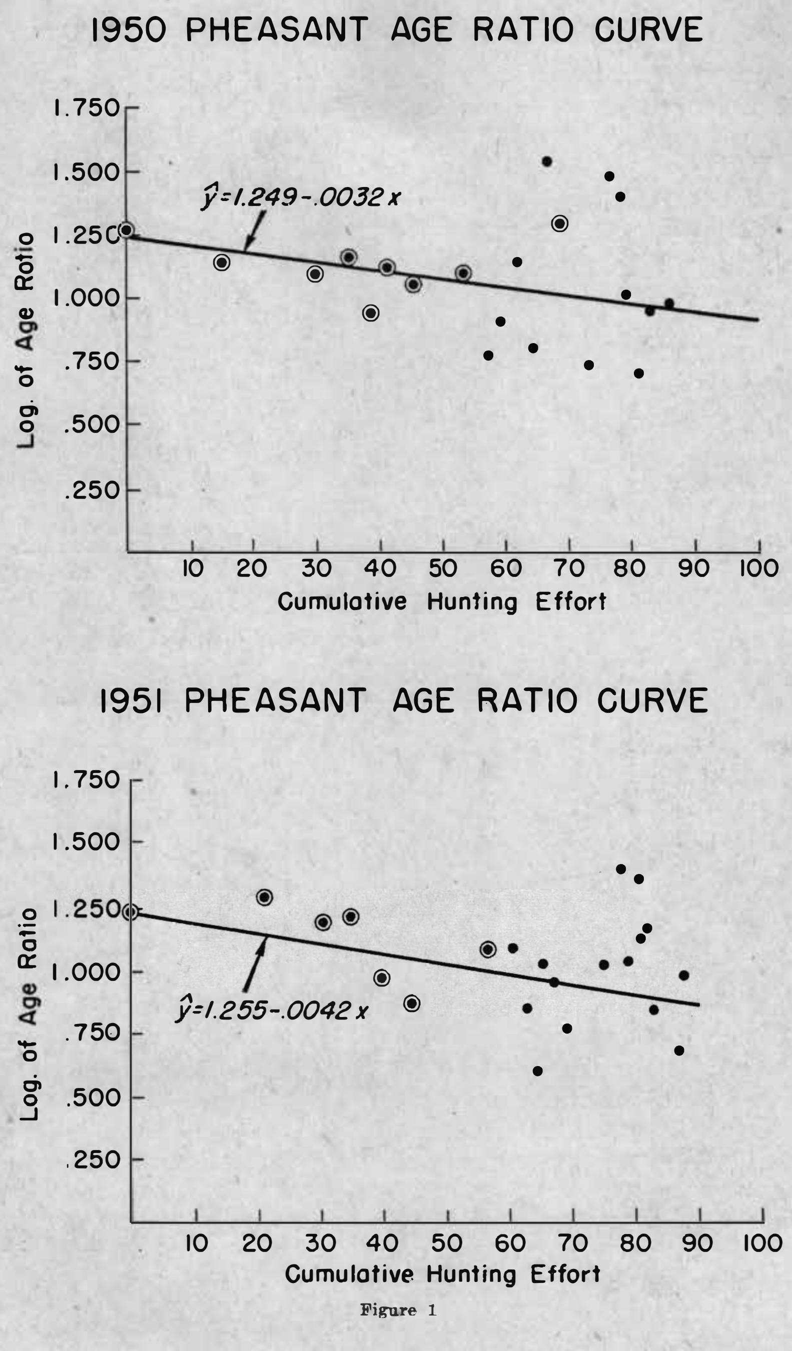
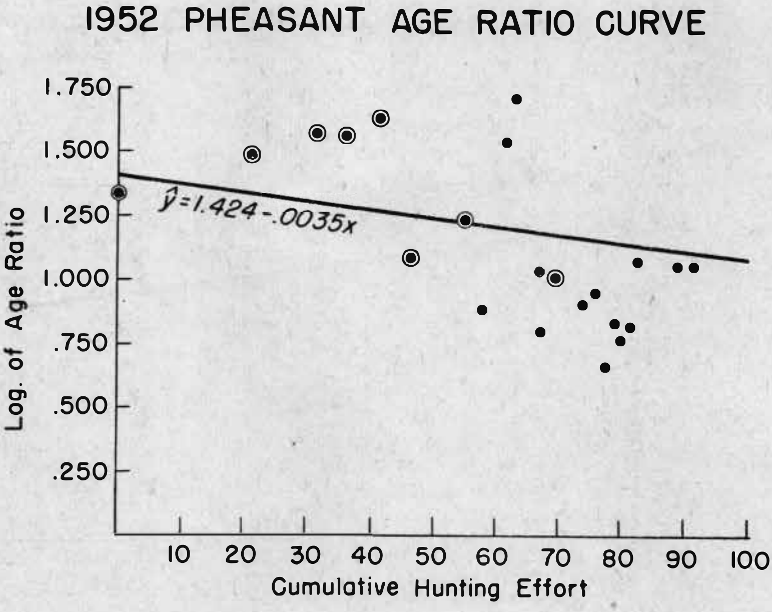

1950 PHEASANT AGE RATIO CURVE
1.750
1.500
1.250
1.000
.750
.500
.250
Log. of Age Ratio
ŷ=1.249-.0032x
10 20 30 40 50 60 70 80 90 100
Cumulative Hunting Effort
1951 PHEASANT AGE RATIO CURVE
1.750
1.500
1.250
1.000
.750
.500
.250
Log. of Age Ratio
ŷ=1.255-.0042x
10 20 30 40 50 60 70 80 90 100
Cumulative Hunting Effort

Figure 1


1952 PHEASANT AGE RATIO CURVE
1.750
1.500
1.250
1.000
.750
.500
.250
Log. of Age Ratio
ŷ=1.424-.0035x
10 20 30 40 50 60 70 80 90 100
Cumulative Hunting Effort

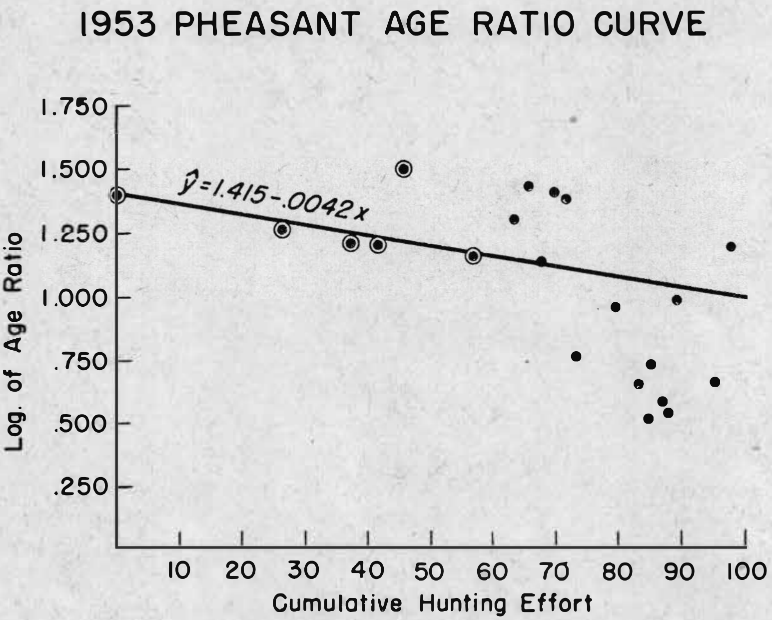

1953 PHEASANT AGE RATIO CURVE
1.750
1.500
1.250
1.000
.750
.500
.250
Log. of Age Ratio
ŷ=1.415-.0042x
10 20 30 40 50 60 70 80 90 100
Cumulative Hunting Effort

Figure 2

In 1953 a postal survey of hunters was conducted (Blouch, 1954). providing effort data which are probably more reliable. These data are also given in Table 1. The regression equation for these records is $y = 1.413 - .0000257\ x$ and the sampling rate was about 1.411 (Blouch, 1954).

The postal survey data for 1953 might be used in an effort to compute the age ratio in the population in several ways:

(1) By computing adult and juvenile populations separately. This could be accomplished by splitting daily kill estimates (for postal survey) into adults and juveniles and using the methods given by DeLury (1947, 1951).

(2) By estimating mortality rates for adults or juveniles and substituting in equation 1. This might be worth while in the case where restricted numbers of adult records are available so that estimation of adult populations is uncertain. Otherwise this depends on the same computations as point (1).

(3) By estimating the ratio of the two mortality rates $\frac{r_1}{r_2}$ from a number of sets of data and substituting in equation 2. Similarly, the individual rates might be more precisely estimated from several sets of data but Dr. Don Hayne (personal communication) has pointed out that these estimates would likely need to be from the same size area since the mortality rates (r_1, r_2) apparently may vary with size of area. However, the ratio of the rates may not be subject to this restriction.

Such computations will not be presented in this paper because we have not adequately investigated the available records. Also, a technical difficulty exists in that both variates (effort and kill) are estimated by sampling methods, and this may introduce a bias in the computation of the regression coefficient (Berkson, 1950).

An additional relationship may be useful in discussing analysis of age ratios. This is the equation for average age ratio for a given amount of effort; for example, for the first day of the season.

The desired equation may be obtained by taking the mean value of

$$k_1 = r_1 \times (1\text{-}r_1)^{n\text{-}1}$$

Over the interval (o, n) by Calculus methods. This results in:

$$\frac{-}{k} = \frac{r_1 \times (1-r_1)^n}{n(1-r_1)\ln(1-r_1)} \quad (\ln = \text{logarithm to base e})$$

And since r_1 is some very small number the equation may be reduced to an approximation:

$$\frac{\text{—}}{k_1} = \frac{X\,[1-(1-r_1)^n]}{n}$$

(This is an approximation for the continuous model used here but is also the exact expression for the fiinite case.)

Combining values for $\frac{\text{—}}{k_1}$ and $\frac{\text{—}}{k_2}$ we have:

$$\frac{n\bar{k}_1}{n\bar{k}_2} = \frac{\bar{k}_1}{\bar{k}_2} = \frac{X\,[1-(1-r_1)^n]}{Y\,[1-(1-r_2)^n]} \tag{3}$$

Which gives the average age ratio for the interval (o, n). This relationship is used in discussing comparison of first day age ratios below.

Several further points concerning age ratio curves follow:

It is possible that as the season progresses juveniles may become more wary and therefore less vulnerable. If this were so then the slope of the age ratio curve (logarithmic form) would decrease as the season progressed. The available distributions do not seem to show this change. However, it seems possible that adult and juvenile rates both might change in such a manner as to maintain a constant slope.

Unrecovered crippling losses have not been considered in computing these curves. The effect of such losses will depend on the magnitude and manner in which such losses occur. Further consideration of this effect seems essential in attempting to determine true age ratios.

The sampling distribution of much of the available data is probably not reliable since both age ratio and effort data depended on voluntary returns by hunters. This means that the average age ratio as ordinarily calculated from such data may not be the average ratio in the bag. In Michigan, hunters apparently send in legs from proportionately more of their early season bag, thus tending to increase the apparent average ratio. It seems likely that this difficulty can be at least partially corrected through use of a mail survey to determine yearly distribution of kill and effort.

If the relationship postulated in this paper holds in practice then the average age ratio in the bag will always be higher than that in the population unless all of the population is bagged. As the proportion of the population bagged increases, the magnitude of the biasing factor should decrease, providing reliable sampling methods are used.

In the absence of data for estimating "true" or population age ratios two methods of comparing age ratios from area to area or between years are available. The first depends on computing regression lines like those shown in figures 1 and 2. If the necessary data are available, these lines may help in judging whether the suggested relationship holds in a given case. At present we do not have sufficient results in theory and in practice to determine whether or not these curves fairly represent the actual situation. If they do, then the ratio of "Y-intercepts" (point where the regression line reaches the vertical axis) might be used to represent changes between years or areas.

An alternative procedure is to compare age ratios for the first day of the season. If the amount of first day effort is the same for the areas or years being compared then the ratio of two first day values may be expected to be an unbiased estimate of the ratio of the two true age ratio values. If hunting effort is not the same, then the age ratio obtained for the area of greater effort will be more nearly its true ratio. If this is the area with lower true age ratio, then the apparent difference between areas will be an overestimate of the true difference. On the other hand, if the area of higher effort has the higher true age ratio then the comparison will be an underestimate.

Age ratios for first day of the season, season averages, and "Y-intercept" values for the 1950-1953 Michigan records are given in Table 2.

It seems likely that age ratios in other species may vary with hunting effort in much the same manner as do those for pheasants. Examination of ruffed grouse age records obtained in a Game Division survey (G. A. Ammann, unpublished) has shown a similar decline in age ratio with advance of the hunting season. Hunter selectivity may result in the same sort of behavior. This has been noted in various age and sex ratios obtained during an "any-deer" season in Michigan (Eberhardt, unpublished).

The idea that differential vulnerability rates might be influencing our pheasant age ratios was suggested to us by Dr. Don W. Hayne of Michigan State College. Dr. Hayne has supplied much helpful advice on development of the material presented here. Results of the ***Free***

TABLE 2. MICHIGAN PHEASANT AGE RATIOS, 1950-1953

Year	Season Average (unadjusted)	First Day	"Y-Intercept"
1950	13.9	18.6	17.8
1951	13.2	17.6	18.0
1952	18.5	22.2	26.5
1953	16.8	24.8	26.3

Press hunting survey were supplied by Mr. Jack Van Coevering, outdoor editor for the *Free Press.*

SUMMARY

Study of pheasant age ratios obtained in Michigan over the four year period 1950-1953 indicates that the average values obtained from hunting data are unreasonably high. Such a situation might result if juveniles are more readily taken by hunting than adults. If this is the case, it can be shown that age ratios computed from shot samples will be overestimates, with the degree of bias decreasing as the proportion of the population taken increases.

Accurate determination of true age ratios seems difficult in view of highly variable data and uncertain sampling. Until practicable and theoretically sound methods for estimating actual age ratios in the population are found, comparisons between years and areas might be made by using age ratios from the first day of the season if first-day hunting effort does not vary greatly between years or areas being compared. A supplementary or alternative procedure for comparisons is available through computation of regression equations as described above.

The problems involved in evaluation of age ratios seem to be worthy of much further study since a similar situation probably exists for other species and possibly for sex ratios.

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DISCUSSION

DR. DAVID E. DAVIS [Baltimore, Maryland]: Could you give us the significant difference of the slopes of those lines from zero?

MR. EBERHARDT: I'm sorry, I can't. The reason that I haven't computed them is, the effort scale is based on involuntary response by readers of newspapers, and I'm reasonably sure that it is not representative of what goes on in the hunting population as a whole. In one year, in 1953, we had a mail survey in which we did get probably an unbiased picture of the distribution of the hunting effort. In other years we don't have that, and I would hesitate to attach any exact significance level to those figures.

MR. DAHLEN [Madison, Wisconsin]: You mentioned the general appearance of the birds. I wonder if you would give us a brief run-down on what you used as criteria for appearance, whether you used the length of the spur. Would you give us just a brief run-down on what you used as criteria in the appearance of the birds to determine age?

MR. EBERHARDT: The hardness of the spur covering its color and general shape, as I understand it, are the criteria; but I personally can't describe any borderline cases.

AGE, BREEDING BEHAVIOR AND MIGRATION OF SOOTY GROUSE, Dendragapus obscurus fuliginosus (Ridgway)

JAMES F. BENDELL

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INTRODUCTION

The purpose of this paper is to present new information on the biology of the sooty grouse. The distribution of the genus *Dendragapus* coincides fairly well with that of Douglas fir (*Pseudotsuga menziesii* (Mirb.) Franco. (Beer, 1943). The altitudinal migration of members of this genus has attracted the attention of naturalists since early times. Anthony (1903) observed the downward migration in eastern Oregon on about the first of March. In this particular region the winter range was at 8,000 feet and the summer breeding range at 4,100 feet and lower. Most of the movement was on foot although in crossing ridges the birds would glide from one ridge to the next in flocks of 12 to 100 birds. The greatest flights occurred at sunrise and sunset. Wing (1947), like Anthony, describes the fall migration as a more gradual movement to the highlands which begins in mid summer and lasts until October. Wing *et al.* (1944) noted that the males migrate to the winter range in the high altitudes earlier than the females and broods.

Van Rossem (1925) provided an age criterion for members of the genus *Dendragapus*. He found that grouse between their first and second fall molts (yearlings) have shorter and narrower rectrices than grouse of a second and later fall molt (adults). This method of age determination has been checked and placed on a quantitative basis by Petrides (1942) and Bendell (in press). Wing (1943) suggests that some yearling males remain on the winter range over summer and may not breed during the first year of life. Buss and Schottelius (1954) present similar views.

TIME AND PLACE OF STUDY

Data were obtained over the years 1950 to 1953 on part of a breeding range near Quinsam Lake, Vancouver Island. The area is 600 feet above sea level and between the Straits of Georgia to the east and the mountain ranges of the island approximately two miles to the southwest. The winter home of the population using the breeding range at Quinsam Lake is presumably in these highlands.

The Quinsam area was once under climax forest, but repeated logging and burning have reduced the original forest to charred logs and stumps and bare soil. At present, willow (*Salix* spp.) and bracken [*Pteridium aquilinum* (L.) Kuhn] are the dominants although many other herbs and shrubs grow in abundance. Some of the area has been replanted to Douglas fir. The vegetation is loosely associated except in moist depressions and along the banks of a river where dense growth occurs. In this present stage of forest succession the grouse are almost completely ground dwelling and can be studied with relative ease.

An area of approximately four square miles was cruised in banding grouse while detailed work on behavior was done on two study plots each 36 acres in size. Grouse were captured with a sliding noose of plastic line on a 10- to 11-foot bamboo cane or by driving them into nets. Various marking techniques were used; such as tail cards of colored plastic, colored bands and bands with bells attached. In some cases grouse which avoided capture were marked by smashing a Christmas tree ball filled with dye on them or nearby vegetation.

THE SPRING MIGRATION

Field work did not begin early enough to permit a study of the spring migration. In eight hours spent in the study area on March 6, 1951, one male was flushed. The absence of fresh droppings and tracks and the complete silence on the area indicated most of the grouse had not arrived on this portion of the summer range. In 1952 field work began on April 13. At this time the study area was populated by breeding grouse of both sexes. Thus if the downward migration occurs at the same time each year the grouse breeding near Quinsam Lake arrive between March 6 and April 13.

The homing behavior of the adult cocks is most striking. Spring after spring they return to the same locality of the summer range in which they were first observed and banded. As an example, male

No. 9 was banded in 1950 and was observed at the exact point of banding in and during 1950, 1951, 1952 and 1953. This behavior has been equally true of 46 adult males. If recorded in subsequent years they were at their original points of capture and in most cases under the same tree, log, or stump where captured.

The adult hens also show this homing behavior although their relatively wide movements on the breeding range makes it less precise than in the males. Five hens banded on one plot prior to the hatch in 1950 were observed on the same plot in 1951. Three of these hens and five others banded in 1951 were observed on the same plot in 1952. This suggests that the adult hens return each year to the same part of the breeding range, although they do not become as localized as the adult cocks.

THE BEHAVIOR OF THE ADULT AND YEARLING MALE IN SPRING

Once on the breeding range, the adult males become localized on territories. Territorial behavior is taken as an indication of a breeding male and in the sooty grouse consists of vocal display (or hooting) and isolation upon a well-defined area. In five adult males with from 30 to 80 observations on each during April and May their recorded positions never fell outside a territory from one to two acres. Within his territory the resident male courts females and repels intruding males by fighting and pursuit. The breeding males rarely leave their territories until the cessation of sexual activity in midsummer. Accordingly, routine activities such as feeding and sleeping are carried out on their territories.

In the spring, males in yearling plumage are approximately one year of age; males in adult plumage are at least two years old. The behavior of most yearling males on the breeding range is quite different from that of the adult cocks. While over 300 adult males have been observed holding territory but one yearling male was so recorded. Moreover, during the entire study only nine other yearling birds were observed. Two were captured, banded and released. None of the nine yearlings gave indication of breeding behavior as noted in the adult males. One banded yearling was observed at points 1,000 feet apart. The other was not seen again until two years later when it was located as an adult male holding territory. These two yearlings were not localized as the breeding males. In another case a yearling male was observed with a female in spring and there was a complete absence of sexual display. One observation, however, suggests abortive breeding behavior in a yearling. A yearling male was observed to hoot and display when a female landed nearby. The male did not remain in the locality.

If full territorial behavior is accepted as a criterion for a breeding male then most yearling males do not breed in their first year. The yearling males recorded hooting are an exception to this and had evidently attained some adult sexual development in their first year.

The relatively few yearling males observed on the summer range leads to a consideration of their migratory behavior. Generally, the yearling males behave as females prior to time of incubation and are observed under the same conditions. In this time, yearling hens are frequently observed while observations on yearling cocks are rare. Thus the small number of yearling males recorded is taken as an indication of their abundance on the breeding range rather than as a failure to detect them in the field. The sex ratio between females and adult males was calculated for one study plot in the spring of 1951 and 1952. This gave a sex ratio of 100:160 in favor of the females and indicates their greater abundance and the absence of the yearling cocks from the population of males on the breeding range. Beer (unpublished field notes) recorded sex ratios in favor of females on the breeding range in Washington.

It seems unlikely that the few yearling cocks observed on the summer range represents the true numbers of this sex and age class in the population. There is no evidence to suggest that yearling males die or move out of the study area at a faster rate than the yearling females to produce the disparity in sex ratio observed on the breeding range in spring. The simplest explanation for the absence of yearling males is that they are not with the breeding population on the summer range.

Beer (unpublished field notes) provides important data from a study made on the winter range of grouse in late June. This was in Washington near Lone Frank Pass and at elevations of 4,500 feet and above. Approximately 50 grouse were observed and all males. Ten birds were collected, and each was judged to be immature on the basis of light-colored plumage, smooth tissues of the neck and small size of testes. Clearly, the birds on this winter range in summer were what we have called yearling males.

These data fit well with the data obtained on the summer range at Quinsam Lake. The observations indicate that the absence of yearling males from the breeding range is caused by their failure to make the downward migration in the spring after hatch. Thus most males leaving the summer range as chicks do not return until their second or later spring and by this time in adult plumage.

A correlation between migratory behavior, breeding behavior and development of the gonads is well known (Bullough, 1945). With the difference in behavior between the yearling and adult male in mind, 53 adult males and 5 yearling males were collected in the spring and summer and the total volume of the testes was determined for each bird. In Figure 1(a) the volumes in cubic centimenters are plotted against the week in which the measurements were made. Week one is the week of April 13 to April 19. The series follows accordingly to week 13 (July 6 to July 12). Note the volumes of testes from year-

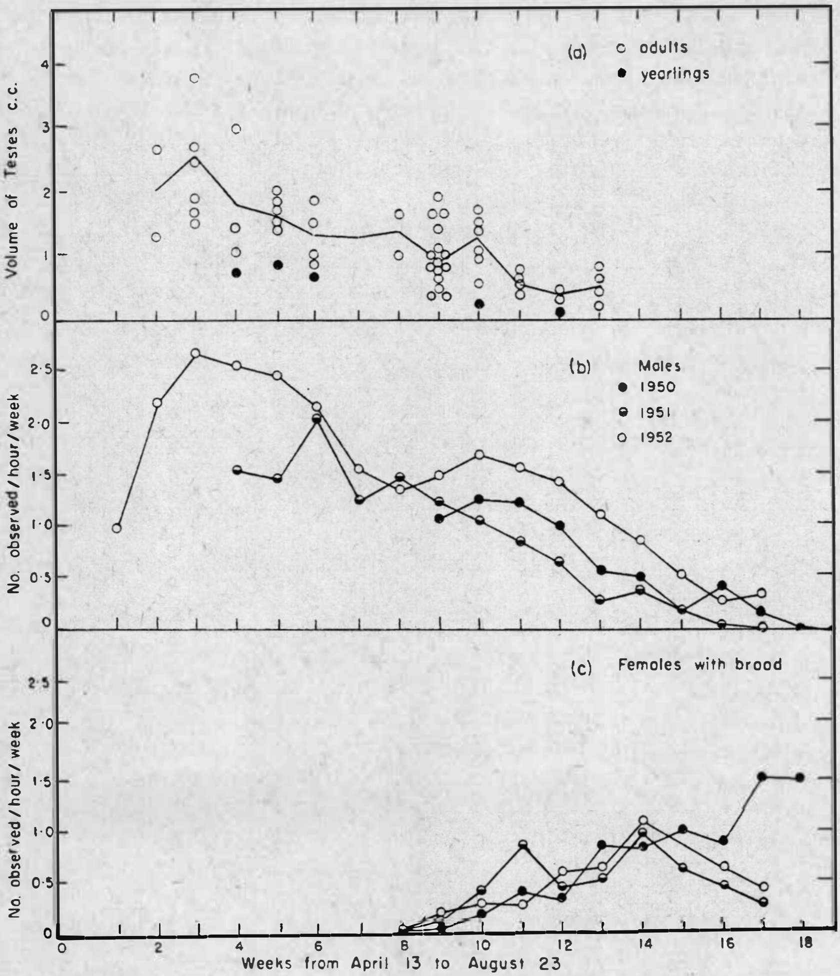

Figure 1. Trends in (a) volume of testes (b) number of males (c) number of females with brood, observed between week 1 (April 13-19) and week 19 (August 17-23).

ling males are less than that for adult males collected in the same week.

Clearly, the difference in breeding behavior between the adult and yearling male can be related to the difference in size of the testes and in turn the level of physiological development attained by the two age groups. The failure of non-breeding yearling males to make a downward migration in their first spring after hatch also indicates a relationship between migratory behavior and development of the testes. Both breeding behavior and migratory behavior are related to gonadal development. In the females, however, enlargement of the ovary does not occur until they are on the summer range. This may also be the case in the cocks for we do not have measurements of size of testes in early spring. There is some suggestion from Figure 1(a) that the average volume of testes did not reach a peak until the last weeks of April (week 2 and 3). From the same figure, average volume of testes decreases as breeding activity decreases and reaches its lowest value as the birds migrate from the summer range. This and the situation in the hens indicates peak enlargement of the gonads is not concomitant with migratory behavior.

THE BEHAVIOR OF THE ADULT AND YEARLING FEMALE IN SPRING

Although yearling hens are observed frequently on the summer range there is the possibility that they, as the yearling males, remain in numbers on the winter range over their first year. However, the absence of females from the winter range in summer and the similarity of breeding behavior between the adult and yearling hen suggests that yearling females descend to the lowlands in their first spring after hatch.

Unlike hooting cocks, hens prior to time of hatch usually make no sound nor display to reveal their positions. They are most often observed singly and sitting under a shrub, stump or log. At times two hens have been observed together and this most often in the early spring. On one occasion, as noted above, a yearling male was observed with a hen. These observations may indicate some tendency in hens to make the downward descent in groups or a more interesting situation wherein yearling males and females remain with the mother over winter and descend with her in the spring. Such a condition might explain the migration of the sexually undeveloped yearling males observed on the study area.

The ranges of the females prior to incubation are large in comparison to those of the territorial male. Seven banded females were observed from two to ten times and within areas from 5 to 20 acres.

Doubtless larger ranges occur. The ranges of the females overlap completely and, what is more important, are not related to the territory of a particular male. The breeding males court all females with which they come into contact. Males have been observed to court from two to six females as the females appeared on their territories. This is good evidence that the cocks, at least, are polygamous.

Between the time of arrival on the breeding range and the time of hatch the activity of grouse follows a most striking diurnal rhythm. By activity we mean the sounds and movements of grouse which can be detected by an observer in the field. In the males this includes: hooting, sounds of fighting, and the courting call which is given by the male at the climax of his courting display to the female. The female gives recognizable calls while in flight. These and actual sight records of grouse are also considered as activity.

To demonstrate the diurnal rhythm of activity two periods were selected: April 13 to May 15 and May 16 to June 5. In these periods all activity of grouse was recorded as the observer uniformly cruised the study plots. Records were kept by standard time in all hours of the 24 hour day. The day was arbitrarily divided into half-hour intervals and all observations made in a given half-hour were divided by the number of half-hours spent in the field during the given interval. This was necessary because time spent in the field was not uniformly distributed over all hours of the day.

The results of this activity analysis are presented graphically in Figure 2. Note the peaks of activity which occur between the hours of 3:00 to 5:00 a.m. and 7:00 to 10:00 p.m. (1900 to 2200 hours) in both periods. The greatest part of the peaks results from the sudden arrival of laying, incubating and non-breeding females at small openings where they forage vigorously. In the course of their movements the hens come into contact with males which respond with hooting and courting behavior. Also there seems to be a tendency for stronger and more general hooting in the morning and evening hours. Throughout the rest of the day in both periods there is a steady but lower level of activity over the study area. If the courting call is a prelude to copulation, then from the figure, copulation occurs at times in and between peaks of activity over the daylight hours. In the hours of darkness activity is nil.

When the hatch occurs and the intense morning and evening activity of the females ceases, the cocks still conform to the rhythm with hooting activity at a peak in the early and late hours although they may remain silent throughout the day. This continues as long as they hold territory.

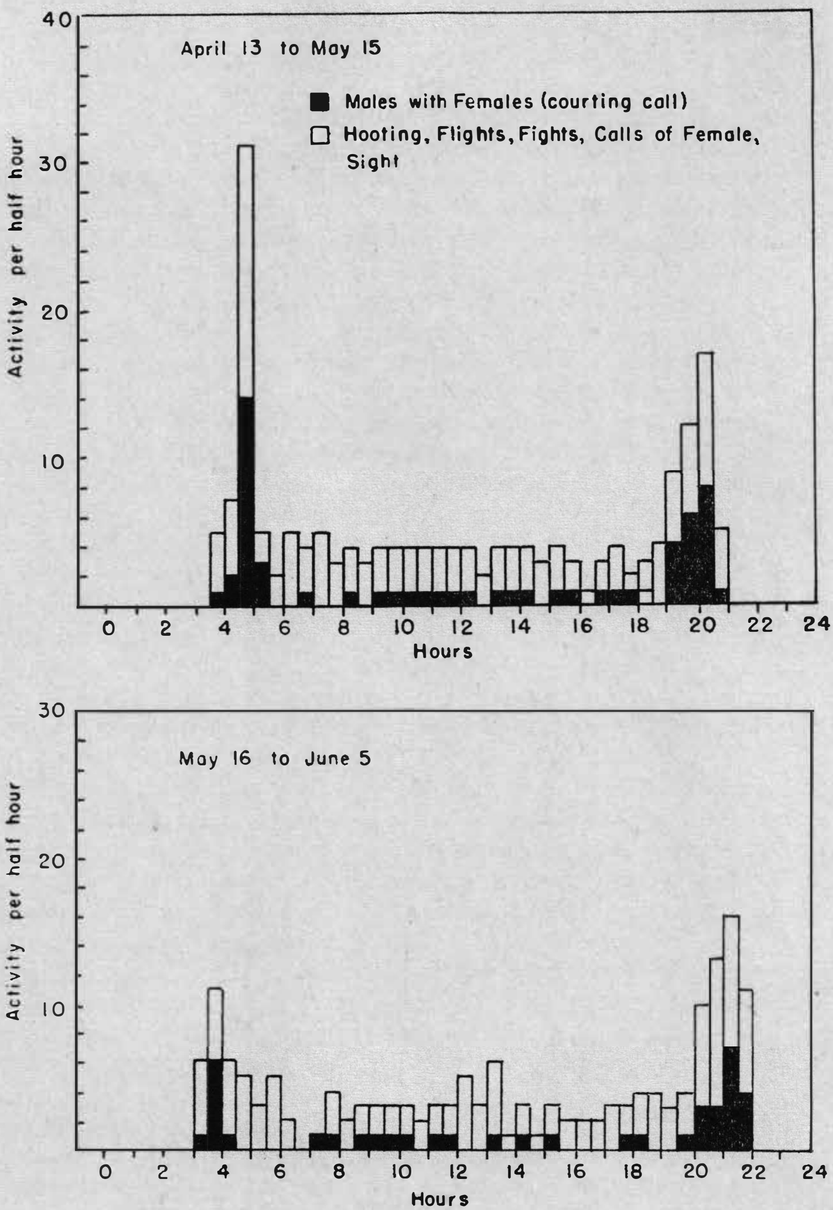

Figure 2. The diurnal rhythm of activity.

In comparing the two periods, note that activity begins later in the morning and ends earlier in the evening in the period April 13 to May 5. The morning and evening peaks are shifted accordingly. The precision with which activity begins and rises to a peak each day is lost in using the total number of days in each period in the expression of one 24 hour rhythm. While the peaks of activity appear to occur over two hours of morning or evening in the figure in any given day they last but 10 to 30 minutes. On a day in the middle of April, morning activity is at a peak between 4:30 and 5:00 a.m., and the peak of evening activity is between 7:00 and 7:30 p.m. On a day in late June the morning peak of activity is between 3:00 and 3:30 a.m. and the peak evening activity is between 9:30 and 10:00 p.m.

In the field, an observer can predict the beginning of evening activity in any given day from the measurement of light intensity. We held a General Electric D.W.-68 exposure meter directly into the sky to estimate light values. Light fades rapidly after sunset to 2 foot-candles then fades slowly to 1 foot-candle over a period of 15 minutes to one-half hour, depending upon atmospheric conditions. Each day between 4 to 2 foot-candles, the intense activity of evening begins and continues past zero foot-candles as recorded by the meter. In the morning, intense activity begins before there is a reading on the light meter and continues until a light level of two to four foot-candles is reached, and then rapidly subsides.

The precision with which activity begins each day, the tendency for morning and evening activity to become progressively earlier and later as the season progresses and the predictable nature of the time of activity, at two to four foot-candles, strongly suggests that light is a factor influencing this phenomenon. Since vigorous foraging is always part of the morning and evening movements of the hens it seems logical to believe that the intense activity is a response to a hunger drive and the low levels of light act to regulate this response. On the other hand, the males do not feed during times of peak activity of the hens and continue the diurnal rhythm in hooting activity long after the females abandon it at the time of hatch. The hooting activity of the cocks can be considered the response to a sex drive which is regulated by the low light levels. In the spring the behavior of the female tends to intensify the hooting of the male.

During times of peak activity the birds leave shelter and move about in the open. By doing this they are probably more vulnerable to predation. Activity at low light levels, however, would offer a compromise between sufficient light for efficient movement and not enough light to make these movements in open vegetation dangerous.

PRODUCTIVITY OF THE HENS

From the examination of 13 yearling and 48 adult hens captured or shot at random, Bendell (in press) found 70 per cent of the yearling hens and 96 per cent of the adult hens were breeding birds. Several of the non-breeding yearlings had resorbing ova and a shed follicle suggesting abortive production of eggs.

The development of ova in the females was studied by autopsy of 22 productive hens. At least five hens were examined in each week from May 4 to June 1. These data were augmented by palpating hens captured in the field.

Two adult hens collected in late April had undeveloped ovaries and oviducts as encountered in the fall. In the first week of May from three to five developing ova and thickened oviducts were observed in hens sampled. By the second week of May the first shed ovarian follicles were observed amongst developing ova which indicated the birds were beginning to lay. Two hens captured in this time had palpable eggs in their oviducts. In the third week of May maturing, ova were still visible with counts of shed follicles increasing. At this time birds were first observed with a developing brood patch. By the end of May production of ova had ceased and counts of shed follicles had reached a maximum. Hens examined after this time had full brood patches. Thus production of ova begins in late April and early May and continues while the eggs are laid in the second to last week of May. The brood patch is incipient at the beginning of laying and becomes fully developed at the cessation of laying activity and the beginning of incubation.

The rapidity with which the ovary shrinks after the brood patch is formed is worthy of note. In all hens sampled, once the brood patch was developed the production of ova had ceased and the ovary had returned to a size found in hens in fall. This would suggest that once incubation had begun a destroyed clutch would not be replaced.

Six nests of adult hens discovered on the study area averaged six eggs per nest with a range from five to seven. Nine yearling hens collected in late May were examined for ripe ova and shed follicles. From this, their average clutch size was four eggs. Thirteen adult hens collected in the same period and examined in a similar manner produced an average clutch of five eggs. The difference between the averages is not statistically significant. Therefore, on the basis of counts of ripe ova and shed follicles, yearling and adult females produce the same number of eggs. The figure of six eggs per breeding hen seems a best estimate for the study population.

The length of incubation was recorded for one clutch as 24 to 25 days. One nest was observed to hatch on June 7 and another on June 16. To estimate the week of peak hatch the number of week-old broods observed per hour per week in the field was calculated from May 18 to August 2 for 1950, 1951 and 1952. A total of 40 week-old broods was observed; 8 in 1950, 15 in 1951 and 17 in 1952. In the three years 80 per cent of the week-old broods observed hatched between June 8 and June 28. Peak hatch occurred between June 15 and June 21.

Behavior of the Female and Brood

Generally the range of the female and brood (or family group) is larger than the range of hens prior to incubation. There is, however, a great amount of variation in the size of ranges recorded for different family groups. Observations on 25 family groups indicate movements of one half mile are not uncommon. On the other hand, over a summer, several families were not observed outside one or the other of the 36-acre study plots.

Wing *et al.* (1944) report that the organization of broods begins to break down in late July and shuffling of young from one brood to another occurs. In 1950 from mid July on, in 128 observations on broods, 37 were records in which the typical family group had changed. In 27 of these cases, 2 or 3 hens were associated with from 10 to 20 young. In six cases a female was observed with from 7 to 15 chicks. Finally in four instances 1 to 4 chicks were observed alone.

Contact between families is apparently made in the course of feeding activity; however, the reaction of one hen and chicks to the calls of another hen and chicks is also a contributing factor. As the female and brood moves about and feed they become temporarily localized in an area of good food supply. Other family groups become localized in the same area and intermingling takes place. There is no evidence that the hen recognizes her own chicks or at this time the chicks recognize their mother. Thus upon the break-up of the feeding groups hens are found with any number of chicks from the original band.

In 1951 and 1952 brood disorganization as observed in 1950 did not occur. It is known that summer temperatures over the three years were comparable but 1950 was relatively wet as compared to 1951 and 1952. In the period July 13 to August 18 in 1950 rain fell for a total of seven days. In the same period in 1951, rainfall was zero and fell for a total of two days in 1952. Under these conditions vegetation on the summer range was rapidly desiccated. It seems likely this reduced the supply and distribution of acceptable foods and in turn minimized the tendency for broods to congregate by remaining in an area to forage. This explanation if correct, suggests that the weather influences brood organization through its effect on the vegetation of the summer range.

THE FALL MIGRATION

The cessation of hooting and the abandonment of territory are preludes to the altitudinal migration of the cocks. As the breeding behavior changes, the number of males on the breeding range dwindles. As an example, on one plot in April, 18 hooting males were recorded; in May, 16; in June, 14; and in July, 6. By the end of July but two hooting males remained on the plot. Again, from a consideration of Figure 1(b) the number of males observed per hour per week in the field, the seasonal trend in the population of males is most obvious. The departure begins in late April and early May (weeks 3 to 5), continues throughout the summer and is near completion by August (weeks 17-19). The pattern has been similar over the four years of study. The marked change in behavior of the males is associated with changes in their internal and external appearance. Note how the trend in average volume of testes Figure 1(a), reflects the trend in numbers of males observed on the summer range, Figure 1(b).

The pattern of fall migration in family groups differs markedly from that of the cocks and probably is attributable in most part to the behavior of the young. From a consideration of Figure 1(c) hens with young did not begin to leave the summer range until mid July (week 14) of 1951 and 1952, and this appeared to be an early migration as compared to 1950. The variation in the three years with respect to time and rate of migration of the family groups fits well with the explanation of shuffling in broods and suggests in this case that desiccation of the vegetation also speeds the migration of hens with young from the summer range.

Some light is shed on the nature of the migration of hens with young by the recovery of bands. Two chicks and a hen banded in the summer had not moved towards the highlands by September 16 of that year. In two family groups and a chick, however, there was a distance of two to three miles between point of banding and point of recovery of the band. Although these birds had moved towards the highlands their directions of movement were not the same. If their lines of travel are projected to the nearest mountains as an asumed winter range, then the birds would overwinter more than 9 miles apart. This if true, suggests the fall migration is a dispersal and the spring descent in the adults at least would be a convergence on a relatively small part of the summer range. Hence the summer range would appear to be a particular area while the winter range is a general area spread over the upland regions.

SUMMARY AND CONCLUSIONS

1. Sooty grouse were studied on a part of the breeding range at Quinsam Lake, Vancouver Island.
2. Adult sooty grouse return to the same part of the summer range each year between March 6 and April 13.
3. The territorial behavior of the breeding male sooty grouse consists of song and isolation upon a well defined area. Within this area intruding males are repelled and females are courted. The male remains on his territory throughout the breeding season.
4. Most yearling males do not breed and do not migrate from the winter range in their first spring after hatch. This behavior is correlated with a lack of gonadal development.
5. The breeding males are polygamous. The range of females is not related to the territory of a particular male.
6. Activity in the spring follows a well-marked diurnal rhythm with two peaks of relatively intense activity in each day. Light intensity between two to four foot candles is important as a factor regulating the activity peaks.
7. The production of ova begins in late April and early May and continues while the eggs are laid from the second to the last week of May.
8. The average clutch size is six eggs with a range between five and seven. Breeding yearling and adult hens are equally productive.
9. Peak hatch occurs between June 15 and June 21. Eighty per cent of the hatch occurs between June 8 and June 28.
10. The upward migration of the males begins in late April and early May and is near completion by August. There is a relationship between the cessation of breeding activity, the upward migration and decrease in average volume of the testes. The upward migration of the hens and young does not begin until july. There is variation between years in time and rate of migration. It is likely that weather influences brood organization and migration through its effect on food supplies on the summer range.
11. The upward migration seems to be a dispersal to a relatively large winter range.

ACKNOWLEDGMENTS

This work was carried on under Dr. J. R. Adams and Dr. I. McT. Cowan in the Department of Zoology, U.B.C. Financial support was provided by the National Research Council (Canada) and the Research Council of Ontario.

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DISCUSSION

DR. PAUL DALKE [Moscow, Idaho]: I have two or three questions. What relationship did you find in yearling birds as to the bursa?

MR. BENDELL: That's a very interesting point, sir. There is evidence that the bursa persists quite a long time in these birds, probably until they are two or three years of age. The presence of a relatively deep bursa and yearling plumage mark a bird as one year of age. Adult plumage and a shallow bursa might indicate a bird of two or three years of age.

This is particularly true of males. In the females the situation is not as clear. The bursa seems to involute faster in the females than it does in the males, and this seems to be associated with the fact that most of the hens breed in the first year, whereas the cocks don't breed until their second year at least.

DR. DALKE: I have another question. Did you have indication of renesting?

MR. BENDELL: I am sure that some renesting must occur.

When you plot the frequency of occurrence of week-old broods over the summer, the curve tends to be bell-shaped with a second bell appearing to the right. The second bell is presumably caused by late hatchings delayed by the destruction of a hen's earlier clutch.

DR. DALKE: Late in the summer did you observe large numbers of family groups starting their migration back to the winter range?

MR. BENDELL: Yes, we did. In 1950, which was a relatively cool and wet year, as you probably remember, on the Coast, we got quite a bit of evidence of shuffling among broods. There were large groups of young birds with maybe two or three hens, and this indicated a shuffling in the broods. These groups were then migrating bands; but in 1951 and 1952 we didn't find anything like that at all, and this may be related to food conditions on the summer range.

We postulated that if the food is not abundant or is not distributed in the proper pattern on the summer range, then there is no tendency for the broods to become localized in certain areas.

I think it's probably a factor of the bird, which has a tendency to do that, plus the factor that conditions in the environment influence this behavior, and this is one way we try to explain it. That's all.

IMPLICATIONS OF SOCIAL BEHAVIOR IN GRAY SQUIRREL MANAGEMENT[1]

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The object of this study was to determine the type of social behavior of the gray squirrel (*Sciurus carolinensis*) and to determine, if possible, the effects of behavior on population levels. The population aspect is being continued for a few months and will appear as a separate paper.

The manner of reactions among individuals of a species has important implications in the population dynamics of that species. The study of social behavior has received considerable attention among birds in recent years (Lack 1953, Nice 1943, Kluyver and Tinbergen 1953, and others) but has been relatively neglected among mammals. A species exhibiting territorial behavior might have its population density at higher levels regulated either directly or indirectly by increasing conflicts over territorial boundaries. A species exhibiting a social hierarchial type of behavior might be subject to population regulation through social strife since increased harassment of low-ranking individuals at high densities could serve as a regulatory factor.

In order to make a detailed study of squirrel behavior it was necessary to employ a method for visual recognition of the individuals. A combination of clipping hair on the tail and a fur dye, Nyanzol A, (Fitzwater 1943) proved satisfactory. This dye is almost black, does not fade, and is lost only in the spring and fall molts. The hair on the terminal 2 inches of the tail of males and on the basal 2 inches of the tail of females was clipped close to the skin. Tail hair was

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[1]Maryland Federal Aid Project W-32-R.

regrown only in the fall molt. The animals were toe-clipped for permanent identification so that after molting they could again be dyed and tail-clipped.

Two woodlots of 10 acres each which were relatively isolated from other forest land were selected. These woodlots were about one-third mile apart and being on the enclosed grounds of a hospital were not disturbed by human activities. The woods are of red oak type (Forest Service Field Manual, 1950) containing huge red oaks, yellow poplars, and beeches, some of which exceed a diameter of 4 feet at breast height. A portion of each woodlot was lightly grazed, which reduced the vegetational obstruction to observations.

A grid was laid out in each woods with white stakes located at 100-foot intersections. Detailed maps were prepared, including the grid and prominent landmarks, and mimeographed for each woodlot. A separate map was kept for each squirrel and, whenever a particular individual was seen, its location was denoted on the map. The type of activity and date were recorded on the reverse side of the sheet. Traps were fairly evenly spaced throughout the woods. Upon first capture the animals were dyed, the hair of the tail was clipped, the toes were clipped to give it a permanent number, and then the squirrel was released. On subsequent capture the dye was retouched as necessary.

Because two or more squirrels were rarely seen close enough together for social interactions to occur, observations made by simply sitting in the woods were impractical. Three feeding stations were therefore erected in each woodlot and the squirrels were observed at these stations from a blind. These feeding stations consisted simply of an ear of corn on a wire coat hanger fastened to the trunk of a tree about 6 feet from the ground.

RESULTS

A. General behavior. Most of the squirrels when feeding at a station hung by their hind feet, with their head level with the ear of corn. The squirrel would tear off a kernel with its incisors and holding the kernel by the forepaws consume the germ while in this inverted position. After the germ was consumed, the remainder was dropped and another kernel was detached from the cob. The remains of kernels falling at the base of the feeder tree were consumed by other squirrels or birds. Some individuals seemed to prefer running to the ground or up to a branch for consumption of the kernels removed from the cob rather than hang by their hind legs. Frequently a squirrel which had apparently satisfied its hunger would begin industriously to remove kernels one at a time and run out about 5 to 15 feet from the tree base to bury them among the leaves of the forest floor.

While feeding, some individuals appeared completely engrossed, but most were alert, and a slight movement from the blind often alarmed the animals. Should an individual's suspicion be aroused, it would watch closely for a while but, finding no further cause for alarm, would continue feeding. Often, however, the individual would begin barking, which might cause any other nearby squirrels to stop and look about. Some squirrels did not respond to the barking and, if the barking continued with no tail flicking, those that did respond would resume their activities. If the alarmed individual became sufficiently aroused, it would flick its tail in jerks. This action would alarm all other nearby squirrels which would ascend nearby trees and frequently sit on a branch to join the barking and tail-flicking. A hunter's squirrel call (giving the barking call) was tried several times from the blind. A few individuals ran off, but almost all looked up at first and soon went about their business paying no further attention to the call.

A .22 rifle was once fired less than 15 feet from four animals, frightening them away. Two of them were back within half a minute. Squeeks, sneezes, or rustling of leaves might sometimes cause alarm, but on the whole it appears that squirrels rely much more on sight stimuli than upon sound as a signal for flight. They respond more to tail jerking than to their calls.

B. Social behavior. A major aspect of this study was to determine if squirrels are territorial or social. The evidence concerning behavior may be considered under three topics (a) communal dens (b) overlapping of home ranges and (c) type of social behavior.

On two different occasions squirrels were seen using communal dens. A den in the south woodlot under observation from April 15 to July 14, 1953 was occupied by at least five squirrels simultaneously. A den in the north woodlot was occupied from February to May of 1954 by at least six squirrels (four females and two males). The age composition in the latter group excluded the possibility of this being a family group of mother and young.

Studies on home range, to be reported in detail elsewhere, showed that the average estimated home range of males was 2.0 acres and that of females was 1.2 acres. A simple t-test shows this to be significant at about the 2.5 per cent level but because such factors as time of year, whether females are pregnant or nursing young, and placement of traps were not considered, these values may be at variance with the actual situation. Since each woodlot occupied 10 acres in area and had up to 50 squirrels apiece, considerable overlapping occurred, and hence it would appear that the squirrels did not have territories.

Territorial behavior was never noted. However, a type of behavior occurred that could be misinterpreted as territorial defense by an observer watching unmarked squirrels. During two days in August, 1954 a number of squirrels were seen feeding in the top of a hickory. An adult male (No. 14) appeared very intolerant of two particular individuals and spent a great deal of time on both days chasing first one and then another into neighboring trees. Other squirrels were not molested even upon coming within several feet of No. 14. This type of behavior is exhibited by a high-ranking individual toward those of lower rank at a feeder as described below. If some of the squirrels had not been distinctly marked, it would have appeared that one squirrel was busy chasing all the others out of the tree, and this might have been interpreted as territorial defense. To be considered territorial defense, the defender would have had to drive out all others especially those of the same sex. Two of the squirrels that were unmolested were males and one of those chased was a male. Robinson and Cowan (1953) state that adult females vigorously defend the tree containing their nest against other females, but the opportunity to observe such a situation never occurred.

A female squirrel which had been captured in Baltimore and released in the woodlot fed amicably at the base of a corn feeder with other squirrels. She had assumed a low rank at the cob, but at the tree base the other individuals did not exhibit any hostility toward her. This observation is interpreted as another example of lack of territorial behavior in the gray squirrel.

During observations from the blind (over 350 hours of observation) four or five squirrels often fed around the base of the tree on the kernels dropped by the squirrel feeding at the cob. If the individual at the cob went away, his place was immediately taken by another. If the first individual returned, he drove off the one at the cob. In only two instances did two squirrels feed together at an ear of corn. The squirrels were extremely intolerant of each other at the corn cobs and any individual approaching to within about 2 feet of the cob was either threatened or driven off. By recording which squirrels won at these encounters it was possible to set up tables showing the relative rank of individuals (Figure 1). Frequently an individual of lower rank would try to sneak up and remove a corn kernel from the ear.

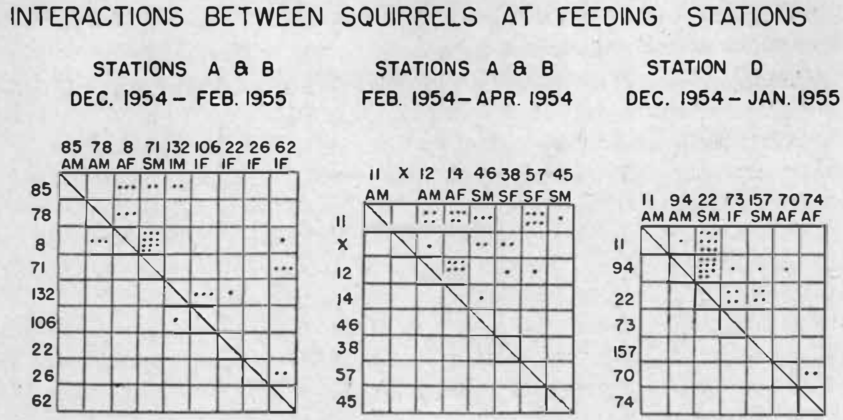

Figure 1. The numbers at the top and side of each square represent an individual squirrel. Each dot represents an encounter between two individuals. If a dot lies above the diagonal line it represents a victory for the individual at the left. If the dot lies below the line it represents a victory for the individual at the top. For example squirrel No. 8 was victorious over No. 71 11 times. A = adult, F = female, I = immature, M = male, S = subadult.

This act resulted in either a threat or a chase by the one feeding at the cob and the retreat of the former. Sometimes there would be an extensive chase over the ground or up the tree trunk and along the branches. To threaten, the squirrel turns its head toward the intruder, lays back its ears and sometimes utters a few high pitched growls. It was impossible to see whether or not there was any tooth grinding as in the European squirrel (*Sciurus vulgaris*) (Eibl-Eibesfeldt 1951). The agressor, in attacking, would strike out at the intruder with the forepaws. The attacking squirrel was never seen to bite though this could possibly occur and not be seen.

One of the individuals, a male (No. 11) behaved as a despot and was especially intolerant of others. He frequently chased those that approached the ear of corn for longer distances than was usual (20 to 30 yards) and was once seen far from the feeding station chasing another individual on an extended course through the trees. He sometimes would chase squirrels feeding at the base of a tree below a feeding station. This individual, however, resided in a den with five others.

On a number of occasions a squirrel was seen chasing another at top speed through tree branches, down and up tree trunks and over the ground. When these chases involved marked animals (five cases) the order of the chase was never reversed. With unmarked individuals it would be easy to imagine that the squirrels took turns chasing each other. These chases were probably a high-ranking individual chasing one of lower rank and appear similar to those that occur sometimes at feeding stations.

Among 64 cases of interaction between squirrels of the opposite sex a male won 48 times (75 per cent). This is a significant difference between victories 50 per cent of the time, which would be the case if there were no difference in rank due to sex. This is probably even more significant since 11 of the 16 victories of a female over a male were made by one particularly aggressive female. Out of 30 interactions involving an older and younger animal of the same sex 29 victories (97 per cent) went to the older individual. Out of 59 interactions between old and young animals in which sex was ignored, 47 victories (80 per cent) went to the oldest. These data indicate that the social rank of an individual may be determined by seniority as in cattle (Schein 1954). The young squirrels coming into a population are subordinate to the older squirrels, but upon maturing they in turn assume dominance over younger individuals. Males (especially adults) assume a higher rank than do females.

This dominance of males over females contrasts with the observations of Eibl-Eibesfeldt (1951) on a pair of captive *Sciurus vulgaris* in which the female dominated the male until pairing, at which time the male assumed the lead.

In contrast to the intolerance shown at the corn cob feeder three to five squirrels would feed amicably together at the base of a feeder tree on kernels dropped from above. Occasionally a squirrel would chase one of lower rank on the ground.

Squirrels appeared to live together amicably with little strife. Antagonism seemed to occur mostly at close proximity at a concentrated food supply. Other cases of antagonism were observed (rapid chases through trees) but they appeared to be relatively infrequent. At the high population levels (approximately 50 per 10 acres) existing in the experimental woodlots social strife has little influence as a population regulatory mechanism. Because a gray squirrel is not a territorial species, the building up of high populations by intensive management would not be limited by territorial strife nor is it likely to be limited, at the densities encountered in this study, by any other type of social strife.

Low-ranking individuals did not appear harassed. If a high ranking squirrel was feeding somewhere, the lower ranking individuals simply left him to feed by himself. The food supply of squirrels is scattered diffusely and favored feeding spots (such as corn-cob feeding stations) are rare. Even in tree tops, a nut crop or buds are fairly wide spread and permit the lower ranking animals to feed without getting near the high-ranking individuals.

C. Mating behavior. Three mating chases were observed between July and December 1954. In one case an undetermined number (5+) of males pursued a female through the trees. The female traveled at a more leisurely speed (in contrast to full speed in the intolerance chases), and the males were noticeably excited. Males sometimes lost track of the female and would run off in a wrong direction through the trees or over the ground. Two males fell out of the trees in the excitement.

The other two mating chases were even more leisurely than the first but only one and two males respectively were in persuit. When the female paused, the male would catch up and appear to sniff the base of her tail whereupon she moved on again. No sounds were noticed during these chases nor was there any apparent antagonism between males. Perhaps the highest ranking male was in closest pursuit and the lower ranking males kept a discrete distance.

Males showed great interest and excitement upon sniffing where an estrous (?) female (object of the chase) had paused. One male even gnawed off a few bits of bark at such a spot. Chases due to intolerance of high ranking individuals toward those of lower rank are performed at a faster speed than are mating chases. Only two squirrels take part in the former type whereas there may be more than two involved in a mating chase.

D. Behavior after introduction of strange squirrels. With hopes of learning something concerning the behavior of "stocked" squirrels 16 individuals that had been trapped in Baltimore were dyed and released in the woodlots. Following the release, intensive trapping and field observations were continued to determine the behavior of the animals. All but three left the woodlots so quickly that no further records of them were made. One squirrel moved to another woodlot (about ⅓ mile) and took up residence in a nesting box. Another squirrel remained in the woodlot in which she was released and built a nest in a box. The third squirrel was reported by a farmer as having stayed around his pig pen (¼ mile from point of release) for several days before disappearing. A fourth squirrel was reported by two boys about two hours after release about 300 yards from the woods where released.

Upon release, the introduced squirrels were conspicuously bewildered. They either ran erratically over the ground and soon ran wildly away or else they ascended a nearby tree and remained in a crotch for varying periods of time up to three hours. Not one was seen to take refuge in the many available dens. This behavior was in striking contrast to that of the resident squirrels which upon release from a trap were well oriented and frequently disappeared into a nearby den.

Apparently squirrels released in unfamiliar areas do not usually remain (only 1 of 16 liberated stayed in the woodlot in which released) but begin traveling, probably aimlessly. The movements in this case were not obviously due to the direct influence of resident squirrels but may be due to some sort of restlessness of the released individuals in the unfamiliar surroundings.

Releasng squirrels in a strange locality may therefore have little value in increasing the stock in the immediate vicinity because most of the animals would leave immediately. Perhaps the population of a general region could be increased, but the effect of stress on animals released in strange surroundings may cause heavy mortality. However, since the squirrels for stocking may, as in the case in Maryland, have been acquired by removal of nuisance animals from urban areas, this is probably the most efficient use for the animals. The public relations value of such a program for the removal of nuisance animals probably greatly outweighs any restocking value.

SUMMARY

Squirrels were trapped and individually marked for field recognition on two 10-acre woodlots in Maryland.

Locations of where animals were seen or captured were denoted on individual maps and observations were made from a blind at feeding stations. Because of the great amount of overlap in home ranges, the use of communal dens, and lack of territorial behavior, squirrels were classified as a social animal and not territorial in nature. They were found to have a definite hierarchial type of social structure.

Sixteen squirrels were trapped in Baltimore and released in the two woodlots. Only one animal remained in the woodlot where released, one moved to the other woodlot, one stayed a few days at a nearby farm, and all the others disappeared.

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DISCUSSION

DISCUSSION LEADER HOLDER: It isn't at all bad that we have rank among squirrels, because it certainly improves hunting when the high-ranking squirrels start chasing the low-ranking squirrels. Are there any questions at this time?

MR. CHRISTISEN: I'm wondering about the seasonal movement of squirrels as a cause for reduction of their activity during a particular season of the year. In Missouri we have hunters who sometimes say that the squirrels aren't there in July and August, and we suspect that maybe the squirrels are there, but we're not certain. Do you have any notes regarding that matter?

MR. FLYGER: Yes. The population aspect of this thing, however, is going to be published elsewhere. It's still being conducted with these marked animals. Often that will give you much different results than trapping would.

For instance, last August it was very difficult to catch squirrels in a trap, but I saw many of them in the trees—many more than I had seen before—but a little while later the tables were reversed. I didn't see nearly as many squirrels as I knew were there by trapping results, and it's just hard to say how many squirrels are there by either method.

MR. CHRISTISEN: Were these isolated woodlots that you worked with?

MR. FLYGER: Yes, quite isolated, but not absolutely. There was some communication with other woodlots, but they were essentially islands.

MR. CHRISTISEN: Did you have any knowledge of the ingress or egress, what amount of movement of strange animals there was into your area where you worked, and did that happen at a particular time of the year?

MR. FLYGER: I'm afraid I can't answer that right now. I don't know.

DR. W. M. SHARP [State College, Pennsylvania]: I was very interested in Vagn's remarks regarding the hierarchy demonstrated at the feeder. We have been taking advantage of that behavior pattern in trapping, in that the traps are located about feeders, and we have found that it's very practical to set the traps around on the ground. The squirrel that gets up there first keeps his other hungry brothers and sisters away, and they naturally wind up in the trap. We have been having very good luck with that type of behavior from a trapping point of view.

I might add what we have done on trapping an area that is not isolated, but is adjoined by other woods. One of our main areas is just a segment of a forest, and the ingress and egress of squirrels is not as great as you might think they are. Last spring the population showed as a result of trapping that around 41 per cent of the population were the tagged squirrels of previous years, and some of them had been there quite a number of years. There is a little mixing up, however, of course. At least that's what we found.

MR. FLYGER: Well, my trapping results show that they stayed pretty close to home too. There were new individuals coming into the area. Some of them stayed in the same place for over two years and didn't move on, that we know of.

THE STRIP INTERSECT CENSUS

PAUL MOORE

Ohio Division of Wildlife, Columbus

INTRODUCTION

Game inventory is generally accepted as a necessary operation in game management and research. Game technicians and administrators, however, are of several opinions as to how precise inventory methods need to be. The greatest disagreement comes between those who feel that indices are adequate and those who believe that absolute measures are a necessity. The first group ignores the fact that a valid index may readily be converted to an absolute estimate. Thus, the split in opinion is actually between those who prefer inexpensive inventory methods which may be of questionable or unknown validity and those who believe that a method cannot be considered cheap if it is not valid. This paper concerns an inventory method which is designed to produce valid, direct estimates of absolute numbers of game. It is a report on six years of experience with the strip intersect census, a method which appears to have many practical uses in game management. The method combines strip sampling methods with the direct proportion principle of estimation (Lincoln, 1930). It is not a new method. Robinette *et al.* (1952) used it in the census of winter-lost deer in Utah. Tests of this method along with other types of strip censuses (Robinette *et al.*, 1954) showed it to be accurate and highly reliable in estimating numbers of deer carcasses or dummies, when proper sampling techniques were used. The principles presented in the present paper were derived from work with farm-game species in the Middle West. It appears certain that a great number of additional situations exist where the strip intersect census method will be of value. The objective of this paper is a thorough consideration of problems involved in sampling and census design.

ACKNOWLEDGMENTS

The writer wishes to acknowledge the contribution of all those who shared in the early field studies or conducted the more recent field operations, especially Archibald B. Cowan who, with the writer, realized the application of the strip intersect census principle in estimating survival of color-banded pheasants in 1949; James O. Lee, James N. Wright, George Fennen, William B. Hendershot, Cleon Webb, Dale Whitesell, Roger McElroy, and many others on the Ohio Division of Wildlife District staffs who supervised or participated in widespread field use of the strip intersect census method; William R. Edwards, Rodney Smith, the late Frank Haller, and members of the field crew at the Olentangy Wildlife Experiment Station who pioneered a box trapping modification of the census.

The writer also wishes to acknowledge help in statistical evaluation of the method, by D. Ransom Whitney and Paul Moranda of the Statistical Laboratory, Ohio State University. Thanks for advice and encouragement are due Willet N. Wandell, Thomas G. Scott, E. D. Martin, and Floyd B. Chapman, the writer's supervisors during various phases of this work. Special thanks are due Charles A. Dambach, who first as a graduate research advisor and more recently as a wildlife administrator, has guided and aided in the development of the strip intersect census method.

CENSUS THEORY

The use of marked individuals in estimating total populations is widespread in the field of biology, although strip counts have seldom been used as samples. Lincoln (*op. cit.*) was apparently the first to suggest the use of recovery rates of marked animals in estimating total populations, although Jackson (1933), working with the tsetse fly, arrived at the same method independently. Jackson (1936, 1937, 1939, 1940, 1943-45, and 1947-49) also did much to develop the mathematical treatment of such data with particular reference to populations rapidly changing in numbers and composition.

Given an original sample of identifiable individuals, randomly selected from an unknown population, the proportion of repeats in a random subsequent sample is an estimate of the proportion of the original sample to the total population.

This is shown symbolically below. The validity of such a method of estimation is conditional upon random selection of samples and

when: X = total population

O = original sample

S = Subsequent sample

R = repeats, individuals common to both O and S

$$\frac{O}{X} = \frac{R}{S}$$

or

$$X = \frac{(O)\ (S)}{R}$$

independent probabilities of occurrence in each sample. Under these conditions, there is a uniform probability (P′) that any one individual will be selected in the original sample and a uniform probability (P″) that any one individual will be selected in a subsequent sample. It is a basic probability theorem that the probability of joint occurrence of two independent events is the product of the two separate probabilities. Substitution of these values in the formula given above, shows that for any individual, X is an estimate of one.

$$X = \frac{(O)\ (S)}{R}$$

$$X = \frac{(P')\ (P'')}{P'P''}$$

$$X = \frac{P'P''}{P'P''} = 1$$

Similarly, when these probabilities are applied to a specified population of n individuals, X is an estimate of n.

$$X = \frac{(O)\ (S)}{R}$$

$$X = \frac{(nP')\ (nP'')}{nP'P''}$$

$$X = \frac{n^2P'P''}{nP'P''}$$

$$X = \frac{n^2}{n}$$

$$X = n$$

Application of this estimation formula to strip counts requires suitable identification procedures and adequate sampling methods. Let us consider, first, the problem of sampling. If a census area is any form of a parallelogram and two intersecting sample strips are selected paralleling the sides of the total area, it can be shown that the total area is estimated by the product of the two strip areas divided by the area common to both strips.

This is illustrated in Figure 1. Diagram 1a shows a hypothetical census area having a length of *m* units of *b* and a width of *n* units of *h*. In *1b*, intersecting strips paralleling the sides are shown, and in *1c*, *1d*, *1e*, and *1f*, the areas involved in strip intersect censusing are described algebraically. Substitution of these algebraic terms into the

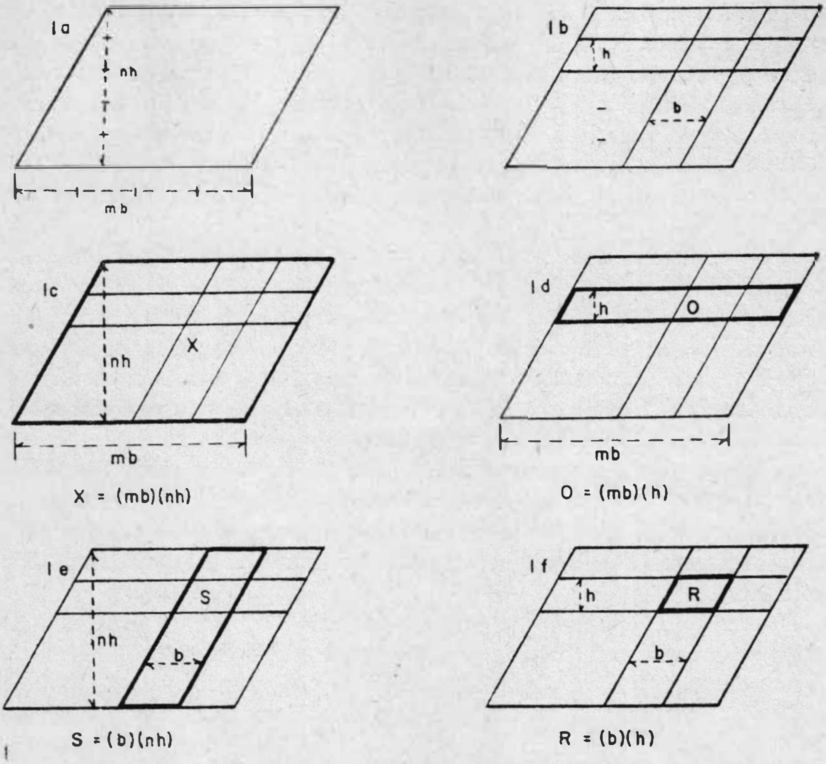

Figure 1. Algebraic relationship between areas of census strips and census.

direct proportion estimation formula shows that total area may be estimated from strip area samples.

Where: $X = mbnh$

$O = mbh$

$S = bnh$

$R = bh$

$$X = \frac{(O)\ (S)}{R}$$

$$mbnh = \frac{(mbh)\ (bnh)}{bh}$$

$$mbnh = \frac{mb^2nh^2}{bh}$$

$$mbnh = mbnh$$

In practice, however, the total area to be censused is known or can be readily determined. It is the area in the sample strips which is unknown. On the other hand, it is the total game population which is unknown while that in the effective census strip is known. Under one condition, that of random distribution of the game population, the sampling probailities are identical for both the proportion of the game population sampled and the proportion of the area effectively censused. Under this condition then, counts of game in the sample strips may be used to estimate the total game population directly without attempting to determine the area effectively censused. This assumes of course, that individuals in the game population are identifiable.

Let us consider now what situations in the field meet the condition of random distribution. The ideal situation, uniform distribution of game, is a rarity, and for all practical purposes does not occur. The practical ideal is that where game density, although not uniform, does not have a pattern correlated in any way with the pattern of census strips. In this situation, replication of census areas will result in practically uniform distribution in the pooled sample.

A second problem in sampling is that uniform sampling probabilities must prevail in at least one of the two samples. Robinette *et al.* (1954) reported one case in which non-uniform probabilities probably biased results. Failure to achieve uniform sampling results in underestimates.

Securing independent samples is a third sampling problem. In most situations, intersecting sample strips paralleling the sides of the census area fulfill this condition. When the occurrence of an individual in one sample, however, limits or otherwise affects the probability of its occurring in the other sample these conditions are not fulfilled. Procedures must be designed to avoid this.

The fourth sampling condition is that individuals occurring in the samples must be identifiable. A great variety of bands, tags and markers are now in use which make this possible in many situations. In addition, individual plumage or pelage characteristics and behavior characteristics such as territorialism, home range, and the integrity of groups of individuals appear to make possible the identification of individuals or groups with sufficient reliability to permit the use of the strip intersect census on many unmarked game populations.

The final condition which must be met is that movement of identified individuals must be confined to the census area or be measurable. In practice, where movement is slight in relation to the dimensions of the census area, this may be disregarded although it does produce a bias.

STATISTICAL CONCEPTS

In addition to the conditions described above which are basic to the validity of the strip intersect census, it is useful in census design to develop a mathematical model to portray the effective census strip and the home range of individuals in the game population. These need not be especially accurate to have a practical use in establishing the amount and intensity of census effort necessary in a specific situation.

The effective width of a census strip has been a matter for speculation ever since King developed the strip census for grouse (Leopold, 1933). In the present method it is not essential that the effective width of the census strip be known. It can easily be calculated when the total population estimate is obtained. Figure 2 illustrates the essential features of sampling efficiency as related to the effective width of the census strip. Diagrams 2a and 2b show the pattern of sampling probabilities which likely prevail along continuous and intermittent census strips. Diagram 2c indicates the pattern of sampling probabilities which would develop with continued census effort on these strips. Diagram 2d illustrates a strip with 100 per cent sampling probabilities which would be equivalent to the indefinite strips in a, b, and c.

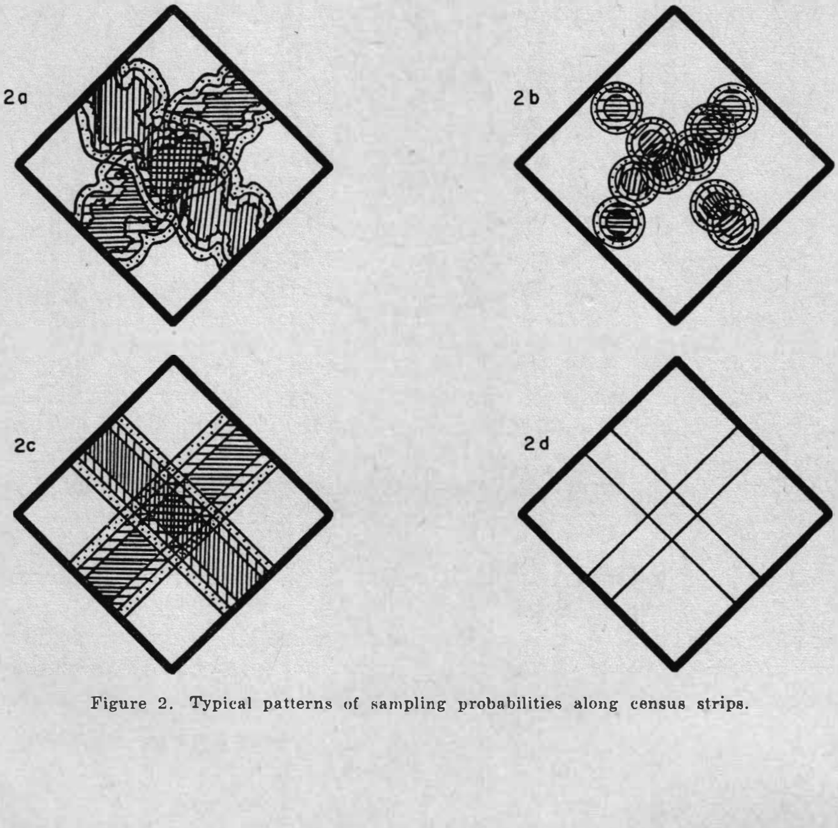

Figure 2. Typical patterns of sampling probabilities along census strips.

Most efforts at describing home range or territory have treated the occupied range as a finite unit. That is, the outermost points of occurrence of an individual have been connected by a line and the area within considered as the occupied range of that animal. The writer has found it useful in strip intersect censusing, and for many other purposes, to consider that the occupied range of an individual is a sample expression of a population characteristic peculiar to the species, sex, and age group to which the individual belongs. The model home range characteristic of the entire population may be considered to be a circular frequency distribution such as that shown in Figure 3.

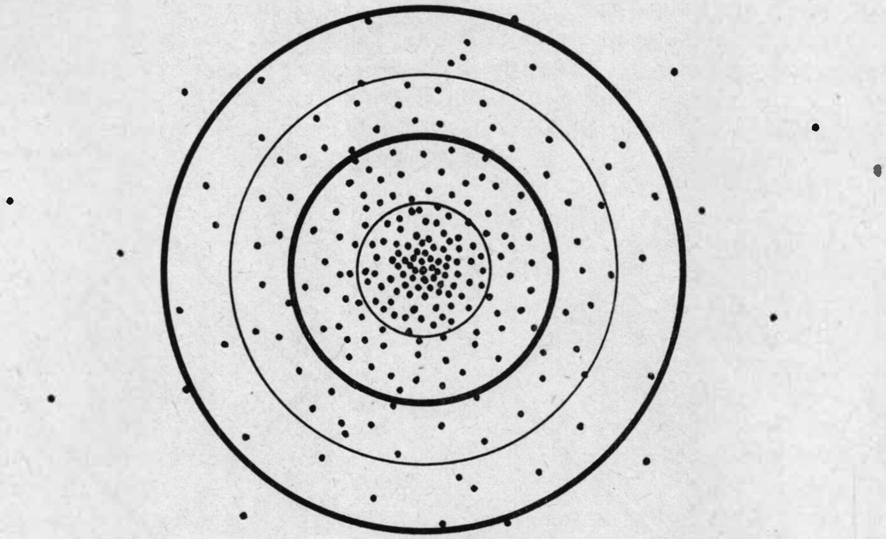

Figure 3. Circular frequency distribution model of home range.

This distribution has no boundaries, yet may be precisely described by appropriate statistics. The author (Moore, 1950) proposed a more specific model for the occurrence distribution of territorial cock pheasants, the bivariate normal distribution. Although this is still an untested assumption, it provides a more realistic description of territory than one requiring definite limits. Moore (*op. cit.*) found that during the period April 11 to May 31, 1950 the range of territorial cock pheasants could be described by a standard deviation of occurrence of 0.1151 mile. This value was obtained during a time when territories were shifting. A value more descriptive of stable territories or home range could be obtained with a shorter observation period. Where an expression of an actual unit area is desired, a circular plot having a radius of two standard deviations will include the bulk of the occupied range.

The foregoing concepts of indivual home range and the effective census strip are useful in determining the optimum size of census areas, the best intensity of census effort on each census area, and the necessary number of census areas. Where sample statistics are available to indicate the standard deviation of occurrence, the effective width of the census strip, and the distribution of expected game density over the total census area, the optimum number of census areas and intensity of coverage can be designed in advance. If past experience has demonstrated the variability inherent in the type of census used, consideration may also be given to securing estimates with fixed confidence limits.

CENSUS DESIGN

Figure 4 shows some of the sampling possibilities when the census area does not greatly exceed the home range in size. The individual having home range A is properly considered a resident of the census plot because its point of mean occurrence (home range center) falls within the area. Its location in relation to the census strip also makes its occurrence in the strip sample possible. Individual B, on the other hand, must be considered a nonresident even though it, too, has a good opportunity to occur in the strip sample. Individual C is another non-resident, but the chances that it will occur in a strip sample are remote. These cases show that strip samples will include some individuals not resident in the census area. This bias should be minimized by making census areas large in relation to home range. The remaining bias can be removed by the use of a correction for movement to be discussed below.

Figure 5 illustrates the relationship between the effective census strip width and the size of the individual home range. Individual A has an excellent chance of occurring in the strip sample, B a good chance, C only a slight chance, and D none at all. Increasing the census effort on the marked strip would increase the chances of including A, B, and C in the sample, but would not affect D. Increasing census effort by adding a parallel census strip through C and D would, however, result in a larger increase in the sample. The latter method of increasing census effort is preferable in most circumstances. Where the home range is large in relation to the effective strip width, however, the first method may be nearly as efficient.

It is sometimes necessary to limit the size of census areas to the

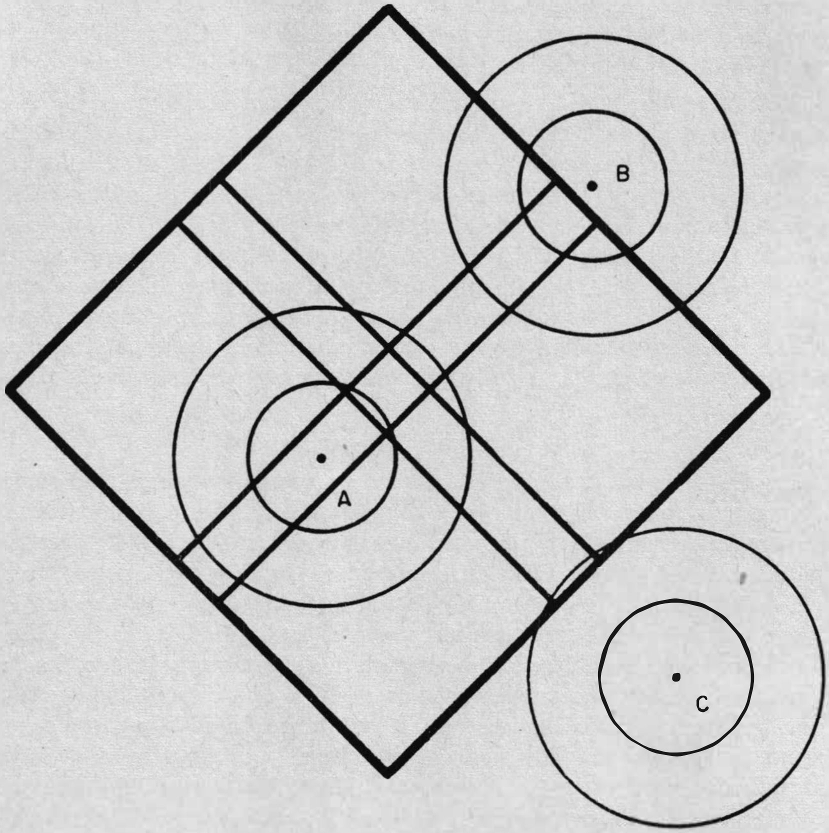

Figure 4. Examples of resident and non-resident sampling in a strip census area.

smallest possible area which will yield valid estimates. On the basis of preliminary studies, this appears to be a square large enough to include one individual home range (a circle with radius of two standard deviations). This necessitates the use of a correction procedure to remove the bias due to movement. Figure 6a shows such a census area. The dashed lines indicate the surrounding region from which movement of individuals into the census area may be expected. Movements exceeding two standard deviations from the home range center are ignored. The procedure for deriving the correction for movement will not be explained in detail, but can be best understood if the census area is considered as consisting of four smaller blocks as shown in Figure 6b. If these four blocks were located within a large group of such blocks, and the problem was to make an estimate which applied only to the four blocks, this could easily be done. The home range center for each individual could arbitrarily be assigned to the block in which it first occurred in the census sample. Animals from other blocks could be identified and ignored in the census sample.

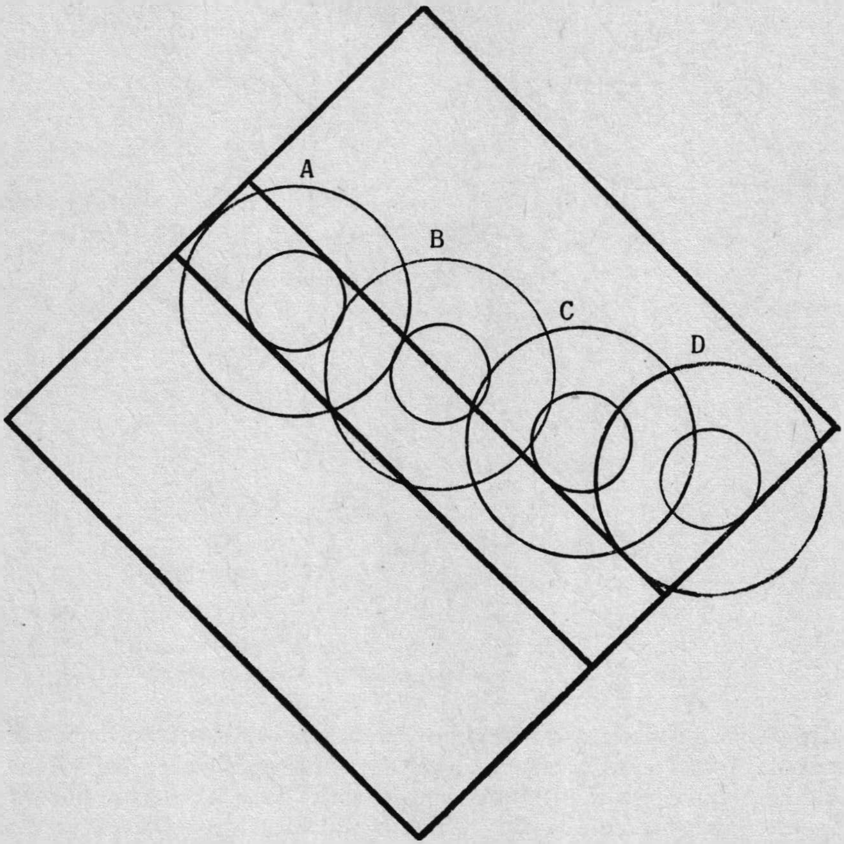

Figure 5. Sampling possibilities from a single census strip.

One of the basic sampling conditions of the strip intersect census is that the game population is distributed at random. Under this condition the recorded movement from one block to another may be used to estimate unrecorded movement between two other blocks. For example, unrecorded movement from A to B in Figure 6c may be estimated by the recorded movement from B to C. When the

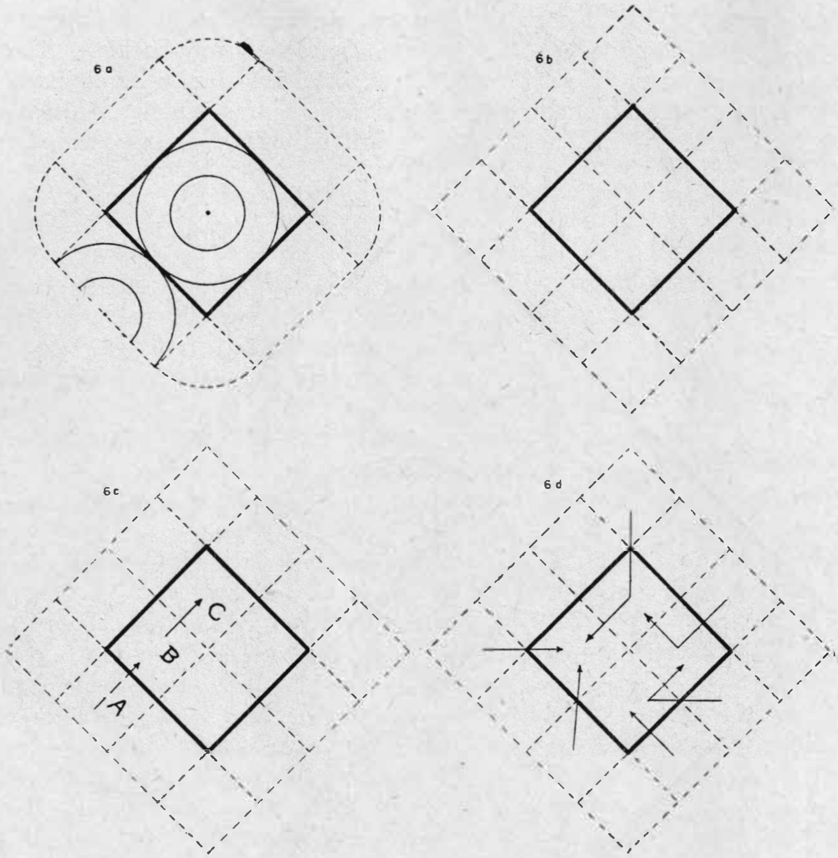

Figure 6

dimensions of the census area are at least twice the standard deviation of home range occurrence and movement between all parts of the four blocks may be recorded, correction estimates may be secured for all types of movements (see 6d) which would bias the census estimate. Extremely long movements (greater than two standard deviations) are ignored but these are probably insignificant in effect.

In the writer's opinion, the principal uses of the strip intersect census will be in the estimation of game populations on large areas from a small percentage sample. In addition to random variability inherent in the census method, variance components related to density and time will usually be important also. Knowledge of the relative importance of these sources of variability can be used to select the most efficient census design. Split sampling to derive two independent estimates for each subsample is highly desirable. This additional feature permits the partitioning of variance into its principal components through an analysis of variance. Standard statistical analysis may also be used to establish confidence intervals of these estimates. In the case where only a single estimate is made the method of De Lury (1951) can be used to establish fiducial limits.

POTENTIAL USES

Future uses of the strip intersect census will be limited to populations which may be individually marked and subsequently captured or observed, or to unmarked populations which are individually identifiable because of some natural characteristic. Mutually exclusive home range in combination with some size, age, color, or marking variability appears to be the characteristic of widest applicability in censusing unmarked population. Low or medium density populations of territorial cock pheasants have been censused in Illinois (Moore, *op. cit.*) and low to high populations of quail coveys have been censused in Ohio (Wright, 1954). Territorial males among waterfowl also appear suited to strip intersect censusing. The minimum counts now being made might be converted to absolute estimates, which would be comparable regardless of cover conditions, merely by redirecting the current census efforts. Audio strip censusing of breeding male grouse, bobwhites, turkeys, and pheasants at the height of the breeding season appears to be a possibility. Early efforts in Ohio, however, indicate that it should not be attempted without observers competent in triangulation of game bird calls. Game bird broods should be suitable under some conditions, for strip intersect censusing in the same manner as quail coveys.

In all censuses of unmarked populations some inaccuracy in identification is unavoidable. The effect of such inaccuracy has not been evaluated, nor has much been done to determine how accurate identification of unmarked individuals can be. These are matters which should be given future study.

The strip intersect census has many possible applications on inanimate or immobile objects. A census similar to that reported by Robinette *et al.* (1954) might well settle questions about the extent of the illegal kill of hen pheasants. On intensive plot counts of hard-to-find objects such as game bird or rabbit nests, strip intersect census design might provide an estimate of total nests which would include those missed. On aerial strip counts of potholes, ponds, etc., strip intersect census design could eliminate doubt as to the exact width of strip covered and compensate for any incomplete counting. Minimum speeds on air counts, however, may be too fast to permit accurate identification.

Strip intersect census also obviates the need for original and subsequent sampling. Except where it interferes with independent probabilities in the two samples, both may be taken concurrently. Movement and differential mortality, hazards of Lincoln Index estimation, are thus minimized.

The writer has been associated with tests of five different applications of the strip intersect census principle. None of these methods has been fully evaluated as to its range of use, but enough is known to briefly describe the methods and discuss problems associated with their use.

Territorial cock pheasants were censused in 1950 during late April and the entire month of May on a 36-square-mile area in Kendall County, Illinois. Roads occurred mostly on section lines and were used as census strips. These strips were covered repeatedly during the census period and samples on east-west and north-south roads were used as independent samples. Location, individual plumage characteristics, and some color-band combinations were used to identify individual cocks. It was suspected that cock distribution was correlated with the census grid pattern and that the roadsides strips sampled below-average densities. This could not be tested, however. Intensive coverage of the census area resulted in an average original sample amounting to nearly 40 per cent of the estimated population so the bias due to non-random distribution may have been negligible.

A similar estimate was made of unmarked midsummer adult cock populations in 1949 in Livingston County, Illinois, and the average original sample was 20 per cent of the estimated population. Here again, non-random distribution was suspected but could not be tested.

An effort was made to extend the strip intersect census principle to roadside counts of pheasant broods in 1950 on the Kendall County Area. An area of 120 square miles received 2,500 miles of roadside observational effort. Only three broods occurred in both samples so that the total estimate may be quite inaccurate. If it were correct, however, it indicates a sampling rate for the average original sample of only five per cent. Such low sampling efficiency would limit the applicability of the method in pheasant brood censusing.

William R. Edwards led a crew in a field census of pheasant broods in Wood County, Ohio in 1953. Quadrate two-square-mile areas with corners oriented north-south and east-west were selected as census areas. These were intensively stripped on foot with 10 strips in each sample. Broods were observed to run for some distance ahead of the crew, making identification of individual broods by location of observation a questionable practice.

For the last three winters, quail coveys have been censused in Ohio on the same kind of two-square-mile census area as described above (Wright, *op. cit.*). Covey behavior is much more favorable to the application of the strip intersect census than that of the pheasant broods in Wood County. Home range behavior is well marked and coveys are not so prone to run ahead of the observer. Sampling rate has been about 30 per cent. This limits the usefulness of single census area estimates on low density range. During 1952 and 1953, however, regression analysis indicated that the flushing rate provided an unbiased index measure of census estimates. The census estimates were used to calibrate the flushing rate index in absolute terms. Whether this will be possible under all weather and cover conditions is not yet known.

Box trapping has been adapted to strip intersect censusing through random placement of traps along intersecting trap lines. During the last two years, areas of about 50 acres have been censused over a six-night period. Thirty-six traps are placed on each of two intersecting sets of six trap-line strips. Here the capture in one set of strips eliminates the possibility of capture in the other set so that original and subsequent samples are taken on alternating nights. The resulting split samples provide two estimates which are desirable for a complete analysis of variance.

Effort involved in box trapping requires that the census area be kept as small as possible, consistent with maintaining the necessary size relationship between census area and home range. Experience to date indicates that the box trap strip census is adapted to sample censusing of large areas.

SUMMARY

Methods of estimating game populations are in need of improvement. The strip intersect census is a method for obtaining improved estimates under certain favorable conditions. It combines the strip sampling procedures of the King census method with the direct proportion principles of estimation proposed by Lincoln (1930).

A discussion of census theory shows that the direct proportion principle of estimation may be applied to estimation of area size where the area is a parallelogram and the samples are intersecting strips paralleling the area boundaries. Such estimates, themselves, are of little value, but, under the condition of random distribution of game, identical sampling probabilities pertain to game populations

of the area. This permits the use of strip counts of identifiable individuals in the estimation of total game populations.

Concepts of the effective census strip and of home range are outlined to further develop census theory and its effect on census design. The actual census strip, an indefinite area in which varying sampling probabilities occur, may be compared to an effective census strip of definite width and 100 per cent sampling probabilities. Home range is compared to a circular frequency distribution of occurrence which may be described by standard statistical terms, the standard deviation alone if the additional assumption of bivariate normal distribution is made. A circular area having a radius of 2 standard deviations is a useful approximation of average home range.

Examples of situations requiring special consideration in census design are discussed. The relationship between home range size, census area size, and effective width of census strip determines the most efficient type of sampling rate and design.

The general principle involved in correcting strip census estimates for movement is described. Methods are suggested for designing census coverage to evaluate variance components and set fiducial limits.

Experience with various applications of the strip intersect census is outlined and the potential value of each use is suggested. Roadside strip censuses of low to medium pheasant breeding populations appear feasible unless nonrandom distribution of such populations is established. Both roadside and field strip censuses of pheasant broods, however, appear impractical. Field strip censuses of quail covies have been conducted on a large scale, and appear to be especially suited to sample censusing of large areas or regions. Box trap strip censuses of rabbits are under extensive field tests and also appear practical for large-area sampling.

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DISCUSSION

MR. EBERHARDT: Presuming this is unbiased, do you feel you would get enough information at the intersects to make this thing useful?

That is, the intersects are a small part of the area you covered. Do you get enough precision at that point to be useful, do you think?

MR. MOORE: I'm not sure that I understand your question. In the illustration I used a simplified diagram of the strip census area. In actual practice we have half a dozen or ten strips crossing a single census area in both directions, so that there are a number of intersections in which repeats could occur, and I think you are primarily questioning the accuracy of the identification of individuals, are you not?

MR. EBERHARDT: No, I was questioning the number of individuals that you have at intersections.

MR. MOORE: Well, that is a problem that limits its application. Now, on the quail covey strip census or coverages, working back, one may give us an estimate of 30 per cent of the coveys in an area being flushed. That would mean about 9 per cent repeats. That is probably about the minimum limit.

This method has its greatest potential usage, I believe, in an extremely small percentage sample of large areas—perhaps statewide areas. The very conditions are such that we can't place any too great reliance on a single census area.

DR. DAVID E. DAVIS [Baltimore, Maryland]: How do you estimate the variance on this method?

MR. MOORE: I believe that the soundest method of estimation of variances is to make split samples. With the box trapping modification of this, for example, we can't use samples from two sets of strips on the same night as independent samples. An aimal is confined to one set of traps and doesn't have the opportunity to repeat in another, so that we use one set one night, and compare that strip sample with the intersecting set on the following night. That second set on the first night is used in conjunction with the intersecting set on the following night, so that we end up with two samples from each area, and we have the ability to evaluate the inherent variability of the method, plus that due to distributional density.

THE CONTROVERSIAL SAN JUAN RABBIT

ROGER M. LATHAM
Pennsylvania Game Commission, Harrisburg

Recently the introduction of a small brown animal from the West has created quite a dispute among sportsmen and farmers, with the Game Commission or Conservation Department squarely in the middle in some cases. This is the San Juan rabbit, which is evidently a feral domestic form of the European rabbit (*Oryctolagus cuniculus*). Within the past two years, sportsmen's clubs and private individuals have been importing this stock from the San Juan Islands in Puget Sound into other states. Because of the destructive history of the European rabbit in many different parts of the world, these importations have aroused a storm of protest from both official and unofficial sources. I should like to present a brief history of this animal and a resume of developments to date in this country.

The San Juan Islands are located off the coast of the State of Washington. The European rabbit may have been introduced to this group first by the Hudson Bay Company, but around 1900 domestic stock was released by a lighthouse keeper as a meat production venture. His rabbits were of mixed domestic breeds, all presumably originating from *Oryctolagus*. By 1924, when the U. S. Biological Survey was asked for assistance in controlling these rabbits, they had reached plague proportions (over 30 to the acre) on Smith Island. All the succulent vegetation was eaten and little more than bracken (*Pteridium equilium pubescens*), tarweed (*Media exigna*), and cheat (*Bromus* sp.) remained. The entire island, which was a national bird reservation and a naval radio compass station, was so honeycombed by rabbit burrows that the buildings were actually undermined. Erosion was thus greatly accelerated, causing the bluffs to cave into the sea and the island to reduce gradually in size.

After poisoned baits were distributed over the 56 acres of the island, 621 rabbits were picked up, and it was estimated that twice that many were killed in their burrows with cyanide. Couch (1929), who investigated the rabbit situation on the islands and supervised the control measures, reported that the rabbits were malnourished and many were diseased.

The story of how the San Juan rabbit was brought before the public eye is an interesting one. As I understand it, the man who presently is the primary shipper of these animals is a beagle fancier. For some years he had been training his dogs on one of the islands. Through a little publicity given this rabbit by a magazine featuring hunting with hounds, some clubs and individuals requested this beagler in Washington to ship them a few pairs to try. They, in turn, kept a few hutches and distributed some of the offspring to other clubs. Very rapidly a demand was created, and the Washington man found himself in business. In a personal letter he states that about 50,000 rabbits are taken annually from the main San Juan island which has an area of about 50 square miles. He also stated that many more could be taken if the farmers did not poison the rabbits in the spring. He catches his shipping stock at night with a jeep and long-handled scoop nets.

In an effort to determine the attitudes of various state game departments toward the importation of this San Juan stock and to determine how many states had the authority to ban or regulate importations, a questionnaire was sent to all 48 of the states. From the 41 returns, it was found that 28 states have the power to prevent or control shipments. No state game departments have imported any of these rabbits, and only six expressed a knowledge of any being purchased by their sportsmen. Eleven signified that there was a demand by the hunters for these rabbits or for permission to import them. No states favored the introduction of these rabbits; 25 were definitely opposed; 13, mostly western states, were indifferent, and 3 did not express themselves. The results of this questionnaire would indicate that, generally speaking, the wildlife profession is opposed to the introduction of the San Juan rabbit.

In Pennsylvania, considerable opposition has developed against the importation of San Juans among farmers and farm groups. Articles and editorials have appeared in farm newspapers and magazines with wide circulation in the state. Many "letters to the editor" have expressed the fear of serious crop damage by this animal. At their annual meeting in December, 1954, the State Council of Farm Organizations passed a resolution demanding immediate cessation of shipments and the extermination of all European rabbits already released in the state. This is undoubtedly the strongest opposition force.

Another group opposed to the shipments is the American Rabbit Breeders Association, Inc. They know that the native rabbits are relatively immune to myxomatosis, but that a wild population of European stock, once infected, could endanger all of their domestic rabbits.

A third group includes the better informed sportsmen who, like most wildlife officials, believe that the San Juan rabbit, if it succeeded, would replace the cottontail rather than supplement it. These hunters are convinced that the cottontail is a superior sporting animal and do not wish to gamble on a substitute. Already in Pennsylvania, many of the clubs which have imported San Juans are disappointed in their sporting qualities. Evidently, the European rabbit is prone to sit in its burrows during the daylight hours and most often provides a poor chase before the hounds.

In its defense, many sportsmen deny that there is any basis for associating the San Juan rabbit with the wild European rabbit. They contend that this is a domesticated rabbit gone wild, and that the destructive qualities of *Oryctolagus* have not necessarily been retained. It should be pointed out, however, that the San Juan rabbit has clearly demonstrated its burrow-digging proclivities and its ability to destroy vegetation on the islands where it originated. Domestication regularly will jeopardize an animal's chance for survival in the wild, but experiences with feral cats, dogs, hogs, and other animals would indicate that their destructive qualities are usually retained.

Skins and skulls of eight San Juan rabbits were compared with skins and skulls of wild European rabbits at the American Museum of Natural History in New York. The Associate Curator of Mammals reported that he believes that they are of domestic stock—the so-called Belgian hare which is not actually a hare but a domesticated breed of the European rabbit. He found that the San Juan rabbit was very similar to specimens collected on Fisher's Island, New York. Except that they are larger, have larger feet, bigger ears, longer fur, and more massive skulls, the San Juan appears to be identical to its progenitor. He also is inclined to think that this rabbit has reverted somewhat toward the original wild stock.

Weights of about 50 San Juans were taken, including both the wild-trapped and pen-reared animals. The average for the shipped rabbits was about 5 pounds. Penned rabbits averaged a little heavier, but most of the specimens weighed were pregnant females. The largest of these weighed 7 pounds and 12 ounces. Records of weights for wild *Oryctolagus* in Europe placed the average at about 3 to 4 pounds with a maximum of about 6 pounds. Cottontail rabbits in Pennsylvania will average about 2 to 3 pounds, with a rare one going 4 pounds. This provides some comparison in size.

The sportsmen also point out that the rabbits found on the San Juan Islands show considerable color variation, as a further indication of their domestic ancestry. The same whites, blacks, and reddish-colored rabbits are also common in Australia, a fact which corroborates the historical records that some of the initial outbreaks on that continent originated from domestic stock. British rabbits are also reported to be multi-colored.

In Pennsylvania to date, upwards of 7,000 San Juan rabbits have been released. Something over 3,000 of these have been shipped from Washington, and about 4,000 have been reared in hutches. Shipments are made under a permit issued by the Pennsylvania Department of Health to the purchaser for a fee of one dollar. These permits were authorized only after the Washington State Department of Health had given the San Juan rabbit a clean bill of health. The State of Washington does not consider the San Juan rabbit as a game animal and believes, therefore, that they do not come under the jurisdiction of the Game Department. They are regarded as being the property of the landowner on whose holdings the rabbits live.

An attempt has been made, during the brief period since the first releases, to determine the survival, spread, and possible destructiveness of this animal in Pennsylvania. Unfortunately, all but a few hundred of these rabbits were stocked during this past fall and winter and there has been little opportunity for a quantitative study. From releases made during 1953 and 1954, it was found that some rabbits did survive two winters in the state. However, survival in general appears to be poor, and many sportsmen's clubs have already abandoned rearing or buying these animals because they disappeared so rapidly. Predation has been severe upon newly released rabbits, particularly upon the pen-reared stock. Almost no survival of young born in the wild has been reported thus far. Hunting season returns have been exceedingly disappointing, and no recoveries have been made from many early fall releases.

Hole digging has been insignificant so far. These rabbits adopt woodchuck burows very readily and show no inclination to dig their own so long as these are present. Noticeable hole digging would not be expected, therefore, until a sizable population was built up on an area. Because the releases for the most part have been in small numbers and widely scattered, there is little likelihood that burrowing would be evident so soon even if survival were good. In only one instance was noticeable burrowing found. This was in a ¾-acre fenced area where a number were being held for propagation. Here some resemblence to the typical "warrens" of the European rabbit was noted.

For the same reason, crop damage is not likely to occur when there are no concentrations of the rabbit anywhere. Some damage has been reported, but it has been restricted largely to woody growth during the present winter. On two release sites inspected, most woody plant material has been browsed heavily. Sumac, flowering dogwood, witch hazel, mountain laurel, blackberry, greenbrier, red maple, hawthorn, and even red oak has been barked and cut. Stems of these species up to one-quarter of an inch and larger were cut in quantity. This browsing is far more intense than normally suffered from cottontail concentrations.

At this point the moot question seems to be "what chance do these rabbits have of succeeding eventually in any of the several states where they have been stocked?" On the favorable side are many factors. Its wild counterpart is notoriously fecund and may produce five or six litters a year with as many as 11 in a single litter. The average litter is about five or six. Unlike the cottontail and most other rabbits and hares, the San Juan builds its nest underground in a burrow. In the United States, this habit alone might make a tremendous difference in final production, because surface nests appear to be quite vulnerable to predators, weather, farming operations, and other miscellaneous forces.

Stringent, in fact desperate, control measures in so many areas have appeared to be relatively ineffective against the wild European rabbit. In about 75 years, this rabbit spread over an area of 2,000,000 square miles in Australia. Over 7,000 miles of barrier fences were constructed across parts of this continent to no avail. Every possible method conceived by man's fertile mind has been used to destroy this animal in Australia and other countries. A few of these are the destruction of cover, trapping and snaring, poisoning (even with airplanes), fumigation of burrows, shooting, driving into nets, the use of dogs and ferrets, and the introduction of exotic predators, plus countless other less effective methods of killing. None of these, either singly or in combination, has prevented the spread of these rabbits, nor has effected a satisfactory control, in a great many countries throughout the world. Myxomatosis, the mosquito-carried virus disease, is the only truly successful control agent found thus far, and its value may be relatively short-lived.

Many sportsmen are inclined to ridicule the thought that these animals could become too numerous. They are firmly of the opinion, that given a long season, they could keep their numbers reduced to a tolerance level. However, present seasons and bag limits in most states do not prevent cottontail damage to farm crops, and the annual harvest appears to be well below 30 per cent of the fall population. In Pennsylvania, with one million hunters and a one-month season, the hunting season kill was found to be less than 20 per cent even on the most heavily hunted areas. It is doubtful whether hunting alone would ever prevent material farm damage once the San Juan rabbit was well established. Others point out that our native predators should keep it in check. The San Juan Islands have minks, raccoons, cats, and dogs in quantity. It must be remembered that this rabbit's burrows may constitute an extensive tunnel system covering as much as one-quarter acre and going to a depth of 9 feet. These are dug entirely by the rabbits themselves.

In summary, it appears that the San Juan rabbit if introduced to the mainland of the United States would have a reasonable chance to prosper. And if it succeeded, there is the ever-present possibility that it could become a prime agricultural pest and could compete seriously for food and living space with native rabbits and other game species.

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DISCUSSION

MR. MEL STEEN [Missouri]: Well, I anticipated that you would call on me, and I have something to say about this subject. In fact, I have quite a lot to say.

I want to point out to this audience and to this industry that this thing we're talking about is not pink tea. The fact is that it has all the potentials of making the English sparrow and the starling look like pink tea affairs.

There is plenty of danger involved in this animal, and in my humble opinion it doesn't make any difference whether he is wild or domesticated, black or white, red or brown.

The domestic strains—and I think there are approximately 125 of them, if I recall correctly—are well recognized, registered variations that have been developed by selective breeding down through the years from one common ancestor. This same animal is not only one genus, but one species.

We have every reason to assume that the domestic variations of that animal may have built-in genetic characteristics that will enable them to beat even a more dreadful beast in the original wild boar. Certainly, ability to destroy vegetation on the basis of size, if that is any criterion—we can expect that, because the one outstanding thing we have done by selective breeding is to greatly increase the size of this animal.

I would like to point out that although some apprehension has been expressed about efforts to confine this animal, the impact upon the domestic rabbit industry on this continent is something that has to be considered. Dr. Latham referred to myxomatosis. I personally doubt that that will be much of a hazard to the domestic rabbit industry on this continent, and we'll go into that if anybody is interested. There are reasons for that, but it would take a lot of time.

I want to point out one other thing, though, that the establishment of this animal on this continent will impose upon the American rabbit industry a very serious hazard—the habit of unfair and destructive competition. That industry even now is taking competition from the Australian rabbit, frozen and flown into this country and marketed in our markets for food.

I think this, that we can and should control the release of this animal, and I don't think we need to quibble about whether it be a wild or a domestic one. I can tell you what we are doing in Missouri.

Under our law nobody may release any animal to the wilds without the consent, the specific consent and supervision, of the Commission. We just got through prosecuting men for releasing wild cottontails in Missouri.

I propose to send my men to the field with instructions to exterminate any of the animals of this breed that may be found in Missouri.

In confinement this animal is a good and useful animal, a valuable fur and meat animal. On the loose he is pure menace, and I think it is incumbent upon this industry to prevent him from becoming established in this country, and protect the rabbit industry of this country from the hazard of this animal. He has been the most destructive for the longest with the blackest record of all the game animals that man has monkeyed with since the dawn of civilization. It's an open-and-shut question, and I think it's time we quit pussyfooting about the introduction of the animal on the North American continent. [Applause]

DISCUSSION LEADER HOLDER: Thank you, Mel. I'm with you 100 per cent.

I'd like to call on Harry Lumsden from Great Britain, and ask him to speak on some of the control measures that they have tried out in the British Isles.

MR. HARRY LUMSDEN: Mr. Chairman, the European rabbit in Britain has been a pest to agriculture for many, many years. It has proved extremely difficult to control. All kinds of methods have been used—snaring, trapping, shooting, poisoning, ferreting, and almost everywhere rabbits get out of control and do great damage to agriculture and forestry.

It's almost impossible, further than that, to reforest areas unless fenced or high up in the mountains. Garden damage is extreme also, and most of them have to be fenced.

I think that's everything I have to say, unless there are any questions.

MR. LAUCKHART: I'm sort of in the center of this thing which has been discussed from all sides, but I'm not here to defend releasing rabbits or sponsoring any program to get them established or anything like that.

However, I could give you a few little facts. The rabbit has been transplanted around the State of Washington for a number of years, and has not become established anywhere, except on the islands. They have been established on various islands in Puget Sound over quite a number of years.

As near as we know, the heavy snow in 1916 killed all of the rabbits, on pretty nearly all of the islands. The weather is pretty mild on those islands, and they have very little snow or cold weather, and I think that when these animals get to Pennsylvania they are really going to shiver, because they sure aren't used to that climate.

Most of the rabbits on San Juan Island itself were started in 1932 by a farmer who fenced his farm and raised rabbits. That's the way they do it there. At the time the fellow went out of business he released probably 2000 to 3000 rabbits all at once, but they were all domestic rabbits, and they have been harvesting them ever since. They have two slaughter houses for harvesting them for the market. The island is not desolated and chewed up. The rabbits do some damage, primarily the crops that they consume in living. That is, if you have 20,000 or 30,000 rabbits, they will eat quite a bit of food.

The farmers are not bothered too much by the rabbits digging burrows. They will dig, but mostly in the hillside type of location. The farmers complain more about the rabbit hunters than they do about the rabbits.

I think that the idea here is that you can't stop them. I believe that every state in the United States has had the Easter bunny released thousands of times, and probably more than that. The kids will keep them a while, and then let them go. The only difference is that they will survive on San Juan Island, and don't seem to survive here.

I don't think releasing any domestic animal for stocking purposes is probably a good principle, but I just a little resent picking on the San Juan rabbit and letting the other people release rabbits from somewhere else.

There is a lot of pressure in the State of Washington to prohibit the transportation of these rabbits, but they are farmed just like domestic rabbits; however, they are domestic rabbits.

There is no scientific classification. This term "San Juan rabbit" is just pinned on by a salesman that wants to sell a rabbit, and he's doing a pretty good job. It's nothing more than a domestic rabbit, and to prohibit exportation from the state we would have to prohibit the transportation of any domestic rabbit. Therefore, I think you are going to have to prohibit it in the states where they are released, and regulate what shall be released in your states, if you want to protect yourselves.

MR. LATHAM: We realize that domestic rabbits have been released a great many times in recent years, but we feel that most of those have been in small quantities, one, two, or three here and there, and oftentimes in the cities, where there's little likelihood of survival for any length of time.

Now, however, we are getting in some areas several hundred put out on one or two farms, which may be controlled by a beagle club, so there is a difference in stocking certainly.

In the second place, I know nothing about the physiology of this San Juan rabbit and how hardy it is, but the European rabbit has been established in all types of climates, within 2 degrees of the Equator to the islands off the coast of Chile in South America, on Tierra del Fuego, where the climate is supposed to be very similar to Southern Labrador—a very uninviting, windy, rocky island with extremely cold temperatures.

MR. STEEN: I want to add my say to what Roger has just said.

The release of a domestic rabbit and an occasional escape is one thing. Mass plantation of a wild animal—and I say he's wild, because he's living in the wilds—is quite another, and the chances for the success of the latter are infinitely higher than the former.

Everyone who knows the art of introducing an animal into a new range will realize that mass plantation is quite a different thing from an occasional escape. As far as ability to survive is concerned, he has thoroughly demonstrated that ability throughout the entire Temperate and Torrid Zones of this earth. He has succeeded in England at a latitude that approximates Hudson Bay. I'll grant you that the climate is milder, but he's that far north.

His history in Australia is that of a national calamity, a catastrophic event, as far as prostrating agriculture is concerned. He has done the same thing in Tasmania, in New Zealand, in Argentina, and he's living, as Roger said, successfully within 200 miles of the Equator.

There isn't any question about this animal; he's bad all the way through.

DR. JOSEPH J. HICKEY [Wisconsin]: I'd like to comment a bit on Mr. Lauckhart's statement that the San Juan rabbit has been repeatedly introduced into the mainland and the State of Washington. This has come out in their state publication this week. The history of this rabbit in the South Pacific is similar, and the animal was repeatedly introduced into New Zealand and, as I recall, it took about 30 years before an introduction took.

If Washington has had only a 30-year experience in introducing the animal, that is not a conclusive test.

Some years ago Professor Leopold said that a thing is right when it tends not to disturb the stability and beauty of the biotic community, and it is wrong when it tends to do so.

DR. CLARENCE COTTAM: I'd just like to add one other note in regard to this thing. The normal note on introduction is that it fails. In the history of introductions all over the world the thing is that it does not take hold. We introduced the English sparrow and the starling many times before they took hold, and when they did they became menaces.

That's the whole story. If it does take, it does so with such a vengeance that it does irreparable damage in most instances.

I was in New Zealand a few years ago. The rabbit has been there for over 100 years. The thing that occurs in latitudes comparable to Southern Canada—and when it was cold there, I darned near froze to death—is that the rabbits are so thick that, in one area of 1,600 acres, they took out over 96,000 rabbits in five years' time.

These things just take with such a vengeance that you have no conception of the population. They reproduce rapidly, and I think if these things got into Texas, where conditions are right for their survival, the result would be about as serious. I think the biological principle there is a very dangerous one, and I think we ought to know biologically what we are doing and why we're doing it and why it's in the public interest. Those things are easier to introduce than they are to eliminate.

Bearing in mind the extent of the damage in Australia it is just beyond comprehension to think of introducing them in the United States. They have been there for many, many years, and they have tried everything under heaven to control those things, and I think we ought to be cautious in the introduction of these animals.

I'm not speaking of the San Juan rabbit at all, but it's the principle involved that I want to call to your attention.

TECHNICAL SESSIONS

Tuesday Morning—March 15

Chairman: R. E. Foerster
Principal Scientist, Pacific Biological Station, Fisheries Research Board of Canada, Nanaimo, British Columbia

Discussion Leader: Francis W. Sargent
Director, Division of Marine Fisheries, Department of Natural Resources, Boston, Massachusetts

MARINE, COASTAL, AND FUR RESOURCES

STUDIES ON CANADIAN ATLANTIC SALMON

P. F. Elson and C. J. Kerswill
Fisheries Research Board of Canada, St. Andrews, New Brunswick

Atlantic salmon have been an important natural resource of eastern Canada from earliest times. For over 100 years, fluctuations in the yearly harvest have given rise to concern about the survival of the fishery. Commercial landings and values in the last two years are given in Table 1.

In addition to the commercial fishery there is an important sport fishery for salmon. The dollar value of this, while difficult to assess with any accuracy, probably equals or even exceeds the value of the commercial fishery.

Widespread concern about the future of our salmon led to the formation, in 1949, of a Federal-Provincial Co-ordinating Committee

TABLE 1. RECENT VALUES OF COMMERCIAL ATLANTIC SALMON FISHERIES IN CANADA.

	1953		1954	
Province	Pounds	Dollars	Pounds	Dollars
Newfoundland	3,088,000	705,000	2,153,000	519,000
New Brunswick	650,000	266,000	853,000	368,000
Nova Scotia	269,000	117,000	226,000	103,000
Quebec	526,000	203,000	444,000	197,000
Total	4,533,000	1,291,000	3,676,000	1,187,000

on Atlantic Salmon. Upon recommendation of this committee, increased emphasis was placed on the importance of salmon. The Fisheries Research Board of Canada expanded its research program, with the Province of Quebec participating in one phase. In addition, the Department of Fisheries expanded its salmon management program.

FLUCTUATIONS IN CATCH

Systematic records of mainland commercial catches are available since 1870. These show wide fluctuations in the annual landings, with a rather steady decline during the last 25 years. This decline has given rise to opinions that salmon stocks are being damaged by overfishing.

Too heavy removal of a year-class would result in a relative scarcity of its progeny. In the Gulf Area of the Maritime Region there is a period of six years between related generations, for nearly all the salmon. This area extends from Cape Gaspé, Quebec, to Cape Breton, Nova Scotia, and accounts for about two-thirds of the commercial landings for the mainland. The annual landings in this Gulf Area for the past 85 years are shown in Figure 1, A. The downward trend of the last 25 years shows clearly. It started at a time when catches were at an exceptionally high level. But the lowest recent year (1953) was not quite as low as the lowest year of the record (1881). The landings for the first 79 years of this period are arranged in order of magnitude, with catches of similar magnitude in chronological order from left to right, (Figure 1, B, upper part). These are the amounts removed from "parent" year-classes. Immediately below each of these "parent" catches are the catches six years later (lower part of Figure 1, B), which are the amounts removed from corresponding "progeny," or "offspring," year-classes.

While the parent catches have been given a definite order in respect to size, their corresponding progeny catches show only a random distribution. This is evidence that large removals from parent stocks have not had any effect on catches in subsequent years. It seems doubtful, therefore, whether additional restrictions on catches would lead to better fishing. However, other ways of increasing the value of our salmon are being brought to light.

PROGRESS IN RESEARCH

In Canada, there has been fairly intensive and continuous investigation of Atlantic salmon problems since 1930. This laid a foundation of knowledge about the life history of salmon and provided experience in useful research techniques. Figure 2 illustrates the general features

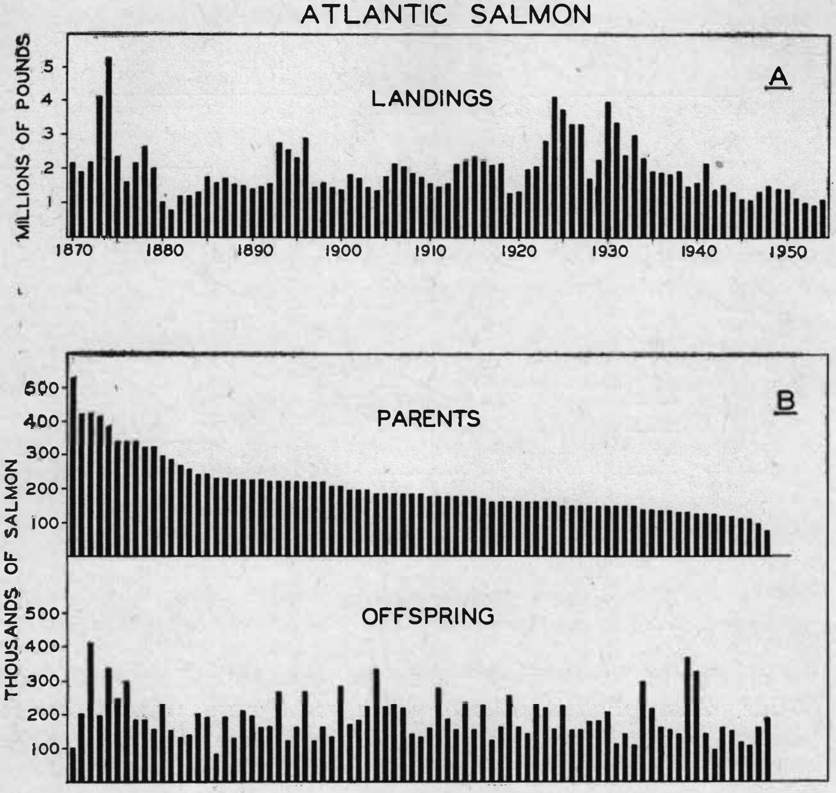

Figure 1. Commercial landings of Atlantic salmon from the Gulf Area (Cape Gaspé to Cape Breton) of the Maritime Region for the past 85 years.
A. Annual landings in chronological order.
B. Same catches plotted as numbers of 10-lb. salmon. Upper panel: numbers of parents in the first 79 years arranged in order of magnitude. Lower panel: numbers of offspring caught 6 years later, drawn directly below corresponding parent catch.

of the life cycle of Atlantic salmon. The duration of river and sea life varies more than indicated. The relationships of the different stages to fisheries are shown. Some of the interesting developments are outlined under the six topics below.

1. *Utilization of salmon stocks.*—Salmon taken in the various commercial fishing areas of the sea, including the estuaries of large rivers, may be produced by many fresh-water streams, often far away. For example, salmon marked as smolts in New Brunswick streams (Miramichi and Pollett Rivers) have been taken in relatively large numbers around Newfoundland, particularly along its east coast. Of those marked in the Miramichi, many have been taken as adult salmon in

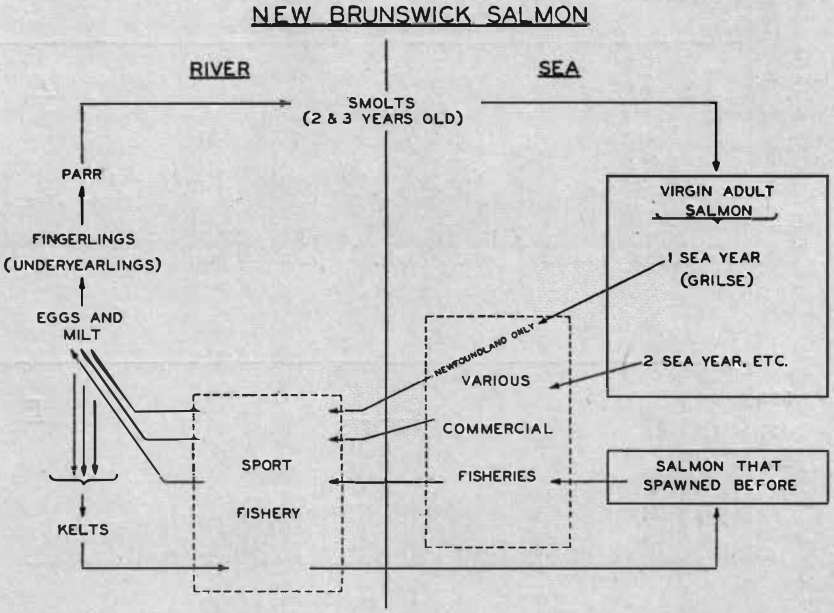

Figure 2. Life cycle and utilization of Atlantic salmon in New Brunswick, Canada.

the nearby drift-net fishery and in trap-nets extending up to head-of-tide. Many Pollett salmon which must have traveled several hundred miles were taken here also. In fresh water, however, marked adults have been recaptured only in the river system from which they came.

These points are shown in Figure 3 which illustrates the data of Table 2.

Since 1952, the smolt-marking program has been expanded. Marking traps are now operated on the Port Daniel River, Quebec, and the Little Codroy River, Newfoundland.

2. *The Miramichi salmon fishery.*—The Miramichi system is the center of the largest Atlantic salmon fishery in Canada. In this area studies are being made of the times, and sizes of runs of grilse and older salmon, in the various commercial and sport fisheries. These are combined with an analysis of life cycles based on scale reading. Such information is needed to evaluate the merits of present and proposed fishery regulations.

About the middle of May, a run of large salmon enters fresh water, followed in mid-June by a larger run of grilse. Both are eagerly sought by sportsmen after the angling season opens on June 5. Large

TABLE 2. RECAPTURES BY OCTOBER, 1953, OF ATLANTIC SALMON MARKED AS SMOLTS IN 1951.

Recaptures to October 1,1953 by	Marked as decending smolts in 1951 Northwest Miramichi River 33,407	Dungarvon River 14,966	Total Miramichi River 48,373	Pollett River 25,187	Total 73,560
Drift nets (sea)					
Miramichi area	100	83	183	17	200
Shore nets (sea)					
Newfoundland	117	84	201	79	280
Bay Chaleur	4	4	8	....	8
Miramchi area	198	57	255	12	267
Nova Scotia (outer coast)	2	....	2	19	21
Bay of Fundy	....	....	....	5	5
Anglers (fresh water)					
Miramichi black salmon	42	19	61	....	61
Miramichi bright salmon	143	43	186	....	186
Total	606	290	896	132	1,028

salmon in the sea can be taken by a limited number of commercial fishermen using offshore drift-nets and estuarial trap nets; these commercial fisheries also open on June 5. This opening date for all fishing came into effect in 1954 on the recommendation of the Co-ordinating Committee on Atlantic Salmon. It is later than formerly, to allow more of the early-run stock to reach the spawning grounds.

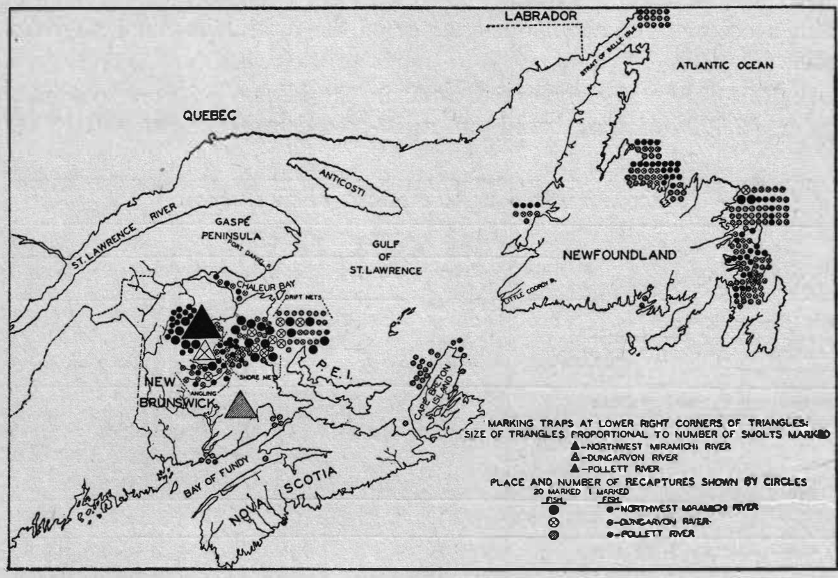

Figure 3. Recaptures by October 1953 of Atlantic salmon marked as smolts in 1951.

Throughout the Maritime region corresponding changes were made in the opening dates. In the Miramichi area both commercial and angling catches in 1954 were much higher than in the two preceding years. Since conditions for both angling and commercial fishing were not the best, owing to continuous high water and bad weather, it is believed that an abundant spawning stock was available in 1954.

In mid-summer the runs of both grilse and older salmon shrink to a very low level even when high water prevails, as in 1954. The fall run starts in September, ends in early November, and comprises fewer grilse but more big salmon than the early run. All commercial fishing is over by August 31, and in most branches the angling season closes on September 30.

One controversial topic has been "black salmon" angling for kelts which is permitted here from ice break-up to May 24. This sport is permitted because there is evidence that fewer than 10 per cent of kelts subsequently reappear in fisheries. A recent tagging experiment has shown that a large portion of this catch consists of late-run salmon that entered fresh water the previous autumn. Relatively few of the more valued early-run stock are taken by black salmon anglers (Table 3).

The black salmon fishery appears to be a worthwhile use for the late-run stock which is quite plentiful in the Miramichi area. Black salmon account for about one-quarter of the total Miramichi angling catch.

In 1954 the total commercial catch in the Miramichi area was just under 49,000 salmon, based on a 10-pound average weight. The

TABLE 3. RECOVERIES BY BLACK SALMON ANGLERS IN 1953, OF ATLANTIC SALMON TAGGED IN MIRAMICHI AREA IN 1952..

	Month of Tagging						
	June	July	Aug.	Sept.	Oct.	Nov.	Total
	Mostly Early-run			All True Late-run			
Total tags applied:	230	268	58	842	659	105	2,162
Tags remaining by winter 1952-53, after 1952 fishing season:	198	236	53	826	658	105	2,076
Recovered by black salmon anglers, 1953:							
April	7	6	2	92	97	16	220
May	1	3		22	40	7	73
June				2	4	1	7
July	1			1			2
Total	9	9	2	117	141	24	302
Percentage of available tags (monthly groups) taken by black salmon anglers:	5%	4%	4%	14%	22%	23%	

angling catch, including black salmon, totalled almost 34,000 fish. Official records of the angling catch show that of the fresh-run fish taken, more than half are grilse.

3. *Fish-eating birds.*—Natural history studies have shown that some fish-eating birds prey heavily on young salmon. Mergansers, especially *Mergus merganser,* and kingfishers, *Megaceryle alcyon,* are the important species. Intensive studies of these birds on salmon waters have shown that mergansers do by far the most damage. These fish-ducks are relatively abundant on many salmon streams. The quantitative effects of bird control were studied on a 10-mile section of the Pollett river. When there were 30 to 40 mergansers reared annually and twice as many seasonal invaders, smolt production from 16,000 planted fingerlings did not exceed 2,000 per year. Even when 15 times as many fingerlings were planted, annual production was scarcely doubled. The mean production from three plantings of 16,000, one of 48,000 and one of 249,000 fingerlings was 2,400 smolts per year. But when mergansers (and kingfishers) were controlled by semi-weekly patrols, average production from 4 plantings of about 250,000 fingerlings each, jumped to 19,000 smolts, *i.e.* by 8 times.

Systematic control of mergansers was then applied to the Northwest Miramichi, a well-known angling stream where the stock is supplied by natural spawning. Annual sampling of young salmon populations was carried out on this stream and the similar Dungarvon River. The latter is also a tributary of the Miramichi system, but one where there was no organized control of mergansers. Average population densities found in 1952 and 1953 combined, are illustrated in Figure 4, A. Both streams had similar runs of adult salmon, and similar populations of young salmon less than one year old. But two years after starting control on the Northwest, the large parr, which would become smolts nine moths after the sampling, had increased by more than four times. At this time the Dungarvon had only half as many large parr as the Northwest. The Dungarvon parr were subject to attack by mergansers for another nine months before migrating as smolts.

Experimental merganser control has been extended to cover the entire Miramichi system in New Brunswick and the St. Mary River in Nova Scotia, so that its effect on associated sport and commercial fisheries can be evaluated.

4. *Experiments with hatchery stock.*—The amount of hatchery stock which must be planted to get the best yield of smolts was studied on the Pollett River, after establishing bird control as a necessary

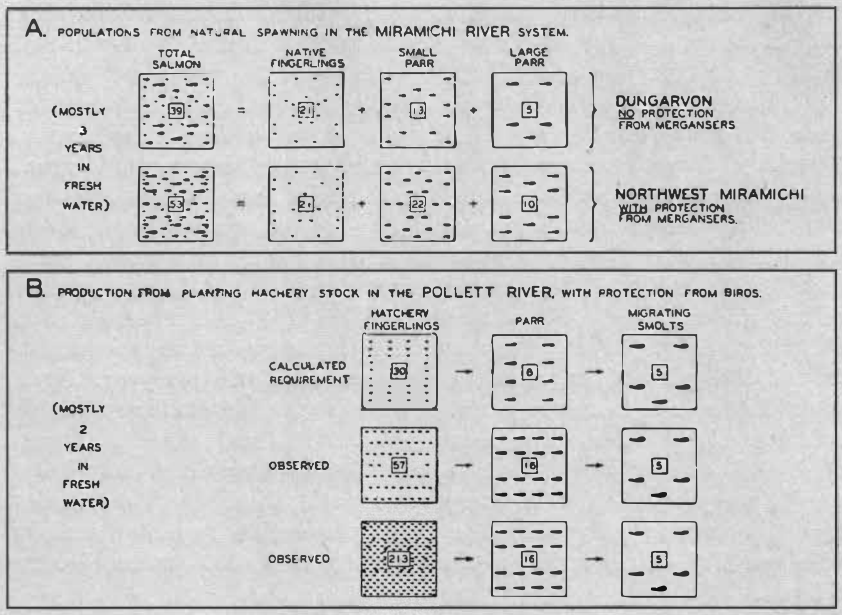

Figure 4. Average numbers of young salmon per 100 sq. yd. of stream bottom, in New Brunswick. With protection from mergansers the number of large parr in the Miramichi compared favourably with the calculated requirement for the Pollett.

adjunct of good production. Heavy populations of hatchery fingerlings were planted in order to learn the capacity of the experimental area to produce migrating smolts. Very light populations were used to determine best survival rates. The amount of hatchery stock required to get full yields of smolts has been calculated from the resulting figures. During the course of this work the parr were sampled systematically each year, using seining and electrofishing techniques. Thus it has been possible to determine figures not only for numbers of hatchery fingerlings, but also for numbers of larger parr required to give best smolt yields. These experiments involved hundreds of thousands of fingerlings and tens of thousands of parr and of smolts. Results have been reduced to numbers per 100 square yards of stream bottom (Figure 4, B). It now appears that the maximum smolt yield to be expected from the Pollett should be obtained by planting hatchery fingerlings at a rate of 30 per 100 square yards to give 8 yearling parr and 5 smolts per 100 square yards. While up to 16 parr have been obtained from heavier plantings these have still given only 5 smolts; this seems to be the maximum rate of smolt production.

Standardized measurements of population densities make comparisons between numbers in different streams relatively simple (see Figure 4, A and B). This is a step towards determining whether streams are supporting as many young salmon as they should.

5. *Number of adults to maintain stocks.*—How many spawners must rivers have to keep stocks at highest levels? This question must be answered if we are to take full advantage of our salmon resources. Measurements have been started on survival rates between various stages from eggs brought into rivers by mature fish, through fingerlings and parr to smolts leaving. Preliminary information from both the Pollett and the Miramichi suggests that relatively high smolt production can be obtained (using merganser control) if eggs are brought in at an approximate rate of 300 per 100 square yards of stream bottom. For a small to medium river (30 to 70 miles of salmon-rearing water) this requirement is roughly equivalent to 150 pounds of female salmon per mile of stream, whether they be 3-pound grilse or 10-pound older fish. Both the Northwest Miramichi and the Dungarvon receive females at a slightly lower rate and both appear to have good numbers of underyearlings (Figure 4, B). The poundage given for adult females allows for usual removal by angling and so on.

6. *Effects of DDT on salmon.*—In early June, 1954, about 1 million acres of budworm-infested woodland in New Brunswick were sprayed from aircraft, using one-half pound of DDT per acre. The area sprayed included most of the Northwest Miramichi and the upper reaches of the Dungarvon Rivers. Because studies of salmon in these streams have included yearly quantitative samplings of young, it is possible to gauge the effects of the spraying on salmon production.

Young salmon were held in cages in the streams during the spraying operations. About 80 per cent of those held within the sprayed area died within a week. Ten miles below the area of direct application only 16 per cent died within four weeks, and 15 miles below only 4 per cent. Dead and dying parr were observed throughout the sprayed area, and even to 15 miles below it, as long as four months after spraying. No such mortality had been observed in earlier years. Thus there were delayed effects in addition to the killing immediately after the DDT was spread.

The annual samplings were made in late August as usual, nearly three months after the DDT was applied. *No* fingerlings (fish of the year) were found in the sprayed sections and they were very scarce for at least 10 miles below these areas. Small parr (mostly yearlings) and large parr (mostly two-year olds) were reduced to *one-*

sixth and *two-thirds* respectively, of their levels of abundance in the two preceding years. Adult salmon entering the river during the summer did not appear to have been affected at all.

Aquatic stages of insects such as mayflies, stone-flies, caddis-flies and midges, which form the food of young salmon, were extensively killed. The possibility of replenishing these insects from outside sources is under consideration. Unless remedies can be found, it may take some years to reach former levels of production.

DISCUSSION

DISCUSSION LEADER SARGENT: Dr. Elson, in my State of Massachusetts, we have practically no salmon fishery. You mentioned, I believe, that the salmon was about fortieth of the commercial catch. In Massachusetts our total catch is forty salmon if it is a particularly good year. What I wondered about is how you obtain your sport catch statistics, whether they are accurate, and whether they are included in your total figures. I would assume that that is quite an important part.

DR. ELSON: Fortieth is the value. It's the sixth most important fish. But these values I gave you of a little over a million dollars a year do not include the sport fishery at all. It is just the commercial value. Sport fishery is worth just as much if not more, so we'll put a two million dollar tag on. I have the feeling myself that the one million dollar figure for sport fishery is probably a little on the low side. I am sure that any enthusiastic salmon angler would agree with me. In fact, he would say I was very pessimistic in setting the value so low, but the value given in the paper is the commercial fishery value.

DISCUSSION LEADER SARGENT: Do you have any method of ascertaining the total catch by sport fishermen?

DR. ELSON: Yes. Our fishery wardens attempt to get records from anglers as they travel the streams. We have contact with most of the salmon camps in the Miramichi area, for example, and get our records there. The department does get records from its inspectors of the salmon that are caught by sport means on the various rivers throughout all the Maritime Provinces.

MR. BRUCE LANE [New Brunswick]: I would like to ask Dr. Elson a question. It seems to me we are debating the issue here. We have a problem which Dr. Elson tells us is doing more damage than anything else to the fishery resource which is the spraying with wide-scale DDT, and we hear a lot about the damage the mergansers do, and as an afterthought, at the tail end of the paper, Dr. Elson made some few remarks of the devastating effect of this broad-scale spraying.

I would like him to elaborate a bit more on what he considers the over-all effect on New Brunswick salmon after five or six years of this continuous spraying of the headwaters of our waters.

DR. ELSON: I should say that the people who kill salmon are fairly close to the people who are trying to look after the interest of the salmon. We had a meeting with them early in February, and a very pleasant cooperative spirit was expressed on their part, and we were glad to see it.

Actually, this year at least, while there will be a little bit of spraying again, we have been shown their maps, as we were shown last year—and I expect we shall be shown in the future—and there will not be any spraying of waters this year which proves very vital to salmon protection. I think if one has to balance salmon against forestry for New Brunswick, or for any of our Maritime Provinces, I have a feeling that perhaps I would be on a losing side of the game. In other words, the forestry, at the present time anyway, is worth a lot of money.

The forestry people are spraying rather small amounts. They do not expect to spray the same area year after year. They expressed the opinion this year that it might be necessary to spray every third year in the same area, but they felt quite confident that it would not be necessary to spray more often than that. We still don't know.

The insects were very, very drastically reduced, and they were in some cases almost eliminated. Some of them were types which do not replace very rapidly; that is, they don't fly very great distances, and if they are eliminated from the headwaters down, perhaps three years won't be enough to get good populations of insects so that the salmon will have good eating again.

This year we are going to concentrate on that particular aspect of it. Of course, there is always the hope that the chemists may come up with some compound that won't be quite so bad. I believe the DDT disintegrates very rapidly in an alkaline area. Unfortunately, most of our salmon streams seem to have a low pH. Of course the ground has a low pH because the water reflects the ground. I think that perhaps if we had some other poison than DDT, the effects would be less drastic.

All I can say at the present time is that the forest people are in touch with the salmon people and are trying to plan their program with a minimum of harm to the salmon, and they have expressed a willingness to cooperate in any experiments. They have even suggested that they might attempt to set up some experiments to look further into this matter of forests versus salmon.

DISCUSSION LEADER SARGENT: Dr. Elson, referring to our state again, we do DDT spraying for gypsy moss. Before the program starts, they plot out very carefully all of the waters and streams and prevent the planes from flying over that path. I was wondering if this could not be done. It seems to me that with control you would be able to eliminate most of the DDT getting into the water. However, I realize that with a wind you would occasionally get a drifting of the spray, but I should think it would be possible to control some of that.

DR. ELSON: As a matter of fact, our air pilots like to fly but they don't like to get lost. They use streams as boundaries for their various courses. This often results in a stream being covered twice. However, I think perhaps this was expressed by the manager of Forest Protection when we were discussing the whole business. He said, "You know, we don't know where half of the DDT goes. We have put out spray cards which are supposed to pick up the droplets. They are scattered all through the area." They try to assess the distribution of the DDT, but they don't know where at least half of it goes.

I think this means that no matter what courses they flew, because of wind drifts and unkown factors, the course they flew would not make very much difference. If they are going to spray the forest areas which includes the salmon streams, they are going to get some DDT in the salmon streams. Perhaps some of it will only flow as you get rains washing it down from the soil later, but it is going to get there.

I would like to add one more thing. I know you have an Atlantic Salmon Fishery down in Massachusetts. A few years ago we had some salmon tagged in Nova Scotia, and about six months later one of them was returned from Massachusetts.

DISCUSSION LEADER SARGENT: I think I know the fish.

MR. HARRY W. WALTERS [St. Johns, Newfoundland]: I would like to ask Dr. Elson in making reference to standards of 150 pounds providing sufficient parr for a river, I wonder if he includes the whole watershed or just those areas suitable for propagation.

DR. ELSON: That 150 pounds are adult female salmon per mile, and that is just sort of a ready figure for somebody to think of. This is just a sort of popular-consumption ready figure which will form the basis of a calculation for all salmon. The average would be 150 pounds per mile. Obviously a narrow part is not going to take anything like that. As you get down to the lower end of a 70-mile stream, it is going to be quite wide and could stand a little bit more than that, but this figure is actually based on ten miles of a stream which ran at summer level about 75 feet wide.

MR. PIMLOTT [St. Johns, Newfoundland]: I appreciate Dr. Elson's remarks, but we hear many rumors coming out of New Brunswick that this control is by no means effective. If that is the case, the comparative levels of the two values may be much higher than it would be here. I wondered, too, about the effect of extensive pulp wood-cutting operations on watersheds such as one finds in Newfoundland. I wondered if the Fisheries Research Board is doing any work on the effect of large-scale pulp and paper operations on streams in Newfoundland.

Dr. Elson: So far as I know, there has been nothing like that going ahead.

I would be very glad to talk salmon with anybody who is interested, after the meeting, or any time during the day.

PROBLEMS OF PACIFIC SALMON MANAGEMENT

FERRIS NEAVE AND R. E. FOERSTER

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THE RESOURCES

Five species of Pacific salmon (genus *Oncorhynchus*) occur on the western coast of North America, namely: the spring or chinook salmon (*O. tshawytscha*); the coho or silver (*O. kisutch*); the sockeye, red or blueback (*O. nerka*); the pink or humpback (*O. gorbuscha*); and the chum (*O. keta*). All these species are commercially valuable in British Columbia, and the first two also provide an important sport fishery. The average annual commercial salmon catch for the province is about 180 million pounds, the marketed value in 1953 was approximately $48,000,000.

This catch has remained fairly constant for some 40 years—a long period by the standards of West Coast history. It might be inferred, therefore, that the fishery is in a satisfactorily stable condition and that the sustaining of current practices and regulations would permit production to continue on the same scale for an indefinite period. Actually, preservation of the salmon fisheries in the past has depended on periodic adjustments rather than on the maintenance of the *status quo* and there is every indication that future conservation will present increasing difficulties and will necessitate radical changes in the management of the resource.

In comparison with the early years of the fishery, volume of production is now maintained by increased utilization of pink salmon and chum salmon which were formerly not exploited as intensively as the other species, especially sockeye. Production of the latter species, in spite of recent and encouraging increases in the Fraser River watershed and well-sustained abundance in the Rivers Inlet area, remains well below the level obtaining in the early years of the century. In recent years, exploitation of all species has been heavy and fishing effort so thoroughly distributed on all parts of the British Columbia coast that there is little chance of increasing the annual catch significantly by the discovery of under-exploited stocks. Consideration is now being given to the possibilities of greater catches by extending fishing into more off-shore waters, out into High Seas areas. Since salmon must, for spawning, come into inshore waters and into rivers and streams, such off-shore operations can only result in capturing, earlier in the season, salmon that would be taken later in coastal waters (probably more economically), or in tapping stocks produced in and subsequently bound for other areas, Alaska or the Pacific States of the United States, where they would eventually normally be caught. Any such off-shore or High Seas salmon fishery, exploiting probably mixed stocks whose rivers of origin are unknown, would greatly complicate the conservation programs for the various river systems.

PRESSURES AND DANGERS

To offset heavy fishing pressures an increased rate of natural reproduction becomes necessary, in other words, a greater percentage production of young salmon to swell the stocks in the sea. This applies to the fresh-water part of the life cycle. Present developments are, however, tending to restrict or impair the effectiveness of those fresh-water habitats which are essential for reproduction success. The major detrimental factors are deforestation, hydro-electric dams and other impoundments, diversion of water for irrigation or domestic uses, and pollution. The adverse pressures brought to bear are not exerted equally on all watersheds or on all species of salmon but their effect is indeed very real.

Deforestation. The most widespread of the above-mentioned pressures is deforestation. By removal of the forest cover, the normal flow of streams is altered. Rapid changes in flow patterns occur. Flash floods, with damaging stream-bed scouring effect, develop, to be followed by low flow during dry weather. Severe erosion of stream banks and adjacent terrain occurs to cover up the salmon redds or impede the proper percolation of water through the stream-bed gravel areas. Stream water temperatures rise, especially during summer months. In most cases of suspected stream deterioration actual quantitative studies of the effects of removing forest cover are lacking and it should be recognized that a great many salmon streams show much fluctuation in run-off even under undisturbed conditions. Nevertheless, a correlation has been shown to exist in Oregon between the decline of coho runs and the progress of deforestation (McKernan, Johnson and Hodges, 1950). On the east coast of Vancouver Island pink salmon populations declined following extensive logging operations and in some instances improvement of the runs has coincided with development of second-growth forest cover. In the case of the Cowichan River, Vancouver Island, stream flow records show that the average summer discharge was higher in the period 1913-1919 than in the years 1940-1946. Much deforestation took place between these periods. A positive correlation was shown between the summer discharge and the abundance of cohos two years later (Neave, 1949). Furthermore, the virtual disappearance of a population of early-running chinook salmon which formerly entered this river in the spring and remained in the water-shed until early autumn spawning time is ascribed (*loc. cit.*) to increased sum mer temperatures caused in part by deforestation.

Man-made obstructions. The most serious of these are dams constructed to provide hydro-electric power or to store water for other purposes. These structures form barriers to salmon migration and in some cases divert water from existing river beds. The necessity of providing ladders or other devices to enable migrating fish to surmount dams is fully recognized, and large migrations take place annually over or around some of these of relatively moderate height, for example, the Bonneville Dam on the Columbia which is around 60 feet high. No really successful method has yet been demonstrated, however, for by-passing the much higher dams, which are now under construction or are projected, where large runs of salmon are involved.

The dangers encountered by young salmon migrating seaward are sometimes even greater. Many fish are killed by falling over high spillways or as they endeavor to make a hazardous passage through turbine intakes. Adequate screening of these danger spots has not hitherto proved feasible. The cumulative effect of a series of dams in a river system, each taking a toll of the passing fish, may seriously deplete a population at a stage long before the survivors present themselves to the attention of the fishery. A 10 per cent loss at each of several dam sites can quickly represent a severe drain on any stock.

The Columbia River is one river where the up-and-down-stream movement of salmon has been appreciably affected by power dams. The degree of loss is still debatable, perhaps, but it seems to be significant that of spring (chinook) salmon tagged off the west coast of Vancouver Island in recent years the proportion recovered in the Columbia amounted only to 15 per cent as compared wiht 65 per cent in similar taggings conducted 25 years ago (Milne, 1953).

In British Columbia, dams have not yet caused widespread interference with salmon runs. Future developments, however, pose drastic threats.

Species' differences. While all species are faced with difficulties and dangers in fresh water, the problems differ to some extent in kind and magnitude. Sockeyes and chinooks, which frequently undertake very long migrations in the larger river systems, are very vulnerable to large-scale development schemes. Sockeyes, in addition, require lakes with sufficient food supplies to permit development for a year or longer. Cohoes are widely dispersed and do not require lakes (though some do frequent lake areas for their year's residence (Foerster and Ricker, 1953) but since the young fish live for a full year in streams they are exposed to the whole range of seasonal fluctuations in discharge, temperature and food supply. Pinks and chums are basically dependent on fresh water only for incubation of the eggs and alevins. By their early passage to sea the young fish avoid the limitations imposed by restricted food supplies and unfavourable summer conditions. Moreover they inhabit principally small streams or the lower reaches of larger rivers, thus escaping some of the man-made perils of a long migration. These spawning grounds are, however, frequently very sensitive to changes in water flow; even during the short journey to salt water the fry are subjected to heavy mortality by predators.

PRESENT CONSERVATION PRACTICE

The essential basis of past and present conservation measures is to ensure that a substantial proportion of the maturing fish is allowed to get through the coastal and river estuary fishing areas and proceed upstream. This "spawning escapement" is provided by prohibiting fishing at certain times and by restricting the kinds and sizes of gear, as well as the manner of their employment. With certain exceptions, fishing in fresh water or near the mouths of streams, where fish tend to congregate, is prohibited.

The problems involved in endeavouring to provide an adequate spawning escapement are two-fold. In the first place, the difficulty of assessing an incoming population and of effecting a given division between catch and escapement is very real. Secondly, there is a lack of fundamental information as to the size of spawning escapements which can be expected to produce the most beneficial results. Hitherto there has been a tendency to think of escapement in terms of a percentage of the total mature population. In the case of the Fraser River sockeye a spawning population representing around 20 per cent of the run has been considered sufficient to maintain existing levels of abundance. For other areas, notable Alaska under the White Act, and for other species there has been a tendency to regard 50 per cent as a standard to be aimed at. In theory it would seem that escapement should approximate to a fixed number rather than to a percentage of the run, this fixed number being related to the quality and capacity of the particular watershed for spawning and for rearing the young. In most instances, present knowledge is insufficient to set numerical objectives; yet, in spite of imperfections due to inadequate information, the efforts which have been exerted to provide sizable escapements have undoubtedly been the main factor in keeping salmon production at a relatively high level up to the present time.

Other conservation measures in recent years have been directed principally toward maintaining and increasing the accessibility of spawning grounds to the adult fish. Thus, as many as possible of the salmon that have escaped the fishery may safely get to suitable spawning areas. Providing fishways to by-pass natural or artificial obstructions is a standard practice (Talbot, 1950; Anon., 1953a, 1954a, b, c). Removal of log jams (a common feature of many salmon streams) is undertaken when necessary, but the advantages of protective cover and the effectiveness of sunken logs in checking erosion make general ''clearing out'' of streams undesirable. A spectacular recent stream clearance operation was the removal, at a cost of some $600,000, of a natural rockfall which in 1951 obstructed the very important salmon runs of the Babine River in a remote part of the Skeena watershed (Anon., 1953b; Godfrey, Hourston, Stokes and Withler, 1954).

Under natural conditions, the downstream migration of young salmon receives, and perhaps requires, less attention than the spawning run. At times, however, numbers of migrants or feeding young become trapped in pools by dropping stream water levels. In some years the freeing of such fish, by netting out or constructing exit ditches, results in potentially important savings, notably in certain streams on the south-east coast of Vancouver Island. In at least one year the number of chum and coho fry released in this way was estimated at over a million in the Cowichan River alone (Neave, 1949).

PRESENT RESEARCH

Heretofore salmon fishing has been essentially the gathering of an uncultivated crop of fish whose abundance has depended on great areas of land and water being left in their primitive condition. Whether we like it or not, this state is being changed and will be changed more. A management policy which merely tries to perpetuate primitive conditions will not succeed, except perhaps in certain especially designated and limited areas.

Broadly speaking, therefore, present management aims must be four-fold: (1) to regulate the fishery in such a manner that the spawning escapements are capable of maintaining a high level of abundance, (2) to develop ways of minimizing the deleterious effects of human activities on freshwater habitats, (3) to increase the productivity of spawning and nursery waters which remain accessible to salmon and (4) to open up and develop new ones for reproduction. In broadly based management or research programs no strict separation of these four categories can or should be expected.

Regulation. Regulation of the fishery to provide satisfactory spawning escapements is an internal matter in that it can be effected by the industry itself and the appropriate governmental agencies. An excellent system of recording catch by species, time, and area, has recently been put into operation. Sampling of the commercial landings to determine the constituent age-classes of salmon makes it possible to designate the productiveness of the previous years' spawnings. It has to be recognized that as fishing operations extend farther offshore and out into the high seas it is going to be much more difficult to relate the catches to individual river systems or coastal areas. Therefore the establishment of catch to escapement ratios and regulation of catch to assure adequate escapement will become much more complicated and difficult. The difficult problem of estimating spawning escapements is tackled on a comprehensive scale, although results must be regarded as largely qualitative. In many cases the extent of country involved and the remoteness of many of the spawning areas makes patrol and examination costly and difficult.

Production in fresh water. The best-laid schemes for regulation of the fishery will come to naught if the fish which the industry refrains from catching are unable to reproduce. Reference has already been made to the problems presented by natural and artificial obstructions and the efforts to overcome them. The deleterious effects of log jams also are being alleviated. In addition, however, every effort must be made to assure that natural production be fully effective, *i.e.*, that egg deposition be as complete and as successful as possible and that the survival of the young fish up to the time of passage into the sea be as high as possible.

For many years, in British Columbia as elsewhere, it was thought that the answer to producing more salmon lay in hatcheries. Widespread use was made of them, particularly for the propagation of sockeyes. A 12 year investigation at Cultus Lake, B. C., showed that this kind of artificial propagation was not improving significantly on the results of natural reproduction (Foerster, 1938). The essential features of the view resulting from this investigation are that where conditions for natural propagation remain favorable (1) little or no advantage is gained by the substitution of hatchery procedures terminating at or before the fry stage of the life-cycle, (2) the protection and care of the young fish to a later and less vulnerable stage, in the numbers required to support the commercial fishery, would be unreasonably costly.

This verdict would not necessarily apply to situations in which natural propagation is restricted or impaired, and it might also be altered by changes in the value placed on the resource. Under present conditions, however, hatcheries do not provide a blanket beneath which salmon management can take cover. As a routine practice the operation of salmon hatcheries has been discontinued in British Columbia since 1937.

One purpose of two research projects of the Fisheries Research Board of Canada has been to ascertain, for sockeye and for pink and chum salmon, the normal range of survival during the various stages of the life-cycle in fresh water and the average overall percentage of production of seaward migrants, whether fry (in the case of pinks and chums) or one or two-year smolts (in the case of sockeye), from a known spawning or egg deposition. At the same time observations are made as to the factors which contribute to the non-survival or mortality. The production data then act as a basis for evaluating any improvement measures that might be taken or for assessing the trend of freshwater production as environmental conditions change. The observations on possible limiting factors for survival serve to reveal which factors are most important and whether they can be remedied or not.

For pink and chum salmon, the over-all fry production, as summarized by Neave (1953) and Hunter (unpublished), has been found to be:

Species	Locality	Number of years	Geometric mean Survival(%)	Range(%)
Pink Salmon	McClinton Creek	6	13.15	6.9-23.8
Pink salmon	Morrison Creek	2	5.61	4.7- 6.7
Pink salmon	Hooknose Creek	7	8.13	0.9-16.4
Pink salmon	Sashin Creek, Alaska	6	1.16	0.2- 4.4
Chum salmon	Nile Creek	5	0.30	0.08-6.03
Chum salmon	Hooknose Creek	7	8.69	1.0-19.4

Breakdowns of total freshwater mortality encounter two main difficulties (Neave, 1953). The first lies in the absence of quantitative data indicating the variation which undoubtedly exists in different streams and different years. The second is that many of the factors are so interrelated that assignment of losses becomes an arbitrary matter. For example, the losses among pre-spawning fish or among the young may be assigned largely to a biological factor, predation. The extent of predation may, however, depend greatly on a non-biological factor, the prevailing water level of the stream. On the other hand, to report such factors as stream-bed scouring or silting as the chief causes of mortality among eggs and alevins may under-emphasize the part played by a biological factor, the size of the spawning run, which may determine the proportion of fish forced to select less desirable spawning sites. On the whole, however, it has been established that (a) variations in water levels and water flow in streams at spawning time and during fry emergence and seaward migration and (b) predation on fry are important factors. Some control measures may be practicable and effective.

It has been shown experimentally (Wickett, 1952) that more young pink and chum salmon can be produced from a given area by concentrating eggs in channels where water flow is controlled, where stream-bed gravel conditions are uniformly favorable, and where predators can to some extent be excluded. In theory, the fresh-water requirements for a population of either species equivalent to the whole existing British Columbia stocks could be met by incubation areas totalling only one square mile, 640 acres. The difficulties of concentrating eggs and ensuring their favorable distribution in the selected places—the problems of providing the right conditions of substratum and water flow—need not blind us to the fact that much may be done beyond wringing our hands at the encroachments of industry.

A small-scale example of the kind of development that can be expected to take place is provided by Wahleach Creek, a salmon-bearing tributary of the Fraser River. As a result of hydro-electric development the flow of this stream has been reduced to a point at which natural propagation might be expected to suffer. In conformity with requests by the Department of Fisheries the B. C. Electric Company constructed an artificial channel, with a gravel bottom and a head-gate for control of water supply (Anon., 1954d). Under the more constant water conditions thus created, the stream may be capable of maintaining an even greater output of young pink and chum salmon than formerly.

As for loss of pink and chum salmon fry through predation during their short-term migration seaward it has been found that large populations suffer relatively less than small ones (Neave, 1953). In the limited time available, the local predators cannot use more than a certain maximum number of fry and large runs therefore take a lower percentage loss than small runs. This is an additional reason for trying to obtain the maximum fry hatch which a stream can produce, thus "swamping" the predators and reducing their influence on salmon fry populations.

For sockeye salmon, the one- or two-year residence in a lake provides that (1) the young fish go to sea at a much larger size and therefore may suffer appreciably less ocean mortality; (2) the losses from fry to smolt stages may be equally as great in fresh water as they would be for pinks and chums in the sea but nevertheless they presumably may be more readily assessed and perhaps remedied and controlled; and (3) the size of population will be limited by the productive capacity of the lake which each race inhabits.

Studies of natural production of young have been made in four areas which differ quite markedly in climate, size and geographic location (Cultus Lake, a tributary of the lower Fraser River (Ricker, 1937); Port John, in the Central coastal area (Robertson, 1954); Lakelse Lake, a tributary of the lower Skeena River (Brett and Pritchard, 1946); and Babine Lake, a tributary of the upper Skeena (Withler, McConnell and McMahon, 1949)). From test streams tributary to the lakes, percentage fry production from known egg depositions has been determined; from counts of estimations of seaward-migrating smolts, the production of smolts from known egg depositions has been computed. These are as follows:

Locality	Fry survival (per cent	Smolt production (percent)
Lakelse	13.7, 9.3, 13.6, 12.2, 10.1(1)	1.1, 0.4, 1.5, (8.5)(2)
Port John	25.2, 1.8, 5.1, 8.9, 5.0, 13.4(3)	0.5, 3.0, 5.0, 3.0(3)
Babine	12.0, 19.0(4)	0.5, 0.8, 1.5, 0.7(5)
Cultus		1.13, 1.05, 3.16(6)
Geometric mean	9.7	1.42

(1) MacDonald, J. G., Unpublished. (2) Brett and McConnell, 1950; Foerster, 1952. (3) Hunter, 1949, and unpublished. (4) Withler, F. C., unpublished. (5) Withler, 1953, and unpublished. (6) Foerster, 1938.

Sockeye fry survival ranged from 1.8 per cent to 25.2 per cent, the geometric mean being 9.7 per cent. This is similar to the percentage production of pink salmon fry obtained in British Columbia test streams and is slightly greater than that for chum salmon at Hooknose Creek. For sockeye smolts, however, production was found to be 1.42 per cent of eggs deposited. It becomes apparent, therefore, that for sockeye, the losses occurring in spawning, incubation, fry emergence and passage from hatching area to lake amount to approximately 90 per cent of eggs available for deposition (*i.e.*, contained in females passing the counting weirs) while the losses in the nursery lakes amount to about 85 per cent of the fry entering them.

A rough breakdown of the mortalities occurring during the various phases of the life-cycle of sockeye, from the time of up-stream spawning escapement to the seaward migration of smolts, and computed on the basis of 4,000 eggs per female, might be made as:

Losses during upstream ascent—10 per cent or 400 eggs. 3600 eggs remaining.
Losses during spawning and incubation in redds—50 per cent or 1800 eggs. 1800 alevins produced.
Losses during fry hatch and migration to lake—75 per cent or 1350 alevins. 450 fry produced.
Losses in lake—85 per cent or 383. 67 smolts produced.

Such breakdown is admittedly very crude and will vary appreciably from locality to locality, from season to season, and according to size and density of the spawning population. It serves to indicate, however, where the heavy losses occur and suggests where effort might most effectively be applied to increase production. It tends to show, also, where and how changes in environment and water conditions caused by man-made developments may affect sockeye production. If, as has been indicated by one series of findings (Foerster, 1937), the return from the ocean be 10 per cent of the smolts entering it, the 67 smolts produced from the spawning of one pair of sockeye (4000 eggs) will return 6.7 adults, a production ratio of 6.7:2, thus producing a surplus or dividend of over 300 per cent. Out of each seven sockeye returning from the ocean, therefore, approximately five can be removed by the fishery and two allowed to proceed to the spawning grounds.

Reference has already been made to the probable beneficial effect of stream water control in increasing production of pink and chum fry. The same will apply to sockeye, though perhaps not as generally, since in many areas the sockeye spawn in gravel reaches below lakes where water flow conditions are relatively stable. Predation on fry in the streams and on fingerlings in the lakes is a serious limiting factor. Reduction of the number of predators in one area, Cultus

Lake, was attempted (Foerster and Ricker, 1941) and resulted in a notable increase in sockeye smolt production. The feasibility of applying this kind of protection to important stocks of salmon merits thorough exploration. It involves a long-term study of interspecific relationships to determine how readily and at what cost populations of predator species can be kept under control.

Evidence available suggests that the production of sockeye is limited by the capacity of lakes to support large populations of smolts (Foerster, 1944). Smolt size, inversely related to the density of lake population, appears to influence directly the survival in the ocean (Foerster, 1954). Therefore, consideration is being given to the feasibility of increasing the productive capacity of lakes by fertilizing with essential mineral and organic fertilizers to stimulate greater crops of food. This represents another means of increasing sockeye production and its value should be determined.

The possibilities of favorable action are more encouraging because some lakes have produced and are producing sockeye crops far in excess of the average. Lakes of the Rivers Inlet and Smith Inlet watersheds of British Columbia today produce average sockeye catches of around 40,000 fish per square mile of lake area which is 15 times the average production of the Skeena or Fraser systems, on a lake-area basis. Even greater production occurred, apparently, in the Karluk Lake, Alaska, area in the early years of the century, the average catch amounting to about 150,000 sockeye per square mile of lake area.

CONCLUSION

The chief objective of salmon management is obviously the production of as great a harvestable surplus as possible. This means, firstly, that full use must be made of the capacity of all available salmon-producing areas and that only sufficient fish be saved from the commercial fishery to re-seed the spawning beds fully and to fill the lakes (sockeye) and streams (coho) to full productive capacity. Data on these points are being obtained, as already indicated. It means, secondly, that wherever possible new areas should be opened up to salmon production. This is being done by removing obstructions or by building fishways around them. It means, thirdly, that steps should be taken to increase production in presently-utilized areas, by improving stream conditions, by reducing predators, by raising the potential productive capacity through fertilization of lakes or by providing artificial spawning areas. These possibilities are all being investigated. It means, fourthly, that the advance of civilization and the inroads of industry through removal of forest cover around streams, construction of hydro-electric dams, increase in pollutants poured into rivers and coastal areas, diversion of water for domestic or irrigation purposes and the like, must be carefully watched, the damaging effects remedied where possible and the general overall effect on production assessed so that in the balance sheet of salmon production the unavoidable losses may be computed and counterbalanced by greater productive efficiency elsewhere.

It has to be realized that in the management of any biological resource there are to be expected wide fluctuations in production, caused by changes in environment, of climate and by the influence of changes in populations of other fish inhabitating the same waters. There will be periods of high natural production, periods of low. All that man can do is work with nature and give it a fair chance.

Where, as in the case of the Pacific salmon, heavy and what-might-be-termed mass mortalities occur (*i.e.*, freshwater losses of 90 per cent or more and subsequent ocean mortalities of 90 per cent or greater) unusually favorable environmental conditions for survival may very appreciably, even explosively, increase the numbers of adults returning from the ocean in the cycle years. For example, a 50 per cent reduction of the sockeye losses at each stage between spawning and fry migration to the lake would increase smolt production by almost 400 per cent. If, furthermore, the lake losses were reduced by one-half, *i.e.*, to 42.5 per cent, the smolt output would rise handsomely and allow a very appreciable increase in both catch and escapement. By the same token, decidedly adverse environmental conditions for production may quickly reduce production of young and seriously limit the stocks returning from the ocean.

There are indications in the history of some of the salmon fisheries that extraordinary situations of this kind have occurred but the evidence is not too clear. It is apparent that greater attention must be paid to conditions existing in freshwater for survival and production of young, to improve the conditions where possible and at least to ensure that they are not adversely affected by man-made changes in the freshwater environment. Relatively small changes in percent survival of young can have a marked affect on the size of returning adult salmon runs.

Hopes for continued abundance of salmon to support a flourishing industry and to maintain an important food resource seem to hinge on our ability to develop a sort of semi-cultivation of salmon whereby inroads into primitive habitats are balanced by well-planned improvements in other places. Also there must be a realization on the part of industrial developments which infringe upon the salmon resource that the requirements of the salmon must be protected and safeguarded or else checked off as a charge against the value of the adversely-affecting industry. By careful planning and proper manipulation there is no reason why salmon production cannot be maintained alongside other industries. The over-all value to the country over many years is the salient consideration. For some of the problems now facing the biologists and administrators there was no advance warning and scientific information to solve them was lacking. In time it will be obtained. Meanwhile the resource should be given protection lest it be irretrievably lost. Its present value and significance does not necessarily indicate its future worth.

Economics cannot be disregarded and the issue will be decided in part by how badly and by how many the continuance of the resource is desired. Implementation of large-scale plans will cost money and be subject to continual appraisal of the balance between expenditure and anticipated benefit. The outcome will also be determined, however, by the imagination, knowledge and wisdom of those who are entrusted with the trials and privileges of research and management. On these perhaps stringent terms we can have salmon for long years to come.

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DISCUSSION

DISCUSSION LEADER SARGENT: Dr. Foerster, those of us who come from the East, particularly the Northeast, are pleased to hear that you are not at present faced with a serious pollution problem. That is, we feel, one of the principal causes of our dilemma here in the Northeast.

You mentioned the matter of predators with respect to salmon, and I recall with respect to Atlantic salmon, that the question of predators also was considered important, and in that case it was mergansers. I have two questions. What are considered the principal predators of the Pacific salmon? The other one is a puzzling one to me, and that is the matter of any kind of predator control. I always feel that the predators were always there even when the fishery was in the virgin state, and unless the predators have grown for some reason that we do not understand, I wonder how much monkeying around we should do with predators.

DR. FOERSTER: You have brought up a very interesting subject, Mr. Sargent, and I wish I had time to argue it out rather fully.

With reference to your first question, fortunately our predators are essentially fish. Many of them are squaw-fish, which is not a commercial fish in British Columbia and is discarded even by anglers. We know that trout are very active predators, and we are continually running up against the opposition of sport fisherman to the removal of trout. I feel, however, that if the sport fishermen could only keep the trout populations under control and could keep them down to a sufficiently reasonable level, we could still have trout and salmon as well.

That is a problem that is facing us at the present time. We are trying to see if we can't arrange some compromise whereby we can let the sport fishermen have their trout and also allow a good production of salman. In one test where sockeye are being studied, we have found that of the trout population, the fishermen are actually taking about 10 per cent of the total population of trout. Is it necessary to have such a backlog of trout to meet the needs of the trout fishermen?

With reference to the other question, these coarse-fish predators have always been there. We take the attitude that they have always been there and we still have quite adequate salmon populations, but the salmon populations are being cut down by at least 50 per cent if not greater. The coarse fish have been allowed to boom up; whereas the salmon are under pressure continually in trying to maintain themselves.

I perhaps am not as concerned about attempting to remove predators. I don't see why we can't keep them under control. But, if by a minimum of effort we can cut down the numbers of predaceous fish, I am sure that the increased production of salmon would more than make up for the expenditure involved. That, I think, is what Dr. Neave means by semi-cultivation; just a small effort at removing predators might have a very good effect in producing more salmon.

MR. B. UREN [Ontario]: Have you had any problems with DDT spraying, and if so, has it had any effect on the population?

DR. FOERSTER: To my knowledge, heavy DDT spraying was only done in one area of the British Columbia coast a few years ago. We knew nothing about it until we saw it in the papers, and immediately, of course, we began to worry about the effect on the salmon.

While we were not able to make adequate tests, nevertheless we sent a chap over to see just what effect it might have had on the young fish. The evidence we got at the time was that the spraying was too extensive, and perhaps the salmon streams involved were too few, to give us anything definite. I think the department involved at that time in the spraying realized its mistake. We were told the next time they try it, we will be advised so that we can make tests of the effect.

PLANNING ANADROMOUS FISH PROTECTION FOR PROPOSED DAMS[1]

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INTRODUCTION

Recent development of the water resources of British Columbia, a keynote in the province's rapid growth, has prompted extensive investigation of the problems of protection of anadromous fish. Anadromous fish populations are affected by high dams in many ways. A dam may divert water from its natural course so that adult fish no longer utilize the former stream system. Where this is not a problem, the dam may be an insurmountable obstacle to adults migrating upstream. If devices can be constructed that will carry the adults over the dam, these devices may only serve the purpose of releasing the adults in waters made unsuitable for the deposition of eggs, or as habitat for young fish. Provided these problems do not arise or can be solved there is still the possibility (1) the young fish will not migrate to sea as they should (2) if they do migrate they may be killed

[1]This paper was read by Dr. W. M. Sprules, Department of Fisheries, Ottawa, Ontario.

in the violence associated with the ride over the spillway of the dam or through its underwater orifices. All the subtle biological and engineering variations of these problems comprise the general concern of the fisheries officials charged with planning anadromous fish protection for high dams.

To a certain extent we in British Columbia can draw on the experience of the Pacific Coast States to design effective facilities and to avoid costly errors. Our first approach was largely directed to canvassing of American agencies who had such experience. But one of the first findings of the survey of protection facilities was the appreciation that variations in details of design and function of the dams and variability in the character of the runs of fishes to the streams, combined to give each project singular problems of fish protection. For instance, if a dam is designed for utilizing stream flow for power development, then every drop of water that is diverted for fish protection may represent a loss of potential horsepower. Provision of minimum flow may thus be a major problem. A dam built to provide a constant head for a domestic water supply and for which water storage is not a main function may pose no problem of maintaining minimum flow in downstream areas. However, it may incorporate features of spillway design that cause a high mortality of downstream migrants. Such examples could be multiplied. Thus, there is no simple series of solutions for protecting anadromous fish when high dams are built. Each particular project calls for detailed study of the local conditions both as they affect the character of the runs of fishes, and the latitude for engineering design.

The Cleveland Dam, constructed on the Capilano River for domestic water storage, posed a typical protection problem, for which the suggested solution was highly influenced by the singular biological and engineering problems that were encountered. This point will be enlarged in the remainder of this paper.

NATURE OF THE PROJECT

The Capilano River drains a small watershed, north of the city of Vancouver. The flow in the river may fluctuate from as low as 10 c.f.s. in the dry summer months of July and August, to peaks ranging from 7,000 to 27,000 c.f.s. in the wet months of October and November, and the spring run off months of April and May. (There may be a minor seasonal low in December and January).

The river provides one of the major sources of domestic water for the Greater Vancouver area. The Cleveland dam, completed late in 1954, was constructed to provide water storage in the recurring dry seasons in anticipation of greater future demands on the water supply system. The dam is a concrete gravity-type structure over 300 feet high, located in a narrow canyon roughly three miles from the mouth of the river. The reservoir formed by the dam has an area of roughly 700 acres and is three and one quarter miles long.

The river supports populations of six anadromous species of salmonoids averaging approximately as follows: pink salmon (*Oncorhynchus gorbuscha*), 10,000 adults in alternate years; chum salmon (*O. keta*)—7,000 adults yearly; coho salmon (*O. kisutch*)—7,000 adults yearly; spring salmon (*O. tshawytscha*), occasional adults; steelhead trout (*Salmo gairdneri gairdneri*), 1,000 adults yearly; cutthroat trout (*Salmo clarki clarki*), numbers unknown. Features of the life history of these species in the Capilano as they relate to the problems posed by the construction of the dam can be divided into 4 chief categories (1) upstream migration of adults, (2) spawning grounds, (3) habitat for young fish, (4) downstream migrants.

PROBLEMS IN PROTECTION

Upstream Migration of Adults—Biological Problems:—The average flow of the Capilano River considerably exceeds the average demand on the domestic water supply system. All of the excess flow will pass over the dam into the natural stream course. The pink and chum salmon and cutthroat trout spawn in areas below the dam and present no problem in upstream migration except as the timing of their runs may coincide with seasonal water shortage. Adult steelhead and coho spawn naturally in stream areas above the dam and their entire runs need artificial assistance in migration past the structure. The problem of providing this assistance involves the designing and locating of facilities that will quickly and effectively attract the migrants. Prolonged delays can be lethal or may result in less efficient spawning. The coho salmon generally begin to ascend the Capilano during the summer period when minimum water flows and maximum water temperatures are expected. These conditions aggravate the effects of delay. Moreover, it might be anticipated that the temperature of the surface waters of the reservoir behind the dam might be even higher than those which occurred naturally in the stream. Accordingly the facilities for upstream migration were designed to assure that there would be a minimum of delay for the adult fish. The possibility was also considered that at times of low flow and high temperature, water might be discharged from an orifice 100 feet below the crest of the dam to provide cooler transportation water for the fish, if it should prove necessary.

The simplest type of artificial transport of the fish would move them from the foot of the dam to the upstream face of the dam. However, the lake reservoir presents a type of environment quite different to the stream conditions to which the fish are adjusted. In the absence of stream conditions of temperature, flow, turbulence and objects for visual orientation the effectiveness of the fishes responses, which result in upstream movement, would undoubtedly be impaired. Knowledge of conditions that would pertain in the reservoir was lacking and for this reason it was decided that the coho and steelhead adults should be transported above the reservoir area.

Engineering Problems. The choice of methods or devices for passing adult fish upstream over a dam depends on several factors, the most important of which are height of the dam, number and species of fish involved, and initial and maintenance costs of the device.

An economical and satisfactory method of passing a large run of fish over a high dam has not yet been devised. Fishways have proved the most successful method of passing fish over dams up to 100 feet in height. The costs of fishways do not increase directly with the height of the dam, but rather the cost increase is disproportionate to the height of a dam. Fishways are not therefore practical as a means of transporting small runs of fish over high dams. In addition, the qualitative and quantitative effects of delay that a long fishway might have on migrants is not known. The answer to this problem might limit the height of dam where a fishway could be used, even though the expense could be justified.

The fish-lift, fish-lock and trucking methods have been used successfully to pass small runs of fish over high dams. The initial cost of trucking or fish-lift facilities is almost independent of the height of the dam. The use of these facilities would normally be limited to a dam where a fish-lock or fishway is not economical or feasible. The fish-lift would usually be given preference to the trucking operation because of lower maintenance costs. Fish-locks have not been developed to a point where large runs could be handled successfully, but this method offers more hope for solution of the problem at intermediate and high dams than the other methods.

In the case of the Cleveland dam, a fishway was ruled out because the estimated cost was over a million dollars, or several times that of other possible devices. It was therefore necessary to design facilities which would pass relatively small runs of fish over the dam without handling, delaying or injuring the fish and which would not be exorbitant in cost. The trucking facilities met these requirements and were also most adaptable to a hatchery operation which may prove necessary (See discussion).

The facilities now constructed include a rack, fishway, holding pool, brailing pool, hopper pool, hopper and tank truck. Coho salmon and steelhead trout migrating up the Capilano River are stopped by a rack several hundred yards below the dam and diverted into a fishway. The choice of location was based on several considerations including the limited areas available for sites, the effects of turbulence at the tailrace of the dam, and the limited working space available on the river banks. The rack is built of steel bars having a clear opening of 1¼ inches. It is 10 feet high and designed to operate at all but peak flows (which latter, in any case probably deter upstream movement of fish). The rack was angled downstream away from the fishway entrance so that fish would move in an upstream and lateral direction along the face of the rack towards the fishway entrance.

The weir-type fishway comprises 19 pools, each 10 feet long and 5 feet wide and separated by drops of 1 foot each, leading fish to a holding pool. The fishway requires about 15 c.f.s. for operation. The width and length of pools was determined by the number of fish expected at peak migration periods. A weir-type fishway was chosen because its water requirements were lower than other types and the headwater at the holding pool was at a controlled level. The first baffle at the fishway entrance was made adjustable by a telescoping arrangement so that attraction water supplied through a floor diffuser from behind maintained an attraction velocity at any operating water level. Fish are prevented from dropping back down the fishway by a finger trap positioned close to the surface at the fishway exit.

The holding pool is 20 feet by 20 feet by 4 feet deep and is designed to hold a maximum of 400 fish (using the relationship: three gallons of water per pound of fish). To prevent the migrants from fighting the inflow to the pool, water enters through a floor diffuser at a maximum velocity of only one foot per second.

A V-type funnel joins the holding pool to the brailing pool. When fish are to be loaded for trucking, the gate at the head of the funnel is opened and the inflow is switched to the brailing pool to attract the fish. The gate at the head of the funnel is closed when a load of fish has entered the brailing pool. The brail or false floor (hinged at the hopper side) is hoisted and fish are forced into the 5-foot square hopper which is then lifted over the tank truck (filled with water) by means of an overhead electric hoist. The bottom of the hopper is circular and is sealed to the circular hatch on the top of the tank. A plunger type valve opens permitting water and fish to pass from the hopper into the tank as water is bled from the tank. Fish are transported to a dumping site a short distance above the head of the reservoir and released through a hinged flap gate.

The tank truck with a 600 gallon capacity, was designed to carry approximately 80 fish averaging 8 to 10 pounds each. It has two circulating pumps each of 120 gallons per minute capacity.

The trucking operation was first used in 1954 and during the first six months of operation 3,000 coho salmon and several steelhead trout were transported and released above the dam.

Spawning grounds—Biological problems.—In addition to its effect on upstream migration of fish, the Cleveland Dam will have some effect on spawning grounds. The increased depth and reduced velocity resulting from inundation will render spawning grounds located in the reservoir completely useless. Below the dam, spawning grounds will be affected by changes in flow pattern and changes in physical and chemical characteristics of the water resulting from its impoundment.

In the Capilano system, the major spawning areas of the coho and steelhead are located some eight miles above the dam site. These are unaffected by the project. Limited spawning of both species in parts of the stream that are now in the reservoir area will no longer be possible.

The effect on the spawning grounds of pink and chum salmon located below the dam will depend on how much the natural stream flow is reduced at the time of spawning and during the incubation period. Initially it is not expected that the flow pattern will be much changed from that at present. During periods of maximum water demand, arrangements are being made for provision of at least a minimum flow in the river below the dam. Assessment of effects of changes in physical and chemical characteristics of the water will not be possible until the reservoir has filled and studies are carried out both in the impounded area and on the spawning grounds below. It is anticipated that water temperature will be the major physical change, and if it is found to be critical then sub-surface water releases from the reservoir would be required.

The dam may eventually be responsible for the loss of spawning grounds in downstream areas. In the continuous processes of erosion, gravel displaced from spawning areas during flood flows is ultimately replaced by gravel eroded from upstream areas. Elimination of this upstream supply through settling action of the reservoir could result in loss of all but the coarsest gravels in areas below the dam, rendering the stream bed useless for spawning.

Engineering problems.—Release of a minimum flow of water for fisheries purposes below a dam is usually made directly from a reservoir. Calculation of the necessary minimum flow can be based on the previous natural low flows and consideration of the numbers of fish to be accommodated in relation to hydraulic conditions in the stream. Arrangements are being made for release of a minimum flow but under conditions of greater demand on the domestic water supply system it may be difficult to maintain minimum flow in all years. In this eventuality some consideration might be given to construction of a controlled flow artificial spawning channel (This would also solve problems of loss of suitable gravels).

Habitat for Young Fish. Although the fresh-water habitat requirements of young anadromous fish of the Pacific slopes vary greatly between species, the species may be divided into three groups.

(a) Pink and chum salmon migrate to sea very soon after emerging from gravel redds. These species require a reasonably constant flow of clean water from the time of egg deposition in the fall through the incubation period and up to the time of seaward migration in the spring. These species have no food requirements in fresh water, but large fluctuations of stream flow may either destroy redds by erosion or, at the lower extreme, expose redds to desiccation or freezing. All species have these primary requirements for successful hatching.

(b) Coho salmon remain in fresh water for a year before migrating to sea. Coho remain in streams and subsist mainly on aquatic insect larvae. Because of their dependence on streams for the first year of growth, coho are more vulnerable than other Pacific salmon to unfavorable stream flow conditions. Minimum summer stream flows have been shown to be directly correlated with the annual abundance of these species. Reduced stream flows restrict the area of available habitat, reduce food production, increase water temperatures and expose fish to predation. High stream flows are also detrimental in scouring the stream bed with resultant destruction of food organisms.

(c) Steelhead and cutthroat trout remain in fresh water for two years before migrating to sea. Although these two species usually remain in streams, some cutthroat may inhabit lakes before migrating to sea. The food of young migratory trout is similar to that of coho and spring salmon. Steelhead and cutthroat, because of the greater length of time spent in fresh water, are most vulnerable to any unfavorable conditions of stream flow as noted previously.

Because of differences in life history of the six anadromous species inhabiting the Capilano river, the effects of Cleveland dam on their fresh-water habitat are expected to be various. The site of the dam was the approximate upstream limit for pink and chum salmon, cutthroat trout and occasional spring salmon. The anticipated changes in conditions below the dam should be of little consequence so far as suitability of habitat for young fish is concerned. Pink and chum salmon migrate to sea before minimum flow conditions occur and only a small number of coho, steelhead and cutthroat would remain to be affected. .

Most of the young coho and steelhead will be reared in the upper reaches of the river. After hatching from the gravels of the headwater area, the young fish become rapidly distributed throughout the system. Some ten miles of habitat in the main stream and tributaries above the reservoir will remain unaltered. The three miles of stream bed immediately behind the dam will be replaced by a narrow steep-sided lake, ranging from 100 to 300 feet deep. At the upper end a small but relatively productive shoal area may develop if lake level fluctuation is minimized. Water entering the lake will be very low in dissolved nutrients and the flushing rate will be high except during late summer months. Decomposition of organic detritus left in the reservoir area may contribute toward a small initial surge of productivity, but all physical features indicate the development of an extremely oligotrophic lake.

Comparatively shallow streams with beds of gravel and boulders, such as the Capilano, are extremely productive of insect larvae per unit area. On this stream a relatively large area of oligotrophic lake will replace 3.2 miles of productive stream. The lake will produce more plankton than the original stream but probably little more bottom fauna. Plankton does not normally constitute a major portion of the food of young steelhead or coho. Resident, non-migratory populations of trout and possibly coho will develop in the lake and compete for food with the valuable migratory stocks. There is little evidence to suggest that the reservoir will replace the section of stream as habitat for young fish.

Downstream Migrants — Biological problems: — The anadromous species of salmon and trout differ considerably in some aspects of their seaward migration. Pink and chum salmon migrate as fry usually 3 to 4 centimeters long in March and April. Coho migrate as yearlings, 8 to 12 centimeters long, from late April to early June, usually with a peak movement in the middle of May. Most spring salmon migrate to sea as fry 3 to 5 centimeters long in March, April and May, while some remain to migrate later in the year. Steelhead are variable as to both time of migration and age at migration. However, most steelhead migrate when 15 to 20 centimeters long at two years of age between May and July. Most cutthroat migrate at 11 to 14 centimeters during late winter and spring of their second year.

In their native waters, there is a general tendency for all species to migrate during the first period of rising temperature and increasing flow in spring.

It has been found by Hamilton and Andrew (1954) and Schoeneman and Junge (1954) that sockeye and coho smolts will not readily sound to depths of 40 feet or more to gain exit from reservoirs, but tend instead to delay migration until a surface exit is available. However, spring salmon will readily sound to depths of 65 feet to gain exit from a reservoir when no surface exit is available.

In young anadromous fish which reside in a stream for a year or more, marked physiological changes as well as changes in behavior occur at the time of seaward migration. In the period of stream residence these fish develop strong territorial behavior which tends to maintain their position against the seaward movement of water. However, when the smolt stage is reached, this territorial behavior is suppressed and activity is increased with a resultant displacement seaward of the young fish by the flow of water.

When a reservoir is placed in the path of downstream migrants, the physiological and behavior patterns which serve the fish admirably in a stream may be quite inadequate to cope with the new situation. The lack of strong directive currents in a lake may result in retarding or terminating the seaward movement of smolts.

In a study of coho in Cultus Lake, British Columbia, Foerster and Ricker (1952) found that of those young fish which move into the lake 80 per cent fail to migrate to sea, remaining in the lake as residuals. These residuals, although attaining at least early maturity, do not reproduce successfully and are lost to the stocks of valuable migratory fish. This development of residuals has been indicated in sockeye and has probably occurred many times in steelhead with resultant development of non-migratory lake populations.

On Capilano River it is possible that some diminution of migratory coho and steelhead stocks will result from the development of residuals in the reservoir. As the reservoir will store domestic water and no angling will be permitted, residuals will be of no value but will instead compete with valuable stocks of migratory fish.

Passage over Cleveland dam will have serious effects on coho and steelhead smolts migrating seaward from the upper reaches of Capilano river. The surface exit available is over an automatic drumgate and down the 300-foot spillway to the stream below. The spillway is of the ski-jump type and insures a free fall of relatively slow-moving spray down the final 100 feet of descent. When a deep pool has been eroded in the heavy gravels below the toe of the dam it is possible this final drop may not be injurious to fish. Hamilton and Andrew (1954), in a study of spillway mortalities over a high dam, suggest that abrasion on the concrete face of the spillway causes many injuries to descending smolts. This abrasion becomes increasingly destructive with deterioration of the concrete. It is anticipated that mortalities due to abrasion on the upper 200 feet of spillway may result in significant mortality of coho and steelhead smolts in Capilano river.

As an alternative, or safety measure to provide some relief from expected losses, a hatchery was proposed for installation below the dam near the adult fish trapping facilities. It was believed that the hatchery could provide a limited supply of salmon and steelhead reared to migrant stage to replace at least a part of those lost over the spillway. The possibility of trapping migrants from upstream areas at a fence near the head of the reservoir was also considered.

Engineering problems:—The physical problem presented to downstream migrant fish by the Capilano Dam was the safe descent from a normal reservoir elevation of 570 feet above sea level to a normal tailwater elevation of 290 feet above sea level, a vertical height of 280 feet, in the short distance covered by the dam.

The three available paths of migration were, (1) into the domestic water supply pipes, (2) through the valves in the diversion tunnel, and (3) over the spillway. The entrance to the domestic water supply pipes was screened with a Link Belt traveling screen, to protect the water consumers, and this screen was also considered adequate to prevent entry of fish. Since the entrance to the diversion tunnel is almost 300 feet below the surface of the reservoir, it was considered unlikely that the migrants would use this path to any extent. Therefore the problem resolved itself into examining the effects of passage over the spillway, which was the most likely path of migration.

As originally designed, the spillway works consisted of an automatic drum gate with a free crest spill, a narrow spillway chute at a slope of almost 60 degrees from the horizontal, and a stilling pool lined with concrete and having a lip downstream to maintain the tailwater depth. The resulting hydraulic conditions in the spillway were expected to be a high velocity at the toe of the chute combined with extreme turbulence in the stilling pool or bucket.

Consideration of the problem created by these conditions involved, as a first step, examination of available data on losses of downstream migrants at high dams.

Experiments were planned or in progress at two locations in the State of Washington to determine mortality to downstream migrants over high dams. The first was at Baker River on the Skagit system, a co-operative experiment under leadership of the International Pacific Salmon Fisheries Commission with the assistance of the Washington State Department of Fisheries and the Department of Fisheries of Canada. The second was at two dams on the Elwha River by the Washington State Department of Fisheries. Reports of these experiments have only been published since completion of the Cleveland Dam, but the various conclusions which were applicable to the Capilano were generally known prior to that time.

These experiments indicated that the coho migrants at Cleveland Dam would be unlikely to sound to any depth to seek an exit from the reservoir, particularly if a surface spill was occurring. Since a surface spill was expected almost continuously at the time of migration, it was likely that practically all migrants would pass over the spillway. Secondly, these experiments had progressed to a point where it was known that heavy mortalities could occur at spillways over 200 feet high. None of the spillways tested resembled the Capilano, so it was not possible to predict the extent of or nature of the loss to be expected.

In view of the certainty of mortality to some degree, the possibilities of prevention of migration over the spillway were examined.

It was known that fish could be successfully prevented from entering water intakes my mechanical screens, various types of which have been in use for many years on irrigation ditches, hydro-electric and other water intakes. Costs of such installations have run to more than $100 per cubic foot seconds of water screened, and the cost to screen the maximum design spillway discharge of the Capilano would approximate four million dollars, which was out of proportion to the problem faced. In addition, it was extremely doubtful if the necessary large screen could be physically accommodated at or near the dam site.

Early in the construction phase of the dam, however, a change in the spillway design was made which resulted in a shelving of alternative measures, at least temporarily. The sound nature of the rock in the canyon as revealed after initial excavation at the spillway site suggested that the ''ski-jump'' type of spillway could be substituted for the original chute and stilling pool at a considerable saving in initial cost. A hydraulic model of the new design was hurriedly constructed and tested in the Bureau of Reclamation Hydraulics Laboratory at Denver, and observations of the model in operation were made by fisheries personnel. The new spillway utilized the same gate and chute as the original plan, but the lower hundred feet was cut off and a lip similar to a ski-jump added. Water now was conducted down the chute and ejected into the air horizontally at the ski-jump, from which point it fell freely in an arc ending in a deep cushioning pool below the dam. With the knowledge that similar, though lower, ski-jump spillways already existed in the path of downstream migrants, and no extremely serious mortalities occurred, it was decided that it would be desirable to postphone hatchery construction for a period of three years.

It was recommended that in the meantime careful tests be made of losses over the new spillway, and the problem be reconsidered at the end of the three-year test period. In making this decision, the fact that extensive research was in progress on methods of diverting and collecting migrants above high dams was also an important consideration.

The widespread contract program sponsored by the U. S. Army corps of Engineers among the American Pacific Coast Fisheries Agencies, and programs in Canada under the Fisheries Research Board of Canada and the International Pacific Salmon Fisheries Commission, all stimulated hope that at least a partial solution to the problem might be reached in the next few years. A complete solution has not been reached to date, but the research is being accelerated and extended.

In the meantime the three-year test program to determine spillway survival is about to be implemented. Hydraulic and engineering data on both model and prototype are very complete, and test results will be a valuable contribution to general knowledge of downstream migrant problems at high dams.

DISCUSSION

The chief problems of fish protection encountered on the Capilano river and their suggested solutions can be tabulated as follows:—

PROBLEM	SOLUTION
Upstream migration of adults	
1. Low water flow for transport of fish to spawning grounds below dam (pink and chum salmon).	Arrangements being made for release of minimum flow.
2. Temperature of reservoir water may be too high and fish might be delayed, etc.	Facilities available if necessary to provide sub-surface cold water from reservoir.

3. Adults proceeding to headwaters blocked by dam (steelhead and coho salmon).	Fence, fishway and trucking facilities installed to transport fish above dam.
4. Release of fish in reservoir may delay migration.	Fish trucked to head of reservoir.
Spawning grounds	
5. Spawning grounds below dam may be subject to low flow.	Arrangements being made for release of minimum flow.
6. Spawning grounds below dam may lose gravel.	Artificial spawning channel could be built if necessary.
7. Reservoir inundates some areas above dam.	Could compensate for losses by hatchery operations.
Habitat for young fish	
8. Low flows affect coho and steelhead below dam.	
9. Loss of stream habitat by creation of reservoir—latter may act as substitute.	Could compensate for losses by hatchery operations.
Downstream migrants	
10. Structure of reservoir causes young coho and steelhead to become "residuals."	
11. Migrants may be killed in fall over spillway—possibly not too serious.	May be able to divert migrants. If not, hatchery plantings could supplement runs.

It is apparent that the solution to the problem of anadromous fish protection on the Capilano river is far from complete. There are no data available to predict the extent that coho and steelhead will use the reservoir as habitat or become trapped in it as residuals. Mortality of migrants over the spillway can only be guessed. These major gaps of information point up the two chief needs in planning anadromous fish protection today. First is the necessity for evaluation of the effects of the local project. Observations have already been made on the dates of seaward migration, the time migrants take to travel naturally in the stream from above the reservoir to below the dam, the effectiveness of stream sampling devices for migrants and related problems. Comparison of these measurements with conditions pertaining when the dam is built, including experimental measure of spillway mortality should enable an adequate assessment of the need for hatchery facilities or alternative protective devices. It is emphasized that this is an *ad hoc* approach based on specific consideration of the effects of the dam on the particular group of fish involved. This underlines the second need in planning protection of anadromous fish—a general knowledge of the basic principles of fish biology and physical environment and the way they interact. We have little quantitative data to employ in predicting in advance the type of facilities that will best ensure protection.

Observation and evaluation of cases such as the Capilano will not only serve the immediate purpose of protecting the fish populations of the river but will also provide empirical knowledge that will stimulate and direct the course of research in more general frames of reference.

DISCUSSION

DISCUSSION LEADER SARGENT: Thank you, Dr. Sprules. That is a fine presentation, and I think the imposing list of authors would feel that you served them very well.

I wonder if you would care to comment on your experience with screening. It has been my understanding that screens in many areas have been unsuccessful because they have gotten plugged up with various types of debris. I wonder whether you have to clean them out, or perhaps you don't have that problem. I would be interested to hear any comments you may have.

DR. SPRULES: The screen that has been developed on the West Coast for any water inlet has been what is known as a revolving drum screen which is self-cleaning. The screen is on a drum which is rotated by a paddle wheel arrangement. It is an automatic revolving drum, and as it revolves the residue which is scraped off by another paddle is thrown over the spillway or something of that nature. We have been using them quite extensively in the irrigation ditches in parts of British Columbia. They work very efficiently and require only a minimum of maintenance.

DISCUSSION LEADER SARGENT: Thank you. Are there any questions from the floor for Dr. Sprules?

I wonder if I might ask one other question. In attracting fish to the little ladder that you had, to start them up, you stated that there was a weir that guided them up into the location. If you run into a situation where you can't use a weir because of navigation or something else, I wonder if you have had experience on any other method of attracting a fish to a ladder where a large dam occurs.

DR. SPRULES: We seem to be at cross-purposes here, sir. If there is a large dam, we certainly are not faced with a navigation problem.

DISCUSSION LEADER SARGENT: But below there, don't you have to have a very long weir to guide them into this area?

DR. SPRULES: No. It can be put directly across the river. We slope it just a wee bit from the bank. But to answer your question directly, the only other success we have had is being able to devise a powerful water attraction through the fishway. Now we are using auxiliary jets at all of these dams. We have a normal flow coming in, from what we think from experience, to be the right spot; and then we have coming through the dam auxiliary jets that augment the attraction. That makes a very specific attraction at that point.

The people at the Fisheries Research Board at the West Coast have been working with many devices such as electrical fences and electrical currents and conceivably a supersonic arrangement. That is getting into use now. But to date our best luck has been with the straight water attraction.

DISCUSSION LEADER SARGENT: I see; and then this weir stretches all the way across the river.

DR. SPRULES: Yes.

DISCUSSION LEADER SARGENT: All the way across the whole dam?

DR. SPRULES: Yes. This particular dam is made of ordinary iron piling driven in on a slant to the river. I think the spacing between the iron bars is about a yard and a half. There is a cleaning problem, perhaps not too much. It is devised at the proper height to take both low and high flows. It is an effective weir at all water stages. It leads across directly to the augmented outflow of water from the downstream section.

DISCUSSION LEADER SARGENT: There is time for one more question if anybody has one.

CHAIRMAN FOERSTER: May I make a remark? I don't know whether it is a question or not. In many of our power projects on the Pacific Coast, the big problem in arranging for downstream migration of the young salmon is getting them over the dam when there is no spill, because these power people naturally want to hold back all the water they can, and they object very strenuously to losing any, either through the spillway or other passages which the fish might use. I think Dr. Sprules will agree that that is the major problem in meeting the challenges of these high dams; how to get the fish over the dam when there is no water coming over it.

GROUNDFISH STOCKS OF THE WESTERN NORTH ATLANTIC[1]

WILFRED TEMPLEMAN

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INTRODUCTION

This paper gives an account of some of the chief groundfish populations of the Western North Atlantic and discusses the status of the stocks, some exploratory efforts, and the conservation of these fish stocks.

The area under discussion is the Western North Atlantic from West Greenland to south of Cape Cod at Latitude 39°00′N. The divisions mentioned are the Sub-areas 1, 2, 3, 4 and 5 of the Northwest Atlantic as defined by the International Commission for the Northwest Atlantic Fisheries (ICNAF), Figure 1.

The Canadian groundfish catches include the Newfoundland catches. Cod, haddock, redfish, American plaice and halibut populations, only, will be considered. There will inevitably be somewhat more data and greater coverage from the central areas which are best known to the author.

It is not convenient to give in each case the sources of the statistics

[1]In the absence of the author this paper was read by Dr. J. L. Kask.

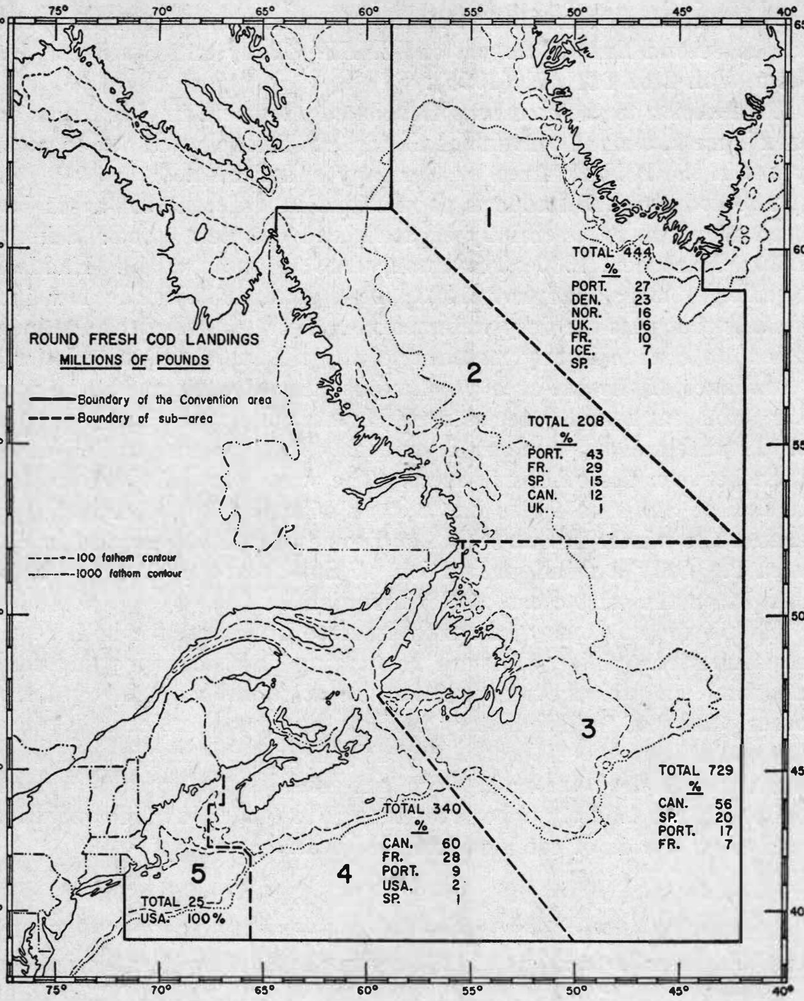

Figure 1. Total landings of fresh round cod from each sub-area of ICNAF in 1953 and the percentage in each sub-area captured by each country. (The catch of a country obtaining less than one per cent of the catch in a sub-area is omitted. Only the southern part of the Greenland sub-area (1) has been shown, although the whole reported cod catch in Sub-area 1 has been included. Catches by Italy of 19 million pounds from sub-areas 3 and 4 are not included. About 11 million pounds caught by St. Pierre and Miquelon, presumably in sub-area 3, are not included in Figures 1 and 2. The data used are the latest available, but are from ICNAF mimeographed data and while generally correct may not be in their find form).

quoted, but the chief sources have been the many unpublished, mimeographed papers prepared in recent years by or for ICNAF, 1952-54, and in addition Sette (1928), Needler (1930a), the statistical portion of the Second Annual Report of ICNAF (1952) and Statistical Bulletin, Vol. 2 of ICNAF (1954).

In addition to the countries mentioned elsewhere in this paper and in Figure 1, a small amount of fishing has been carried out in recent years in the ICNAF Area by Germany and Russia.

The cod and the haddock populations in different sub-areas have been shown to be essentially separate and distinct. (Cod: Schmidt, 1930; Schroeder, 1930; Thompson, 1943; Hansen, 1949. Haddock: Needler, 1930b; Vladykov, 1935; Thompson, 1939.) The few individuals that pass the deep-water barriers between sub-areas are insufficient to change in any measurable way the racial characteristics or abundance of the sub-area populations. An exception must be made for Sub-areas 2 and 3 between which there is no barrier except that of distance and where there is evidently more intermigration of cod than between the other sub-areas. The west coast of Newfoundland section of Sub-area 4 also has a stock of cod, a large part of which spends the winter in Sub-area 3 and some part doubtless also in Sub-area 2. Only a small proportion of Sub-area 4 cod, however, are caught on the west coast of Newfoundland.

Redfish populations apparently migrate little and for both redfish and American plaice, although no tagging has been done on redfish and little on plaice, evidence from size at sexual maturity and from other phases of the life-history indicate no very significant degree of movement between sub-areas. Probably in redfish there is some movement in the deep water between sub-areas, but if such movements exist they are unlikely to be great enough to render invalid conclusions regarding trends of catch within sub-areas.

COD

Exploitation. By far the greatest fishery in the ICNAF area is for cod, *Gadus callarias.* The cod fishery in the Northwest Atlantic began very shortly after Cabot's discovery of Newfoundland in 1497 and has continued for more than four hundred years. Traditionally from the beginning, the fishery has been an international one in the Newfoundland area, Sub-area 3 of ICNAF, which is the greatest cod area. The fishery is also international in Sub-areas 1, 2 and 4. In Sub-area 4 Canada obtains the greatest share of the catch (60 per cent in 1953). In this year France obtained 28 per cent of the catch in Sub-area 4. In Sub-area 5 almost the whole catch is taken by the United States.

At the present time the most important cod fishing countries of the western North Atlantic area are Canada, Portugal, France and Spain. The total of the cod catches by other European countries fishing in this area, namely, Denmark (including also West Greenland and the Faroes), Norway, the United Kingdom, Iceland and Italy, is rapidly increasing.

The total cod catch for the whole area has averaged more than 1,850 million pounds per year from 1950 to 1953.

Over the ICNAF area as a whole from Greenland to Cape Cod the cod catch has been increasing considerably since the early 1930's, and there has been a considerable increase since the end of World War II after a decrease in effort and in catch during the war. There is as yet no indication that the catch of cod in the area has reached its peak. Traditionally in Sub-areas 2, 3 and 4 Canada has caught by far the greatest amounts of cod. The Canadian proportion of the catch, however, has been decreasing and that of the European nations increasing (Figure 2). This has partly been due to the increased catches in Greenland, almost entirely by European ships, but also there has been a decreased Canadian catch relative to the European in Sub-areas 2 and 3. From lack of statistics on the European catch in Sub-area 4, the proportions of the catch caught by European and by North American fishermen in this sub-area are in doubt except for 1953. The United States cod catch has decreased very greatly since 1880 when many of her dory-vessels fished for cod on the Grand Bank and other northern areas.

The total catch and the percentage of the cod catch in each sub-area obtained by various countries in 1953 are shown in Figure 1.

In the northern Sub-areas, 1 and 3, West Greenland and Newfoundland, respectively, there is as yet no indication of overfishing for cod, and the Labrador area, Sub-area 2, is almost certainly underfished for this species.

In Sub-area 4, the Nova Scotian Banks and the Gulf of St. Lawrence, the larger part of the catch (60 per cent in 1953) is obtained by Canada. In this area there has been some decline in Canadian and U.S. cod catches in recent years from peak landings in 1945-46 and the proportion of large cod in the landings has decreased. The available statistics of catch and effort, however, are for too short a period and the data on the European catch in Sub-area 4 is too incomplete to indicate clearly a depletion in the cod population. The 1953 statistics, which for the first time show the apparently large (28 per cent in 1953) catches by France in Sub-area 4 leave us in doubt of the magnitude of the downward trend and render it imperative that

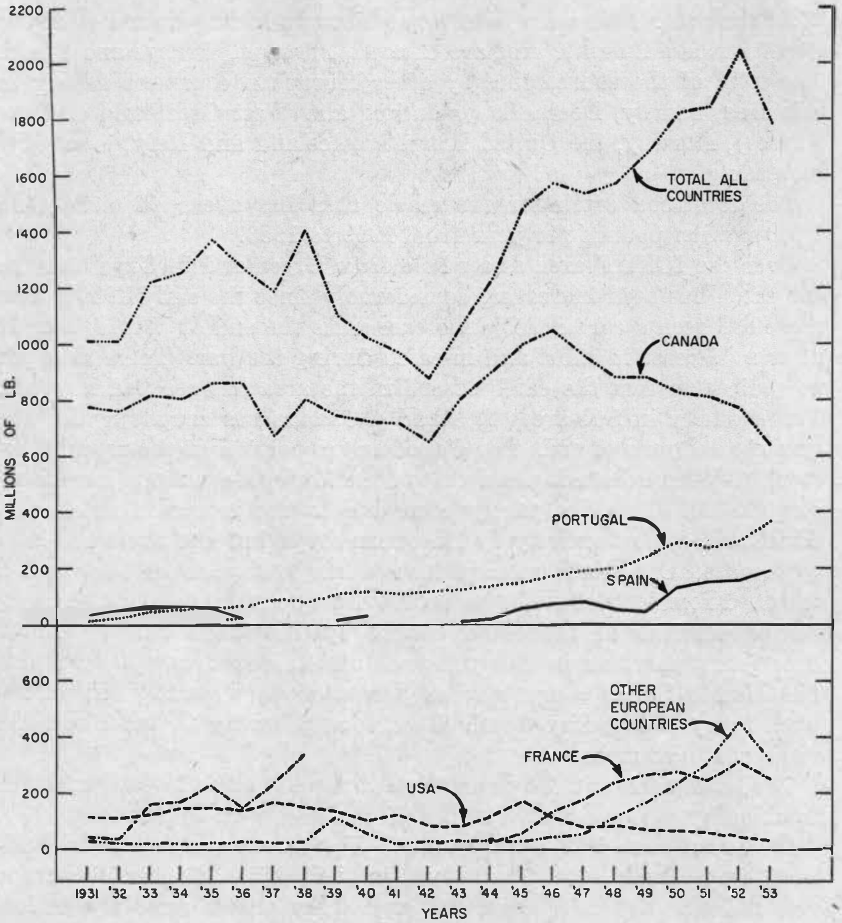

Figure 2. Landings in millions of pounds of round fresh cod, from all five ICNAF sub-areas, 1931-1953. ("Other European countries" include Denmark, Norway, United Kingdom, Italy and Iceland.)

attempts be made to obtain better sub-area statistics and to ascribe past catches, if such is possible on the basis of existing information such as vessel logs, to the appropriate sub-area.

In Sub-area 5, including Georges Bank and most of the Gulf of Maine, and fished entirely by United States vessels, the cod landings have decreased gradually since 1930 and more rapidly since 1946. It does not appear to be certain whether this decrease is a natural one or whether it resulted from a decrease in effort by line vessels specifically directed toward cod, with an increasing proportion of the effort by trawlers devoted to catching haddock and redfish. There is a possibility that the decrease is due to increasing water temperatures which in the long run are unfavorable to the cod near its southern limits, but this should not be too readily assumed until the question of the diversion of effort from cod grounds and from methods such as line fishing, especially suited to cod, is examined closely.

Exploration. It would seem to be almost an impossibility, after four centuries of intensive exploitation by many nations, that any large unfished grounds and virgin stocks of cod remain in the Newfoundland area. Such fishing grounds and stocks, however, have been discovered recently. During its long history, the Newfoundland shore fishery for cod has been typically a fishery of the very shallow waters. The cod, attracted to the very shore by the onshore spawning migration of the capelin, are readily captured in June in 5 or 10 fathoms and progressively deeper as the summer and autumn advance. These inshore cod are generally small and young. The usual gear has been the trap, the handline, and the longline pulled by hand.

On the northeast coast of Newfoundland where these inshore cod are so abundant, fishermen in the main cod-fishing areas, which are near the headlands, have occasionally tried fishing a short distance outward from their regular fishing grounds and finding cod scarce or absent proceeded no further. Their small boats of 30 feet or less in length were unsuitable for offshore voyages and their gear was unsuitable for deep-water fishing. They also had a firm belief that there were no cod available beyond the immediate inshore area. Their fishing was thus restricted to the fishing grounds out to about six miles offshore. Here the cod were typically small, and there were violent fluctuations in numbers.

By its exploratory work since 1950 the Department of Fisheries of Canada working through the Fisheries Research Board and the Board's Newfoundland Fisheries Research Station has changed completely the mental outlook toward this fishery.

Using 55-foot longliners with longline gear worked by mechanical gurdy, the area from 10 to 40 miles offshore has been explored systematically. In the area along the northeast coast of Newfoundland and extending northwards to Labrador the coastal water deepens rapidly to 150 fathoms or more. The Labrador Current hugs the coast and is deepest near the coast. This current has a central core of cold water usually −1.0 to −1.5 degrees centigrade or even lower in temperature and extends in the early spring from the surface to 120 fathoms or deeper. In late spring and early summer the shallow layers warm up and the traditional early summer fishery is for small cod which follow the capelin into this shallow layer of newly warmed water.

In this northern region the bigger and older cod do not usually come inshore, but remain, where all the cod both large and small have spent the winter, in the deep water typically below 120 fathoms. This deep water is Atlantic water connected with the deeper waters of the Atlantic Ocean and is warmer and saltier and heavier than the more superficial waters of the Labrador Current.

These large cod were found to be very abundant in the water near and below the depth where the lower border of the below 0 degrees centigrade layer meets the underlying warmer deep Atlantic layer. Once this relationship had been determined by experimental fishing off Bonavista in 1950 and 1951 the search for deepwater cod was concentrated along the deeper borders of the Labrador Current mostly 10 to 60 miles offshore and abundant supplies of large cod were found. Thus new fishing grounds with abundant large cod were found along a several-hundred-mile stretch of coastal shelf from Trinity Bay to Southern Labrador. Except in a very few places where the deep water came very close to the coast or where, farther offshore, European trawlers had made an occasional set, these deep-water cod had not been fished. The best of the new fishing grounds, which were in deep water 20 to 60 miles off bold headlands, had apparently never been previously fished.

Here was a great reserve of large cod which, as they grew older and larger and became less and less likely to visit the shallow water, were relatively safe from the effects of the fishery.

A new fishery by mechanical longline is rapidly developing for these large cod, and these newly discovered grounds and cod populations appear likely to be able to provide a catch of over a hundred million pounds of large cod per year.

Redfish

Exploitation. The North American fishery for the redfish, *Sebastes marinus,* began to increase rapidly in 1935 when 17 million pounds were landed in the United States as compared with less than 2 million pounds in 1934 and less than 300,000 pounds in 1933. The redfish fishery is in deep water and until recent years has been almost entirely and even now is chiefly prosecuted by United States vessels. The chief area fished at first was the Gulf of Maine but there were some catches on the Nova Scotian Banks. As the local supply became reduced the American fishery on the Nova Scotian Banks increased and gradually spread northward.

From the Fisheries Statistics of the United States (1954) the record American redfish catch of 258 million pounds was obtained in 1951. For the whole Western North Atlantic the highest catch was about 298 million pounds in 1951. Eighty-seven per cent of this catch was by U.S. and almost all the remainder by Canadian vessels. The catch in 1953 was about 230 million pounds. The landings are declining in Sub-area 5 and in the Nova Scotian Bank areas of Sub-area 4, and redfishing is gradually spreading northward. This involves the building of larger and faster redfishing vessels by the United States, to fish the Gulf of St. Lawrence and the Grand Bank areas. Beginning in 1953, the redfish in the West Greenland area, Sub-area 1, began to be exploited in quantity, particularly by Icelandic trawlers.

Exploration. Since 1947 the fisheries research ship *Investigator II,* operated by the Newfoundland Station of the Fisheries Research Board of Canada, has explored the abode of the redfish in the deep water between 100 and 400 fathoms. Some large virgin populations of redfish were found in the Gulf of St. Lawrence, on the South Coast of Newfoundland, on the southwestern and the eastern Grand Bank, on Flemish Cap, north of the Grand Bank and east of Hamilton Inlet Bank in Labrador.

In the warmer more southern parts of the area, such as on the southwestern slope of the Grand Bank, redfish are plentiful from 80 to 200 fathoms, while off Labrador commercial quantities occur only deeper down, from about 160 to 300 fathoms. The northern redfish are considerably larger than the southern and the deep water redfish are larger than those of the shallow water.

In the past few years Canadian trawlers have begun to exploit in quantity these newly discovered redfish populations and American trawlers also have moved into the Gulf of St. Lawrence and to the Grand Bank areas and are landing yearly from these areas a catch several times as great as the Canadian catch. The redfish populations off the northeast coast of Newfoundland and east of Hamilton Inlet Bank have not yet been exploited commercially.

AMERICAN PLAICE

There is a small fishery for American plaice, *Hippoglossoides platessoides,* in the New England area, Sub-area 5. This is of the order of three or four million pounds yearly. The American plaice, however, is more abundant in colder water in the northern Sub-areas 3 and 4.

In the Newfoundland area, Sub-area 3, the fishery for American plaice has developed rapidly within the past few years from a catch of less than half a million pounds previous to 1948, to about 30 million pounds a year in 1951-53. Almost all this fishery is on the eastern and northern Grand Bank. Also in the Nova Scotian and Gulf of St. Lawrence areas, Sub-area 4, the catch of plaice has increased from two to three million pounds a year previous to 1948, to about 15 million pounds a year in 1952 and 1953. About 12 of these 15 million pounds were caught on fishing grounds of the Gulf of St. Lawrence which are in the northern colder water parts of the southern half of the Gulf.

The plaice is abundant and large in the very cold-water areas with bottom water close to 0 degrees Centigrade for most of the year. The Grand Bank areas were discovered largely through the efforts of the *Investigator II* and of Newfoundland otter-trawlers.

The Gulf of St. Lawrence fishery for plaice developed with the recent introduction of large numbers of small 55-60 foot otter-trawlers particularly in the Caraquet area of northern New Brunswick.

The *Investigator II* has found a small area on Hamilton Inlet Bank in Sub-area 2, where American plaice are plentiful, but as yet there is no commercial fishery for plaice in Sub-area 2.

It is a little uncertain at the present time whether or not the catch of American plaice in Sub-areas 3 and 4 is reaching its peak. Doubtless a greater quantity could be caught, at least for a few years, by more intensive fishing.

HALIBUT

The Atlantic halibut, *Hippoglossus hippoglossus,* is less abundant than its Pacific relative *Hippoglossus stenolepis.* On the Atlantic Coast most of the halibut catch is landed by vessels using longline fishing gear. Quantities are, however, landed by otter-trawlers, particularly from the warmer slopes of the banks. These otter-trawl fish are mostly small, young halibut caught in shallower water than the line-caught larger halibut. European vessels fishing in the slope areas of St. Pierre, Green and Southwest Grand Banks for cod and haddock for salting, must catch considerable quantities of these halibut and usually discard them.

Although of lesser magnitude than the halibut population on the Pacific Coast, the halibut catch in the ICNAF area on the Atlantic Coast of North America and West Greenland might be one-quarter to one-third that on the Pacific Coast if protection could be given to the young fish. The United States catch alone was 20 million pounds gutted weight in 1896 and was over 10 million pounds in each of the years 1893 to 1900. Even in 1921 the United States catch was 6 million pounds, while the United States catch per year, in recent years, has been only about 400,000 pounds. The U. S. catches were made chiefly in Sub-areas 3, 4 and 5 of ICNAF and small amounts were taken, in the earlier years, from Sub-areas 1 and 2 and even from the Iceland area. A considerable decrease would have occurred in any case but the rapidity and extent of the decrease has been largely due to the decline in the United States line fishery for halibut.

The Canadian halibut catch between 1933 and 1939 ranged between 3 and 5 million pounds head-off, gutted weight per year. In the period from 1940 to 1948 the average yearly catch was less than 2 million pounds. The low catch was due to reduced effort in halibut fishing during the war years. This led to an increase in weight of the halibut population, which when line fishing effort for halibut rose again, resulted in a catch of over 10 million pounds in 1950, 8 million in 1951, five and three-quarter million pounds in 1952 and 6 million pounds in 1953.

The United Kingdom landed over 11 million pounds of gutted halibut from the Greenland area (Sub-area 1) in 1929, 9 million pounds in 1930, 11 million in 1931, and over 5 million pounds, mostly from Sub-area 1 with smaller amounts from the Labrador sub-area, in each of the years 1928 to 1935. The United Kingdom halibut catch had fallen to 700,000 pounds by 1938.

At present European countries land small amounts of halibut almost entirely from the Greenland area. Complete statistics are not available but the European catch in Sub-area 1 in 1953 was approximately two and a half million pounds.

HADDOCK

There are three large and essentially independent groups of haddock populations on the North American Banks. These have their main winter and spawning abodes on Georges Bank, the Nova Scotian Banks and on the Newfoundland Banks respectively. These main population groupings, each of which in the two northern sub-areas, at least, includes some relatively independent sub-groups, are separated by deep channels.

The New England stock in Sub-area 5 is caught entirely by American vessels and had a peak catch of 278 million pounds of round fresh haddock in 1929. During the past 20 years the catch has stabilized at about 100 to 120 million pounds per year.

The landings of haddock in the Nova Scotian area, Sub-area 4, reached their peak in 1935 when the landed catch in the United States and Canada from this area was 148 million pounds of round haddock. In 1953 the catch was about 100 million pounds, approximately the same amount as in the New England area. In 1953 Canada obtained 56 per cent of the haddock catch in Sub-area 4, the United States 43 per cent and Spain 1 per cent.

Haddock populations on the Newfoundland Banks, Sub-area 3, were first explored by the research trawler *Cape Agulhas* of the Newfoundland Fisheries Research Station in the years 1931-35 (Thompson, 1939). Haddock fishing on these banks was negligible before 1946 and the peak catch was in 1949—about 150 million pounds of round fish. Since that year there has been a decline in the catch which in 1953 was 77 million pounds. Omitting other countries which land only very small amounts of haddock, in the 10 years between 1944 and 1953 about three-quarters of the haddock catch in the Newfoundland area (Sub-area 3) was landed by Spain and one-quarter by Canada. In 1953 in this sub-area, Spain obtained 58 per cent and Canada 40 per cent of the haddock catch.

CONSERVATION

Cod. In considering the general picture of these great fisheries for groundfish in the entire ICNAF area, it is obvious that the cod fishery, after more than four centuries of heavy exploitation, shows by far the greatest resilience and ability to withstand a very heavy fishery. The actual and the potential volumes of production are also apparently very much greater for the cod than for the other groundfish.

There are many features favoring the abundance and survival of cod in the area under consideration. The cod has equally projecting upper and lower jaws and can feed at will on the bottom or in midwater. It can range and feed from the surface to 200 fathoms although it is normally not plentiful below 160 fathoms. With its wide vertical and horizontal range the cod can accommodate itself also to a wide range of temperatures.

In the northern sub-areas the great abundance of the plankton feeding and pelagic capelin, *Mallotus villosus,* provides vast quantities of cod food and is the greatest single food factor in the maintenance of the great mass of the enormous northern cod populations. As the cod grows older and larger it favors deeper waters where it is not so readily fished. While, if food is plentiful, it may at times be present over any type of bottom, the cod, especially at the larger sizes, favors rough rocky bottom which is not readily trawled. In the adult stages cod migrate far and change their abode rapidly. In the colder areas the cod may spend a month or more, during and after spawning, largely in the upper layers of water where they are not available to ordinary otter-trawl gear. The greater and more northern portion of the cod population cannot be fished in the winter and early spring. The cod grows rapidly to a large size and this reduces the number of predatory enemies. Over most of its range, neglecting the relatively scarce sharks, the cod is the dominant large fish. The presumably predatory pollock and silver-hake and young dogfish generally live south of the main cod areas. The cod is not purely a bottom fish and the younger stages are typically farther above the bottom than the larger fish; consequently the destruction of young cod by otter-trawlers is not generally nearly as great as with young haddock. (There are some exceptions to this statement in certain shallow-water bank areas where young cod are abundant.) By far the greatest factor, however, is probably that especially in the northern Sub-areas 1, 2 and 3 of ICNAF, Greenland to Newfoundland, the rough bottom and the very large area covered by low temperature water of minus 0.5 degrees to 3 degrees centigrade favor the cod more than any other groundfish and render more likely its survival on settling from the pelagic post-larval stage. The large expanse of suitable area also, probably close to 100 thousand square miles in Sub-area 3 alone, renders failures of year-classes unlikely and unusual in this central part of the cod range. Also the inshore waters of the east coast of Newfoundland are nurseries for young cod and the shoreward set of the Labrador Current in this area ensures an abundant supply of young cod to settle in these favorable waters. To the northward, however, in the Greenland area there are great fluctuations in year-class survival.

While, doubtless, the cod population could be managed for greater economy of effort and the sizes at capture could be increased and while through natural changes in the sea environment or through very greatly increased exploitation the situation could change rapidly, there is at present no obvious need for repressive efforts to protect cod from over-exploitation in the three northern sub-areas. The situations in the New England area and in the southern Nova Scotia area are more uncertain and should be studied carefully. A number of essentially separate populations of cod, however, exist in the northern sub-areas as well as in the southern. In Sub-area 3 for example there are at least three essentially separate populations. Some of these populations may be over-exploited while others are under-exploited and the total for the whole area or sub-area may not reveal the true picture. It is wise, therefore, to study in detail each population and the effects of the fishery on it. Many countries are carrying out studies on various populations of cod in the ICNAF area.

Redfish. The redfish is a very slow-growing fish, attaining in many areas a size of less than 10 inches in 10 years (Perlmutter and Clarke, 1949). It is usually fished over very rough bottom, in deep water on the slopes of the banks. It is landed round at a low price, and to produce a paying trip, redfish must be caught in large quantities per day's fishing. This species, because of its slow growth and concentrated abundance in very narrow slope areas, suffers quickly under intensive fishing and each new population fished is quickly reduced in numbers. Only recently has very much been discovered on the life-history of the redfish and even now the information is so recent that most of it is unpublished.

The slow-growing redfish forms a concentrated fringe on the slope areas especially on the seaward edges of the banks and in deep depressions and channels. In all these situations it can readily be found and fished in quantity. From its slow growth and concentration and relative lack of migration it is probably continually in danger of being overfished, and once overfished recovery is bound to be slower than in the case of the haddock or cod. At present prices, however, the depletion is likely to be usually more economic than biological, since, especially in distant waters, redfishing must cease when heavy concentrations are no longer available. At this population level the redfish will still have great reproductive potential.

The redfish feeds on pelagic organisms and in Europe has been caught on longlines over very deep water. Thus, there is the possibility that there are great pelagic populations of redfish, generally unrelated to the bottom, of which the schools presently fished are only a fringe where the pelagic populations impinge on the slope areas. Apart from larval distribution, (Taning, 1949), however, there is no evidence as yet for the Western Atlantic that these great pelagic populations exist. The evidence from the experimental fishing of the *Investigator II* in deep water between 200 and 400 fathoms has been opposed to the existence of large pelagic populations of redfish. Also the quick decline in the catch per unit of effort and in the abundance of the larger sizes of female redfish in the newly fished redfish populations of the Grand Bank and other areas, does not give any indication of replenishment of exploited slope and bottom related populations from pelagic populations. The question, however, needs much more research before an adequate answer can be given.

Suitable methods of protection which will also allow a large and profitable fishery for redfish to proceed are not yet obvious to the fisheries scientist. Mesh regulations may be possible but there are considerable difficulties. Among these are the traditional practice of capturing and filleting redfish down to a very small size and the spiny nature of the redfish which causes them to mesh readily. The very slow growth of the redfish, if natural mortality is at all large, may prevent increase in population mass from the smaller to the larger sizes at present utilized. Also, owing to the limited fishing period per day (usually in daylight only), the low price of redfish and the practice of landing them round, it is necessary to catch redfish in large quantities per hour's fishing. Thus a mesh which would liberate a very significant portion of the catch might be uneconomical. On the other hand, on some fishing grounds there might be some increase in efficiency from the larger mesh. Also in many areas during certain months at least commercial quantities of larger redfish may be obtained by fishing more deeply.

American plaice. The very slow-growing American plaice is a bottom-living species, readily caught by otter-trawlers. In northern areas it is especially related to water of about 40 to 120 fathoms in depth. In the most heavily fished plaice areas the bottom is moderately smooth. Also on the Grand Bank, at least, a considerable reserve of plaice exists where the bottom is rough and the plaice population apparently is less concentrated and less available to trawlers.

The plaice in their cold water areas of abundance, in Sub-areas 3 and 4, grow to a large size but are also very slow-growing. In the commercial catches from the Grand Bank (according to Mr. R. S. Keir's age determinations at the Newfoundland Fisheries Research Station), plaice are on the average more than 20 years of age.

These concentrated populations of plaice are newly fished, slow-growing, and readily exploited from their consistent location on or near the bottom in the same areas. The fishery is too new to supply adequate evidence on whether or not the catch has reached its greatest yearly amount and what the maximum sustained yield will be. It is to be expected, however, that a considerable reduction will occur in the catch per unit effort and in the total catch when the accumulated virgin stocks of plaice are reduced by the fishery to the point where the catch must depend on the annual growth of the plaice population.

There is at present no specific plan for protecting American plaice in the ICNAF area. The 4½-inch mesh trawl-net regulations, if applied for cod, will in certain areas be a protection for young plaice. Owing to their preference for even lower temperatures than cod, quantities of plaice are encountered during many cod trips but not usually during haddock trips. For the same mesh size, due to the depth of the body in the plaice, the 50 percent selection point for plaice is at a considerably smaller length than for haddock or cod.

Halibut. The halibut of the Northwestern Atlantic live chiefly in the waters of intermediate temperatures, particularly on the seaward slopes of the banks and in the deep channels. The deep-water distribution parallels that of the redfish which is one of the chief foods of the halibut (McIntyre, 1953).

In some parts of its distribution, the northern part of the Gulf of St. Lawrence, on the southwestern portions of the Newfoundland banks, the east coast of Nova Scotia and where warm water impinges on the slopes of the banks, much of the halibut population in spring and summer moves into shallower water near the slopes of the banks or inshore. Elsewhere as on the east coast of Newfoundland and Labrador, where there is a barrier of below 0 degrees centigrade water, the halibut remain throughout the year in deep water, usually more than 120 fathoms, and do not approach the shore.

A great deal of otter-trawling for other species occurs on the slopes of the banks and young halibut are particularly vulnerable as incidental catches in these fisheries for cod, haddock and redfish. These young otter-trawl halibut are doubtless caught many years before they have reached their best population weight for capture.

In the halibut areas of the Pacific Coast the halibut is the dominant large fish on the banks and otter-trawling of halibut is regulated to the advantage of the line fishermen. In the Atlantic area, however, the most important fishes on the banks near the halibut areas are the cod, haddock and redfish and the value of the halibut both actual and potential is not large compared with the value of the fishery for the other groundfishes. Up to the present time no good method has been found to protect halibut in the Atlantic areas where otter-trawling and line fishing for other species are greatly predominant. The present and the potential values of the Atlantic halibut fishery, however, are by no means negligible, particularly for Canada, since the landed value of halibut per pound is considerably greater than that of cod, haddock or redfish. In spite of the discouraging prospect of success, fishery biologists should occasionally try a little imaginative effort in search of ideas for the protection of the Atlantic halibut.

Haddock. Haddock grow rapidly in comparison with redfish and American plaice. They are, however, considerably faster growing in the southern part of their range on Georges Bank, than in the northern part, on the Newfoundland Banks. Haddock may thus enter the American fishery on Georges Bank at 16 inches in length and less than three years of age, while haddock of the same size on the Grand Bank and St. Pierre Bank are four or five years old. Each population has its own growth and survival characteristics and must be studied independently. Considerable yearly differences in the survival of year-classes occur in all haddock populations. These fluctuations, while always great, are much more extreme in the northern than in the southern parts of the haddock range in the ICNAF area. On the Grand Bank and St. Pierre Bank the survival rate of one haddock year-class may be a hundred times greater than that of another. Under these conditions accurate long-range forecasting of haddock abundance is at present impossible. Short-range predictions, however, are being given by studying the abundance and growth of year-classes of haddock from about one year of age and older.

Compared with the cod, the haddock with its underslung mouth is much more closely related to bottom living and feeding and to occupancy of smooth bottom areas. All stages from one year of age and upwards are thus more vulnerable to destruction by otter-trawls than are the young of other groundfish species of similar age. In contrast with the cod where the greatest populations live beyond the influence of the Gulf Stream, all the major haddock populations of the Western Atlantic live and spawn on the northern edge of the Gulf Stream. Large eddies exist between the Gulf Stream and the haddock areas of the various banks. These eddies in certain years and months may suck off the water from the neighboring haddock areas of the banks. With the water, at any time during their several-month-long pelagic existence, the young haddock may be drawn off over oceanic areas with water more than a mile deep (Iselin, 1939). As in the European area, winds are doubtless of great importance in the northwest Atlantic in the movement to favorable or unfavorable environments of the superficial layers of water containing the haddock larvae (Carruthers, Lawford and Veley, 1951; Carruthers *et al,* 1951; Carruthers, 1951). In the New England area the widespread nature of the dispersion of haddock eggs and larvae and the importance of currents in producing this dispersion were shown by Walford (1938).

Inevitably, when several months after hatching, these young haddock settle to take on a bottom-feeding existence, they must perish if they are not over bank or slope areas preferably less than 100 and at the most about 150 fathoms. All this is somewhat hypothetical but very likely represents the actual case. The haddock in all areas of the Western Atlantic spawn near very deep water and it is unlikely that variations in food or temperature can account for such complete losses of haddock year-classes as occur in the Newfoundland region.

In addition to their dangerous spawning situation, the young haddock have the cod and presumably the pollock, silver hake and other fishes as predators.

In the New England area, Sub-area 5, an experiment on using larger meshes in otter-trawls was begun in 1953, following advice from ICNAF. This and other measures of haddock protection were advocated much earlier by Herrington (1932, 1935, 1936 and 1941). It is hoped to prevent the killing of small haddock of sizes which were formerly thrown away at sea, and also to release for further growth some of the small haddock at sizes formerly landed. Thus it is expected that increases will occur in the landed size and in total landed weight. Since the larger fish are more valuable per pound, the values should increase also.

In Sub-areas 3 and 4, gear and population researches are in progress to study the biological basis for mesh regulations under ICNAF.

From the typically bottom abode of the young as well as of the older haddock a mesh regulation requiring a minimum inner wet measurement of 4½ inches in meshes of otter-trawls should assist the preservation of haddock much more than that of cod. In most areas a larger mesh than 4½ inches would probably be required to have a measurable effect on the cod population.

For areas and degrees of haddock abundance in which the slowing effects of crowding on growth are sufficiently small or not evident, it is possible to say that saving young haddock which are discarded at sea should represent in the future some increase in the landed catch. In the New England area, until recently, an otter-trawl cod-end mesh with internal dimensions of 2⅞ inches was used. It is possible for this area to calculate mathematically with some degree of confidence that the gain in weight by growth is probably greater than the loss from natural mortality at the sizes which would escape from a 4½-inch or even somewhat larger mesh (Graham, 1952). Thus it may be argued that greater total catches and larger more valuable fish will be landed by the larger mesh. The expected eventual increase in yearly landings for the Georges Bank area is about 30 per cent if the fishing intensity existing at the time of the change to the larger mesh is maintained (Graham, 1952). Graham and Premetz (1955) state, on the basis of the first year's experience with the mesh regulation and in the light of recent changes in haddock sizes exploited on Georges Bank, that the long-term increase is expected to be greater than 30 per cent.

There are, however, many more unknowns than knowns involved in the argument. There is first of all the fact that the large mesh will protect not only haddock but also cod, silver hake, small dogfish, small flatfishes and other species which either are predators on haddock or compete for food with them at some stage. The effect of the larger population of small haddock below the size of retention by the large mesh, on the survival of members of still younger year-classes is unknown. Herrington (1944) has presented data supporting the idea that the presence of large quantities of older haddock is unfavorable to the survival to the scrod size of large numbers of young. It is also not possible at present to know either the total or the natural mortality rate in the youngest year-classes of haddock. A net, for example, with all its meshes so small that no one-year-old haddock can escape, will doubtless be very inefficient for capturing the larger sizes. Research vessels, also, are probably not numerous nor available enough to cover bank areas adequately for abundance studies. Thus mortality calculations for younger fish are projected back, probably incorrectly, from the older year-classes of haddock which are fully represented in the commercial fishery. While the total yearly mortality of the larger sizes can be estimated with some degree of precision, the natural mortality is in doubt at all ages but especially at the youngest and the greatest ages.

The large-meshed net is apparently more efficient for catching the larger fish (H. W. Graham, 1954, and Graham and Premetz, 1955) and thus while younger immature fish formerly caught are spared to increase in size, the older fish, from the smallest sizes fully retained, are under greater pressure and consequently should have a greater percentage total mortality than previously. Ships using larger mesh will, when it is economically worthwhile, deliberately fish for larger fish. Even when operating in the same very large and concentrated schools of small haddock as presently exist on the Newfoundland Banks, large-mesh trawlers pull the net for a considerably longer period for each set and doubtless per day's fishing than the trawlers using small mesh. Since the large-meshed net also should move somewhat faster for the same power expenditure and strain water more readily so that less water is pushed ahead of the net, it is inevitable that more fish, of the sizes fully retained by the large-meshed nets, will be caught per day's fishing by the large-mesh than by the small-mesh trawlers. Even the larger sizes below those fully retained by the large-meshed nets should show increased capture rates. If there is an increase in size and numbers and consequently value per trip, the fishing pressure will increase to reduce survival and size (M. Graham, 1954). These situations may be unfavorable only at sizes and ages where the total weight of the population is still increasing rapidly and may be favorable if an age has been reached where natural mortality equals or exceeds increase in weight by growth.

It is becoming apparent that in the Newfoundland area, where the extremes of hydrographic conditions may often concentrate haddock in dense schools over comparatively small bottom areas, the possibility cannot be overlooked that crowding retards growth. It is quite possible that similar crowding effects may be present in the cod of the Greenland and the Labrador sub-areas. Where such crowding is extreme the use of the large-meshed net and the saving of small haddock, whether previously discarded or retained, may not produce the expected benefits.

Since the older fish, with very much greater numbers of eggs per fish than younger fish, are being reduced in numbers faster by the more efficient large-meshed net, and the younger fish saved to grow toward maturity, it is uncertain whether or not the total number of fertilized eggs produced by the haddock population will be increased by the large-meshed net. Many biologists will argue that within very large limits the size of the spawning population is of little or no importance. Experiences with Pacific salmon, however, show that while percentage survival of young is greater with small spawnings the total survival is greater with large spawnings, although with still larger spawnings there may be some reduction in total survival of young (Ricker, 1954). With the tendency toward destruction of new haddock year-classes before settling, it may well be important that haddock should spawn in as great a quantity as possible, and especially over as great an area as possible. The latter is obviously more likely to occur when the spawning population is large. Thus the chances are increased that at least some fringes of the floating young will settle in suitable depths and locations.

The institution of a larger mesh on otter-trawls, for groundfish populations with suitably fast growth in relation to the natural mortality, is very likely a step in the right direction but it is not necessarily the only step to be taken. At present it is best to regard the institution of a larger mesh size as a very worth-while and long-range experiment, which, with all the variety in natural survival of young haddock and of fishing intensities, will take many years to work out. For a considerable time it will be unwise to allow other conservation measures to interfere with the assessment of the mesh experiment. In the Northwest Atlantic the best biological and statistical background for this experiment is for the haddock of the Georges Bank area. Since the area is fished by only one nation, the most complete biological and statistical data and the most efficient control of the enforcement of the mesh regulation should also be possible in this area. Proceeding northward there is less background data available and more nations involved in the cod and haddock fisheries. Thus there is much less likelihood for sub-areas north of Sub-area 5, that if in the near future, larger mesh sizes are established by regulation, it will ever be possible to offer scientific proof of the value of the regulation.

It would be preferable that one sub-area be left without mesh regulation, or even better, to have the fishery in one sub-area deliberately carried out by the usual type of small-meshed nets, so as to serve as a control to the large-mesh experiment. This, however, may not be possible since ships in one trip may fish several sub-areas and difficulties of law-enforcement arise.

Climate. Over and above all the considerations discussed is the overriding effect of climate (Rapports et Procès-Verbaux, 1949). Increases in sea temperatures in recent years are credited with the great increase in the cod population in the Greenland area (Jensen, 1939; Dunbar, 1951; Tåning, 1953). Warmer water species such as billfish and mackerel have recently shown a phenomenal increase in the Newfoundland area (Templeman and Fleming, 1953). Recently, pollock have very greatly increased in numbers in Sub-area 3. Sea temperatures have increased, also, in the New England area (Taylor and Graham, 1953) and decreases in cod abundance in the southern part of the ICNAF area may be a response to warming waters. By the same token, if water temperatures increased more, it is possible that on the Newfoundland Banks the haddock population and the area populated by haddock would increase, and the cod population in the whole northern area shift slightly northward. The higher temperatures, however, will also increase, in the northern haddock areas, warm-water predator or competitive species such as pollock, silver hake and dogfish but decrease the predator cod. The resultant effect on the haddock population cannot be predicted adequately.

Slight increases in temperature might favor the growth of the American plaice of which the great commercial populations are in cold waters near 0 degrees centigrade. The lack of large American plaice populations, however, in the New England and the Nova Scotian Bank areas and in the relatively warm Southwest Grand Bank area indicates that a very few degrees temperature increase would probably render the present plaice areas unsuitable for maintaining large populations of plaice. Other flatfish species with higher temperature requirements—the winter flounder and the yellowtail flounder—would increase in numbers in Sub-area 3.

Particularly at the northern and southern extremes of the range of a fish, water climate and natural fluctuations in survival of young have usually much more influence on fish numbers in the sea than any interference by human efforts. Our reasoning regarding the effects of mesh and other regulations on the fisheries is based on environmental conditions remaining approximately the same. Unfortunately for our ability to reason accurately in these matters, climatic conditions change continually and at present unpredictably. Continual routine hydrographic observations at the same stations in the fishing areas are a necessity and the biologist must do what he can to understand and assist nature in relation to fish populations. He must be wary of a typical device by which the usefulness of regulations is shown. A common method and often the only safe one politically is to make a regulation at a time when through natural fluctuations or for economic reasons the catch is low and then attribute to the regulation the increase in catch which is inevitable in any case. Fisheries regulations should usually be regarded as experimental. They should be as few as possible. It is my opinion that the fisheries biologist should not as a rule recommend an important fisheries regulation until a serious effort has been made to provide the essential background by which the effect of the new regulation may be assessed.

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DISCUSSION

DISCUSSION LEADER SARGENT: Having served with you, Dr. Kask, on the International Commission, I feel that you have presented a very long and difficult problem in a very brief period of time.

I wonder if you would care to comment on the question of climatic change as respects the fishery on the banks of Newfoundland. We understand the cod fishery of George's Bank, off of New England, has decreased presumably because of the warming of the waters. Likewise, we understand very definitely that the hatch has been increasing off the West Coast of Greenland where there was formerly virtually no cod fishery at all, and I wonder whether there has been a noticeable change of the current population on the Grand Banks as a result of this climatic change.

DR. KASK: I would like to refer to that very interesting development. Most of us think of climatic changes as taking great, great lengths of time. Most of us, I think, are not too much concerned with being overtaken by a new Ice Age. But during the lifetime of most of us in this room there has been a marked change in the climate of this Northwest Atlantic area. The Sub-Arctic area has been progressively warming up. Although the effect on land and real estate is not so great and although people in the northern climate still believe their climates are getting a little warmer or better, but a change of a fraction of a degree in the temperature of the sea makes an enormous difference in the distribution of fishes.

Distribution of these ground fishes has changed quite materially in the last couple of decades. The New England banks are now almost entirely haddock banks because the haddock like warmer water than do the cod. The haddock migrated off George's banks and have been caught on Nova Scotia banks. Areas west of Greenland are now the scenes of active and large cod fisheries.

The haddock that Dr. Templeman reviewed in his paper—haddock catches being made on the Newfoundland banks are relatively new, because with this warming up of Arctic and Sub-Arctic waters—the haddock have moved from the southern areas on to these more northern ones and are producing sizable fisheries there.

PHYSIOLOGICAL INVESTIGATION OF CAPTIVITY MORTALITY IN THE SEA OTTER (*Enhydra lutris*)[1]

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An attempt in 1951 by U. S. Fish and Wildlife Service refuge personnel to transplant sea otters from Amchitka Island in the western Aleutians to other areas of its former range met with failure because of the inability to keep the animals alive in captivity. Subsequent efforts along the same lines were equally unsuccessful. The causes of death in captivity were not immediately apparent although superficially the pathology of the dead captives resembled that of animals dying naturally around Amchitka. Survival in captivity was relatively short, varying from a few hours to one instance in which an adult male lived for 11 days. The average survival period was three to four days. The only striking and constant gross lesion attending death was a severe gastro-enteritis often to the extent of sloughing of the intestinal mucosa. The otters gave outward appearance of accepting and adapting to captivity with relative ease, often accepting food within minutes after capture.

Extensive discussion and correspondence with Robert D. Jones, Jr., Refuge Manager, Aleutian Islands National Wildlife Refuge, who had made all previous attempts to keep the sea otters in captivity, led to the formation of a hypothesis concerning captivity mortality based upon his observations and experiences. According to this

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[1]Journal Paper No. 853 from the Purdue University Agricultural Experiment Station.
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hypothesis, the otters suffered from an acute stress or shock reaction, possibly neurogenic in nature, arising from the circumstances of their capture and confinement. It was difficult to test the hypothesis with known facts. Except for a preliminary study of the parasitology (Rausch, 1953) and numerous incomplete or sketchy field observations of its habits, virtually nothing was known about the fundamental life processes of the sea otter prior to this investigation.

During February and March, 1954, a project was undertaken at Amchitka Island, in collaboration with U.S. Fish and Wildlife Service, having the following objectives:

1. To test the hypothesis that captivity mortality was due to a physiological stress or shock reaction.
2. To devise techniques for capturing and holding sea otters which would prevent captivity mortality.
3. To make fundamental physiological observations on the sea otters which would provide a basis for future investigations.
4. To collect such other basic scientific information about sea otters as time and circumstances would permit.

The work resulting is admittedly incomplete and should be considered as primarily an exploratory investigation.

PROCEDURES

In order to get tissues and observations from as nearly normal animals as possible, six wild sea otters were shot from ambush and brought into the laboratory immediately. Five were killed instantly with a head shot from a .257 Roberts rifle. The sixth was shot through the thorax and died in less than three minutes. A seventh was killed with a lethal dose of Nembutal a few minutes after capture while asleep on the beach. The shot specimens arrived in the laboratory in less than 10 minutes after the shot was fired, the average time being 6.5 minutes. The animals were observed, usually feeding, from 10 minutes to over an hour before being shot and were judged to be healthy.

Immediately upon arrival of the carcass at a dockside laboratory the temperature was taken, the pituitary was excised and preserved, blood was drawn and either diluted directly for subsequent procedures or an anticoagulant added, and liver samples were taken for liver glycogen analysis. The temperature was taken by inserting a prewarmed thermometer to a depth of about 10 centimeters into the rectum for 40 seconds and then to a depth of 15 centimeters for 20 seconds longer and read. Blood was obtained by cardiac puncture from the left ventricle by inserting a 3-inch 17-gauge needle between the 5th and 6th ribs just left of the sternum.

Following these initial procedures, the carcass was weighed and transported to the main island laboratory. The gross necropsy was started immediately. Samples of brain, liver, diaphragm and leg muscle were first excised and weighed for the tissue moisture content procedure. All organs were critically examined and any signs of apparent abnormalities were noted. The thyroid, adrenals, kidneys, and liver were weighed. Samples of all major body organs (brain, spinal cord, skeletal muscle, heart, lung, spleen, liver, kidney, thyroid, pancreas, lymph node, stomach, duodenum, jejunum, ileum, colon, gonads, and bone) were preserved in A.F.A. and/or Bouin's fixative. The gastrointestinal tract was opened along its entire length and the contents, condition of the lining, and presence of parasites noted.

For comparison with the tissues and observations from the normal animals, similar procedures were carried out on dead or moribund animals which had been taken captive. Captures were made with a dip net while the sea otters slept on the beach or inshore rocks or while swimming and feeding off shore. They were transported back to the base in an aluminum dog crate or simply loose on the floor of the Jeep or bottom of the dory. Upon arrival at the base they were weighed and placed in confinement.

Some animals were held in a tank of sea water about 30 inches deep and 10 feet in diameter with a rock "island" 3 feet square for the animals to rest upon. The water was pumped directly from the sea and probably remained a few degrees below sea temperature (39° F). A building located a few feet east of the tank afforded a windbreak from this direction, but no other protection from the weather was provided.

Other animals were confined in cages 8 by 24 by 48 inches covered with 1 by 1-inch welded wire fabric. Subsequently a small unheated wooden building was remodeled for an animal house. The plank floor of this building was covered with one-half inch of sawdust overlaid with an inch or more of dried grass and provided an area about 15 by 20 feet in which the otters could move about freely. Windows in the east, north, and west walls admitted ample light, and although not open to the direct force of the wind, the walls and windows allowed some penetration of wind and driven rain so that a damp draughty atmosphere prevailed. Grass-filled cages, described above, were placed against one wall of this building and were available to the otters in the daytime and used to confine them at night.

One otter was kept for 11 days as a "pet," having the run of the men's quarters when not confined in a grass-filled cage.

During the first few days of their captivity, feeding was haphazard due to difficulties of securing enough palatable foods; but when a backlog of live fish was accumulated, the otters were fed four times daily at 0700-0730, 1100-1200, 1600-1700, and 2100-2300. Depending upon the available supply, the amount given at each feeding varied from one-half to two pounds of fish per animal. The entire fish was fed, usually chopped into small chunks. Occasionally marine invertebrates, primarily blue mussels, limpets, hermit crabs, and octopus, were given in small amounts to supplement the fish diet. Every other day about 10 grams of a dehydrated milk-like product, Terralac[3] was sprinkled on the food of each otter.

The passage of food through the alimentary tract was timed by feeding a meal containing blue mussels or other shelled mollusks to animals that had been receiving an exclusively fish diet for at least 48 hours. The occurrence of shells in the feces then gave the time of passage of the material through the gut. The frequency, character, and amount of the feces were noted.

Fresh water was provided the otters in tip-proof pans or in the form of snowballs which they readily accepted. No conclusive attempt was made to determine the otters' preference or need for fresh as compared with sea water. Otters held in the tank were not provided with fresh water; although snow was often on the rocks in the tank, the otters were never seen eating it.

Breathing rates and pattern and heart rates were obtained by direct observation of sleeping or quiescent captives. Rectal temperatures were secured from anesthetized animals.

Necropsy of the dead captives was carried out exactly as for animals shot except that no liver glycogen analysis was made. If the animal was judged to have been dead longer than three hours no blood determinations were made.

The following laboratory procedures were completed on Amchitka: tissue moisture content; organ weights; blood cell counts; blood hemoglobin determination; blood sugar determination. Procedures started on Amchitka and finished on campus were histological work and liver glycogen determination.

RESULTS

The animals used in this study are divided into three groups. The first group will be referred to as "normal" and includes one

[3]Terralac was supplied for this work by Charles Pfizer and Company, Inc., Brooklyn, New York.

adult and two subadult males and three adult females which were shot in the wild, as well as one subadult male killed immediately after capture by a lethal dose of Nembutal. These animals ranged in weight from 11.3 to 37.2 kilograms.

The second group referred to as "pathological" is made up of one adult and seven subadult males and one adult female which died after 12 hours to 7 days in captivity. Also included are two subadult males and one subadult female found dead on the beach. These animals ranged in weight from 9.9 to 31.8 kilograms at the time of capture.

The third group, consisting of one subadult male and two subadult females, lived for more than three months in captivity and were still alive when these observations were concluded.

Necropsy. At necropsy the seven normal animals appeared to be in good physical condition as indicated by the presence of considerable amounts of subcutaneous and visceral fat. The organs of these animals appeared quite normal except in the lower gut where there were indications of a mild inflammation of the jejunum and ileum in the form of local, mild hyperemia and petechiae of the mucosa. In no case was blood found in the lumen of the gut. Very little fluid was noticed in the body cavities.

The pathological group revealed a somewhat different picture. Two animals of this group were devoid of fat and two others showed markedly less fat than was observed in the normal animals; however, three of them were quite fat. Nearly half of these animals had marked pulmonary congestion. In three instances, the lungs were nearly black and filled with blood. Cardiac decompensation, as indicated by engorgement of the large veins and right side of the heart, was observed in over half of these animals. The abdominal and thoracic cavities usually contained several hundred milliliters of a pinkish or brownish serous fluid.

Gross examination of the abdominal organs showed few abnormalities except in the gastro-intestinal tract. The organs were usually darker colored, indicating vasodilation, but specific lesions were noted in only two instances. Cysts were observed on the spleen of one animal and the liver was pale in another. Two of the 13 animals in this group showed mild enteritis. In all others, gastro-enteritis was moderate to severe. In some animals the gut was so inflamed that externally it was an angry purplish-red color along its entire length. The condition of the lining varied from isolated areas of hyperemia and petechiae to a continuous severe ecchymossis with erosion and sloughing of the mucosa. The lumen usually contained blood. No perforating ulcers were found.

Histology. The histological structure of the sea otter closely resembles that of other mammals. The tissues are quite vascular and even in the normal animals there was a condition which might be construed as hyperemia in some organs, particularly lung and kidney. The secretory epithelium of the gastro-intestinal tract is well developed. An indication of the high level of activity of these organs is indicated by the branching of the gastric pits, crypts of Lieberkuhn, and the villae. Most of the specimens, both normal and pathological, showed numerous old scars in the enteric mucosa, submucosa, and muscularis, indicating some old damage to the mucosa. This was true of juvenile as well as adult animals.

The speen is highly trabeculated, with numerous thick-walled blood vessels, which might indicate that the sea otter's spleen functions primarily as a blood reservoir.

The adrenal gland is somewhat unusual. It is irregular in shape, usually with several lobes. The capsule and various zones of the cortex are easily recognizable but the medulla has no distinct definition. The group or cord arrangement of the cells usually seen in the adrenal medulla is nearly absent. In the center of the gland is a large sinus which is usually filled with blood.

In general the tissues of the pathological animals were more hyperemic than those of the normals. This was particularly true of the lungs which in some instances showed extravasated blood.

The most remarkable difference between the normal and pathological animals was found in the gut. Extensive necrosis and erosion were found in many pathological animals. In some instances this included sloughing of the mucosa. Severe hyperemia and extravasation of blood to form large sinuses were often observed in the submucosa and muscularis.

In several pathological animals the liver showed some abnormality also. The hepatic sinusoids had lost definition and lobules were indistinct. In a few cases the pyramidal cells of the central nervous system seemed to be slightly shrunken, usually an indication that the animal had been subjected to stress prior to death. There was also some indication of increased thyroid activity (diminution of colloid and increased height of epithelial cells) in one or two of the pathological animals.

Organ weights. The weights of the pituitary, adrenal, thyroid, liver, and kidney and their percentage of total body weight are given in Table 1 for both normal and pathological animals. The figures

TABLE 1. SUMMARY OF BODY AND ORGAN WEIGHTS AND VALUES FOR BLOOD COMPONENTS AND TISSUE MOISTURE IN NORMAL AND PATHOLOGICAL SEA OTTERS.

		Normal Animals			Pathological Animals		
		No. of animals	Range	Average	No. of animals	Range	Average
Body weight (kilograms)		7	11.3-32.7	19.6	12	7.2-30.8	13.3
Organ weights	Pituitary (mg)	3	117-156	142	9	101-176	136
	mg % of body wt[1]			1.0			1.1
	Adrenal (gm)	7	1.480-3.030	2.373	12	1.029-5.378	2.387
	mg % of body wt			13.1			19.0
	Thyroid (gm)	5	0.848-2.423	1.597	9	0.605-2.369	1.120
	mg % of body wt			7.6			8.6
	Kidney (gm)	6	227-1077-	414	11	170-539	244
	% of body wt			1.9			2.0
	Liver (gm)	7	653-1589	970	11	397-1785	821
	% of body wt			5.1			6.2
Blood	RBC/mm^3	7	4,008,690-5,524,700	4,918,700	5	4,090,500-9,494,000	6,001,320
	WBC/mm^3	7	2302-4686	3485	5	1520-4767	3339
	Eosinophils/mm^3	7	44-283	127	3	11-88	39
	Hemoglobin gm %	7	12.0-15.5	14.2	5	10.5-21.0	15.1
	Sugar mg %	7	87-137	112	5	50-167	87
Tissue moisture (per cent)	Leg muscle	7	71-75	73	9	55-76	73
	Diaphragm	7	69-74	72	9	66-89	75
	Liver	7	64-72	67	9	65-82	70
	Brain	5	66-77	72	8	72-81	76

[1]Averages of organ weight/body weight calculated from individual percentages.

for the animals which were shot may be slightly low because of the blood lost at the time of death; however, this cannot account for the total difference between these figures and those for the pathological animals.

Tissue Moisture. The percentage of water in leg muscle, diaphragm, liver, and brain (Table 1) may be slightly low because of inadequate facilities for completely drying tissues. The figures for normals may be still lower because of fluid lost at the time of death; however this loss cannot account for the total differences between the normal and pathological groups.

Blood studies. See Table 1. In two cases values were obtained from the same animal at the time of capture as well as after death. In one there was a very slight increase in the red blood cells, and a decrease in the white blood cells, eosinophils, and blood sugar at death. This animal lived only six hours in captivity; it became hyperthermic following Nembutal anesthesia. In another anesthetized animal which lived 38 hours in captivity, there was a marked increase in the red blood cells and homoglobin concentration, with a marked decrease in white blood cells, eosinophils, and blood sugar at death.

Body temperature and temperature control. The deep rectal temperatures of six normal animals taken a few minutes after they were shot were 95.0 degrees, 96.5 degrees, 99.5 degrees, 99.5 degrees, 100.0 degrees, and 105.5 degrees Fahrenheit, averaging 98.5 degrees Fahrenheit. The deep rectal temperature of animals shortly after the onset of Nembutal anesthesia were 100.0 degrees and 100.5 degrees Fahrenheit. The sea otter apparently does not have good temperature control. In captivity, particularly if insufficient food is available, it suffers greatly from temperature changes. Shivering was noted on numerous occasions, in the animals kept in the tank and in others regularly kept in a dry environment but given a swim. When three animals that had been kept in cages for several days were placed in the water tank for less than one minute, two shivered excessively and did not recover full strength for at least 12 hours after the experience.

An animal that had recovered from anesthesia was placed on the damp wooden floor of an unheated shack over night. In the morning (0700) it was comatose and the body temperature was 79 degrees Fahrenheit. Successive baths in water 105 degrees Fahrenheit, and intraperitoneal injection of glucose and Metrazol effected a temporary recovery of this animal with a return of body temperature to 98 degrees Fahrenheit at 1100. The animal accepted food at this time. At 1800, eight hours after recovery the animal began to scream, and though it was still weak it appeared normal. At 2400 its body temperature was 102.5 degrees Fahrenheit, and it had become restless. At 0120 the next day it was given a subcutaneous injection of phenobarbital to quiet it. At 0245 it was sleeping, breathing regularly, and its temperature was 104.0 degrees Fahrenheit. The animal was dead at 0715. It was noted subsequently that the fur "slipped" on the hide of this animal which may indicate the hyperthermic condition at the time of death.

Panting of sea otters was noted on several occasions following the exertion attending capture on the beach; however, this activity was never sustained for more than a minute or two at a time. The mouth was only slightly agape and the tongue was not extended.

Panting was not observed in an animal which died when the body temperature was 106 degrees Fahrenheit following Nembutal anesthesia. The body temperature at the time of capture and anesthetization was 100 degrees Fahrenheit At 4 hours the animal began to recover righting reflexes; the body temperature was then 103 degrees Fahrenheit. The animal was placed on a cool (30 degrees Fahrenheit), damp floor. It showed no further signs of recovery. The breathing became dyspneic and the heart beat was irregular about 7 hours after anesthetization with a body temperature of 105 degrees Fahrenheit. Ten minutes later the body temperature had risen to 106 degrees Fahrenheit, breathing stopped, the heart fibrillated, and the animal died.

Captive otters were observed to be rather restless and to seek the coolest area in a heated room. They avoided spots of direct sunlight which warmed their dark fur rapidly.

Heart rate. The heart beat of otters that seemed to be in reasonably good condition was strong and regular. The resting rates for three subadult animals that had been in captivity for two to three weeks are given in Table 2. Heart rates of two animals immediately after capture and injection with Nembutal, but before anesthesia was complete were both 168 beats per minute. In deep anesthesia, a heart rate of 188 beats per minute was recorded. In two animals which were comatose and in which the body temperature had begun to fall, but which subsequently recovered temporarily, the heart beat was very weak and irregular. During recovery, the strength of the beat was recovered appreciably sooner than a constant rhythm was established. At the point of hyperthermic death, the heart went into fibrillation.

Breathing rate. The resting breathing rates of three sub-adult otters after two to three weeks captivity are given in Table 2. There

TABLE 2. HEART AND BREATHING RATES OF RESTING OR SLEEPING CAPTIVE SUB-ADULT SEA OTTERS.

Sex	Heart Rate Per Minute No. of observations	Range	Average	Breathing Rate Per Minute No. of observations	Range	Average
Female	3	144-148	146.7	12	9-16.5	12.2
Female	3	122-136	129.7	9	8-14	10.6
Male	1	121	121	8	12-15	13.6
Average ..			132.4			12.1

are two distinct breathing movements without regular rhythm; the more frequent shallow breath is mainly thoracic while the deeper ventilation is more abdominal in nature. The intervals between shallow breaths varied from 1 second to 15 seconds in the same animal during the same period (several minutes) of observation. This irregular rate was seen in animals both asleep and awake.

Breathing rates of 16 per minute and 22 per minute were recorded on animals shortly after capture. Several otters were observed to pant for short intervals at the time of capture.

Passage of food and digestion. The sea otter masticates its food very thoroughly. Most bones and pieces of skin are completely ground before swallowing. The shells of smaller mollusks are also ground and swallowed, but larger shells may be cracked and discarded. In timed experiments the shells of mollusks included in the food of the sea otters appeared in the feces from 2 hours 45 minutes to 3 hours 15 minutes after ingestion.

The feces of otters on a diet entirely composed of fish were not formed but quite watery. Only a small percentage of the total bone ingested appeared in the feces, these being mostly head bones, ribs, and fin rays. All other parts of the fish were completely digested. The shells and chiton of invertebrates did not appear to be digested at all and the feces of animals receiving this type of food were semiformed and less watery.

When goose and seal flesh were included in the captive otters' diet, considerable amounts appeared in the feces apparently completely undigested. This was particularly true if this food was provided in chunks, though even when the meat was ground it could be recognized in the feces. When fish and meat were ground together, the fish was never recognizable in the feces, but the meat often was.

When three otters received about 21 pounds of fish in a 24-hour period, about 3¼ pounds of fecal material were produced during the same period. When the otters received insufficient food or were fed at intervals exceeding 10 hours, the feces became black and tarry (blood). Unless adequate food was provided immediately, death usually followed in a matter of hours.

Anesthesia. The first attempt to anesthetize an otter with Nembutal was fatal. A dose of 40 milligrams per kilogram injected intraperitoneally (the anesthetic dose for dogs) resulted in death of the animal in less than 30 minutes. Two other attempts, using half the dog dose gave good anesthesia. Undesirable after effects, as noted above, probably due in part at least to the anesthetic, contraindicated further use of anesthesia in light of the limited number of animals which could be handled.

Survival in captivity. Three animals were successfully maintained in captivity. For the first 10 days these animals lost weight and frequently appeared listless and in a weakened condition. During this time they were housed inside, protected from the weather, and denied the use of water except for drinking. They were fed approximately 2 pounds 4 ounces of fish and meat per animal per day (12 to 15 per cent of body weight). On several occasions they passed black tarry feces indicating that they were probably suffering from enteritis. After 10 days the rations of these animals were increased to about 7 pounds of fish per animal per day (25 to 35 per cent of body weight). Immediate improvement was apparent by no further signs of enteritis, weight gains, more activity, and more strength as they moved about.

These sea otters successfully withstood captivity, steadily improving, for three and a half months, including 6,000 miles of transportation by ship and air.

DISCUSSION

In many respects the sea otter represents an interesting intermediate stage in the adaptation of terrestrial mammals to an aquatic existence. This adaptation falls far short of that seen in true marine mammals such as Cetacea and Sirenia which not only have no requirement to leave the water but are so highly specialized that they are unable to do so. Most Pinnipedia, though they can go ashore for relatively long periods to rest and bear their young, show marked specialization to an aquatic environment. Their morphological and physiological adaptations are much more pronounced than those seen in the sea otter.

On the other hand the sea otter has largely lost the ability to survive on land in the natural state. It is unable to travel any distance or procure food out of the water as most semi-aquatic mammals such as the river otter and beaver are able to do. Morphologically the sea otter resembles these semi-aquatic animals more closely than it does the true marine mammals.

Because of the lack of adequate information about the life history, ecology, and physiology of the sea otter, it is difficult, if not impossible, to understand clearly its requirements and stage of adaptation between a terrestrial and aquatic existence. This adaptation may not be complete and the evolutionary changes which are in progress appear to have left the animal with a very narrow range of environmental tolerance. This might explain the slow recovery of the species from near annihilation even with complete protection for over 40 years, and the difficulties encountered in working with individuals in captivity.

On the basis of the work reported here, the causes for captivity mortality in sea otters stem from a combination of inadequate feeding and environmental stress. The lack of food does not usually result in a total energy deficiency as seen in chronic starvation, since many sea otters which died were fat; few, in fact, had no fat reserves. It is rather a case of acute starvation in which the mere absence of food from the digestive tract brings about changes in these organs which are intolerable to the sea otter. It is also possible that the sea otter is unable to mobilize stored energy resources (fat) rapidly enough to satisfy entirely its immediate needs under stress.

When the digestive tract remains empty for more than a few hours very rapid degenerative changes occur. Whether these changes result from the mere mechanical absence of material in the gut, a chemical attack on the lining of the gut, or the presence of some bacterial or viral organism is not clear. The rapidity of food passage through the long gut[4], and the histological evidence that the gut is very active, indicates that it is adapted to handle large quantities of food material almost constantly. When this material is not available there is apparently no mechanism by which it can be slowed down. A severe gastroenteritis then develops which leads to fatal shock or circulatory collapse.

The destruction of gastro-intestinal mucosa and loss of blood usually seen in this condition is adequate stimulus for the shock syndrome (Selye, 1950). In a few instances the enteritic condition did not seem sufficiently severe to have been the sole causative factor of shock. However, the excitement and stress of capture and early confinement is in itself a strong neurological shock-producing stimulus and would tend to aggravate the situation.

[4]The small intestine and colon together, from an adult with a body length of about 4 feet and a weight of 30.8 kilograms, measured 40 feet without undue stretching a few hours after death.

There is considerable evidence in the results from pathological animals to support this shock theory:

1. A hyperemic condition, indicating vasodilation, was observed at necropsy on both gross and histological examination.
2. A slightly higher moisture content of the tissues, congestion in the lungs, and free fluid in the body cavity indicated loss of fluid from the blood and edema of tissues.
3. Higher red blood corpuscle and hemoglobin values are an index of hemo-concentration.
4. Eosinopenia (reduction in eosinophils) is a well recognized symptom of acute stress.
5. The animals died very suddenly, showing few or no symptoms of morbidity up to a few hours before death, which always occurred in the early morning hours. The natural stimulations of nearby human activity during the day and evening would have a sympathomimetic effect on the animals, blocking the shock syndrome temporarily. When this stimulation was removed the animals would succumb to shock rapidly.

In addition, it is possible that the entire otter population has a latent, as yet undetected, infection of a bacterial or viral organism which causes enteritis. When the animals are subjected to environmental stress or are unable to get adequate food, this infection might become violent, and if it subsides, leaves the scar tissue found in the gut of all animals examined, both normal and pathological. Such a disease, and the toxins produced by it, might explain all of the symptoms noted.

In the early part of their confinement, the captive animals were not receiving adequate amounts of food. Judging from their condition and the presence of blood in their feces at that time, it was fairly certain that they suffered from attacks of enteritis from which they later recovered. Gastro-enteritis is a recognized symptom of stress. In nature, a limited food intake during rough weather may cause a sublethal environmental stress resulting in enteritis and the intestinal scarring without the presence of a disease organism.

The rapidity with which food material travels through the gut of the sea otter is probably the reason why certain foods such as seal and goose flesh are not completely digested. These substances are more resistant to mastication and digestion than fish. The digestive fluids would not have an opportunity to mix with and attack this type of food as readily as the fish flesh, which contains less connective tissue, in the short time it remains in the gut.

The metabolic rate of the sea otter was not determined. The high caloric intake of the otter per day would seem to indicate that it might be high; however, observation of the habits of animals kept in captivity did not bear this out. They display none of the nervous activity frequently seen in captive animals of other species. They did not pace and rarely moved about except at feeding time, spending most of their time sleeping or resting. Much of the time they were not even alert to their surroundings, being undisturbed by noisy human activity in their immediate vicinity. A high metabolic rate may be necessary to maintain body temperature, but it is difficult to assume that their heat loss is excessive through the fur that they possess, even if wet. The captive animals kept dry most of the time and showed no decrease in food requirements, even when the environmental temperature rose. Possibly the artificial environment of captivity results in loss of insulation and waterproofness of the pelt, which becomes dirty and, in places, matted.

The captive sea otter has a rather narrow range of temperature tolerance and is sensitive to abrupt changes in temperature. Captive animals held in a tank of water only a few degrees below the temperature of the sea shivered excessively and screamed in apparent discomfort. The inadequacy of the diet provided these animals probably accentuated their inability to cope with this condition; however, they all had considerable fat reserves at the time of death.

Animals held successfully for more than a week in a dry environment, which were then placed in water, shivered intensely and were seriously weakened by a very short exposure, taking 12 to 24 hours to recover from the experience. It is difficult to understand this reaction in animals which spend their lives in and out of the sea unless one considers the relatively narrow range of environmental temperatures encountered naturally by the wild sea otters. The sea temperature at Amchitka has an annual fluctuation of about 9 degrees Fahrenheit (38 degrees to 47 degrees) while the air temperature varies only about 40 degrees Fahrenheit (15 degrees to 55 degrees Fahrenheit). Individual animals may be subjected to much less variation than this depending upon their immediate surroundings. Changes within these ranges are relatively slow and would give the animal ample time to make slight seasonal adjustments necessary. The temperature of the sea in the winter may be at or near the lower limit the wild otters can tolerate and even a small temperature drop may cause severe stress, particularly if the animals are not in the best physical condition. The wild animal could compensate by hauling out to conserve its energy or by gathering more food as the situation requires. On the other hand, captives are subjected to more abrupt environmental temperature changes probably outside their natural range of tolerance, and their confinement prevents their making beneficial adjustments. Abrupt changes, therefore, constitute potential stress-provoking stimuli.

The obvious discomfort and distress of otters subjected to environmental temperatures higher than 60 degrees Fahrenheit, or when subjected to direct sunlight for more than a few minutes, indicate that they cannot withstand excessive heat any better than cold. The lack of panting, except following strenuous exercise, when it is very brief and apparently ineffective, indicates that this method of controlling body temperature is not well developed in the otter. Another indication of the lack of an adequate temperature control mechanism is the uncontrolled rise of body temperature in captive otters following anesthesia.

One of the first effects of high environmental temperature in most animals is a loss of appetite and refusal to eat. In the sea otter this is very serious and may result in the gastro-enteritic lesions discussed above.

Not enough data of comparative nature were obtained in this study to show definitely how the blood sugar and liver glycogen affect, or are affected by, captivity mortality. There seems to be a decrease in the blood sugar with captivity mortality, but whether this is associated with the causes of captivity mortality or merely a side effect is not established. The value for the blood sugar and liver glycogen of normal sea otters falls within the range of other animals.

The values obtained for the organ weights and their percentage of body weight are difficult to evaluate because of the wide range of size and age groups of sea otters worked with. The apparent increase in size of the adrenal gland of pathological animals supports the theory that stress or shock is a causative factor in captivity mortality since adrenal hypertrophy is one of the characteristic changes in the adrenal during stress (Selye, 1950). Evidence for adrenal atrophy in river otters (*Lutra canadensis*) held in prolonged captivity is reported by Christian and Ratcliffe (1952), but the changes possibly may be attributed to inactivity. In the pathological sea otters there were slight increases in the other organ to body weight ratios which may reflect the body weight loss during captivity.

The heart rate observed in sea otters is comparable to that of other mammals of similar size. The irregular characteristics of the breathing rate of the sea otter is similar to that observed in seals (Irving,

1939) and elephant seals (Bartholomew, 1954) and may be an adaptation to an aquatic existence.

Success in keeping sea otters in captivity depends largely upon adequate feeding and environmental temperature control. The importance of keeping the alimentary canal supplied with food has been discussed. Because of the rapid passage of food through the tract this requires more than one or two daily feedings. The ideal situation would appear to be four meals spaced at 6-hour intervals, although an interval of 8 hours at night does not seem to have a deleterious effect.

The quantity of food is also important. Enough must be given at each meal so that the stomach contains sufficient food material to be passed into the intestine over a period of hours following each meal. The total daily intake of food necessary to maintain body weight of the sub-adult sea otter is probably in excess of 25 per cent of the body weight of the animal. The food should be of a type that is readily digestible. The kinds of food acceptable are reported in another paper detailing the behavior of captive sea otters (Kirkpatrick *et al.*, in press). The Soviets earlier had reported success in maintaining sea otter captives (Malkovitch, 1937; 1938), but their reports were unknown to the present investigators until this project ended.

Maintenance of the animals in a dry environment was essential in the experiments reported here. However, if adequate food supplies were available, it would probably be possible to allow the animals at least limited access to water for bathing, particularly if they are protected from the weather. Air temperatures within a range of 30 degrees to 60 degrees Fahrenheit, appear to be satisfactory. How much lower the air temperature may be allowed to drop before it has an adverse effect is not known. Water temperature should not be below 40 degrees Fahrenheit.

Evidence for environmental stress occurring in wild sea otters has been pointed out. When animals already suffering from such stresses are subjected to the additional stress of capture and captivity their chances of survival are greatly diminished. Since there is as yet no way of recognizing stress in wild animals, it is to be expected that some sea otters in this condition will be taken captive and subsequently die, regardless of the care given them.

Individual variations in temperament may also be a source of failure of animals to survive in captivity. Some animals, although they outwardly appear to accept captivity placidly, remain nervous and uneasy. If they persist in this attitude, refuse food, or stop eating after a few days, they are doomed.

Excessive handling, abrupt temperature changes, change of diet, isolation, and unusual stimulations should all be considered potential neurological stress-provoking stimuli capable of directly or indirectly precipitation captivity mortality.

ACKNOWLEDGMENTS

The authors wish to express their appreciation for the interest and cooperation of the U.S. Fish and Wildlife Service, Branch of Wildlife Research, Washington, D. C.; and the Alaska Regional Office, U.S. Fish and Wildlife Service, Juneau, Alaska. We also wish to acknowledge the encouragement and assistance of Captain G. S. Coleman, USN, and the U.S. Navy at Adak, Alaska; and of Dr. Robert Rausch and the U.S. Public Health Service, Arctic Health Research Center, Anchorage, Alaska.

We are deeply grateful for the moral, physical, and intellectual support given us by the other members of the team which collected all the material and data at Amchitka Island. Mr. Ford Wilke, Biologist, U.S. Fish and Wildlife Service; Mr. Robert D. Jones, Jr., Refuge Manager, Aleutian Islands National Wildlife Refuge, and his staff; David Hooper, Calvin Lensink, and William Golly, were indefatigable in the face of adverse and often hazardous climatic conditions in their efforts to facilitate the work.

We are also indebted to our colleagues Drs. L. P. Doyle, G. M. Neher, R. M. Claflin and M. X. Zarrow of Purdue University for assistance in processing and evaluating the materials collected.

SUMMARY

1. During February and March, 1954, an exploratory physiological investigation was conducted at Amchitka Island primarily to determine the causes of captivity mortality in the sea otters.

2. Twenty-two sea otters were studied including seven normal animals (six shot from ambush), 12 pathological animals that died in captivity or on the beach, and three that survived.

3. Necropsies on all dead animals and appropriate laboratory procedures were performed for study of blood components, liver glycogen, tissue moisture, organ weights, gastro-enteritic activity, and histological structure. Observations were made on living animals for temperature regulation, breathing, and heart rate.

4. Normal values were established for RBC, WBC, and eosinophil counts; hemoglobin concentration; blood sugar; liver glycogen concentration; tissue moisture; organ weights; body temperature; heart rate and breathing rate and pattern.

5. Comparison of results from normal animals with those dying in captivity showed that the most remarkable difference was a severe gastro-enteritis in the latter group. Other differences seen in captivity mortality were a distinct eosinopenia, vasodilation, and a slight hemoconcentration and tissue moisture content.

6. The captive sub-adult sea otter has a high food intake requirement of 25 to 35 per cent of body weight per day necessary to maintain body weight. Food passes rapidly through the long alimentary canal in about three hours. There is also evidence that the sea otter has a narrow range of environmental temperature tolerance.

7. Results of this work indicate that captivity mortality in the sea otters studied was probably due to an acute stress or shock reaction. This is characterized by a rapidly developing gastro-enteritis which is precipitated by insufficient food intake, intolerable environmental temperatures, and adverse sensory stimuli.

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DISCUSSION

DISCUSSION LEADER SARGENT: I wonder if I could ask you one question that is really not perhaps applicable to your paper. I understand that the sea otter was harvested in great quantities at one time and is now being strictly protected. I wonder if you would care to comment on the size of the population now, and whether the population is increasing or whether it is continuing to decrease.

DR. KIRKPATRICK: I know less about the population of sea otters than Dr. Buckley here at the table with me. It is my understanding that around some islands, particularly Amchitka, the population has been on the increase in the last few years. It is also understood they are nonmigratory. It is the idea, or one of the early purposes, to try to relocate some of the sea otters. In so doing the animals died in captivity, and this was the reason why we got involved in this thing.

DISCUSSION LEADER SARGENT: Thank you very much.

Are there any questions from the floor:

MR. R. W. WOLFGANG [McGill University]: Recently Schiller in Alaska has reported on the parasites of the sea otter, and he has reported the *Porrocaecum* in the stomach which he suggests causes gastroenteritis in the sea otter. We have the same parasite in seals along the Northeastern Atlantic Coast. Although we sometimes find a gastroenteritis, we have not necessarily associated it with this *Porrocaecum.*

I wonder if Dr. Kirkpatrick has found this parasite in the sea otters and what he thinks about the helminth diseases causing gastritis.

DR. KIRKPATRICK: We were aware of a rather major paper by Dr. Rausch on ecology on parasites in sea otters. He examined all the otters thoroughly and collected all the parasites found. I might say that in quite a few of the otters that died on our hands and also that died on the beach, we did not find any evidence of porrocaecum. We did not have any idea whether they were infected with *Microphallus pirum,* a small trematode in the intestine. It was at least our idea that the animals we were working with were not dying of parasites.

DR. M. J. DUNBAR [New Brunswick]: It sounds as though the sea otter is altogether too nervous an animal to go to sea in the first place. I wonder if Dr. Kirkpatrick could tell us anything about similar effects on land otters and fresh water otters of this sort.

DR. KIRKPATRICK: So far as I know, nothing like this has been done on land otters.

DR. V. D. VLADYKOV [Quebec]: I would like to make a remark. In the Paris Museum magazine was an article similar to this which showed evidence of things like this as a cause for death in the sea elephant. They tried several things and eventually saved them by administering drugs. I think it is interesting to make the comparison. I could send you the exact reference. It was a bulletin of the Paris Museum. The causes were similar as probably in the cases of your sea otters. They referred also to gastrointestinal disorders. He referred also to stress. He eventually succeeded in saving half of six or eight animals.

DR. KIRKPATRICK: Gastroenteritis, as I mentioned in the paper, is a very definite symptom of stress and it is not unlikely that it may occur in any species of animal which is subjected to the condition of stress which is a change from the normal resting state of the animal's body.

It would be very much appreciated if I could have that reference, sir.

GROWTH RATES AND AGE DETERMINATION IN ALASKAN BEAVER[1]

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Although it is generally recognized that animals do not grow at uniform rates throughout their lives, or indeed, throughout a single year, exact rates of growth are not known for most species. Recognition of this differential seasonal growth is implicit in several of our techniques for aging animals, *e.g.* the use of growth lines on the scales, otoliths, vertebrae and bony fin rays of fish, annuli of sheep and goat horns, growth ridges on the claws of seals and in the baleen of whales, and growth layers of the teeth of seals, all of which are permanent records of varying growth rates (Scheffer, 1950; Lawes, 1952). Although the reasons for differing growth rates probably vary among the several species, the occurrence of varying growth rates is widespread.

In the course of management investigations of beaver carried out since 1950 in interior Alaska, we have accumulated a considerable amount of information on beaver growth. The paucity of published data on beaver growth rates and the unsatisfactory criteria for determining age of beaver lead us to publish our observations, although our data are satisfactory for only the first two years of beaver life. The data presented are all from beaver that were live-trapped, weighed, and marked as kits[2] or yearlings, and subsequently recovered as known-age animals (except for nine kits and three yearlings that were taken by dead-trapping and had not previously been marked). To extend the usefulness of the growth data, age-weight, weight-pelt size, and age-pelt size relations were computed. The latter were then applied to the pelt measurements from the past four years as an aid in assessing the effects of harvest regulations on beaver populations. In addition, the usefulness of skulls in age-determination is discussed. Although some of the applications set forth in this paper may apply only to Alaska, we believe that some of the methods described will, with minor modifications, be found useful throughout much of the range of the beaver, and that the weight-pelt size regression may be a parameter that will hold for most beaver.

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[1]Contribution from the Alaska Cooperative Wildlife Research Unit: the Wildlife Management Institute, U. S. Fish and Wildlife Service, University of Alaska, and Alaska Game Commission cooperating. The study was undertaken with Federal Aid in Wildlife Restoration funds under Pittman-Robertson Project W-3-R. the Alaska Game Commission.

[2]In this paper animals up to 12 months old are termed kits; those between 12 and 24 months, yearlings; 24 to 36 months, two-year-olds; and more than 36 months old, adults.

GROWTH

Hakala (1952) was, to our knowledge, the first observer to report a step-like pattern of weight increase in beaver, with rapid growth during the summer season and slow growth during the winter. By combining Hakala's data from Alaska with weights we have obtained subsequently, we have been able to compute linear regressions of weight on age for the first 24 months of beaver life. These four regressions, based on 105 animals weighed a total of 164 times, are presented in Figure 1. Forty-one weights were used in computing the regression of summer growth of kits, 19 weights for the winter

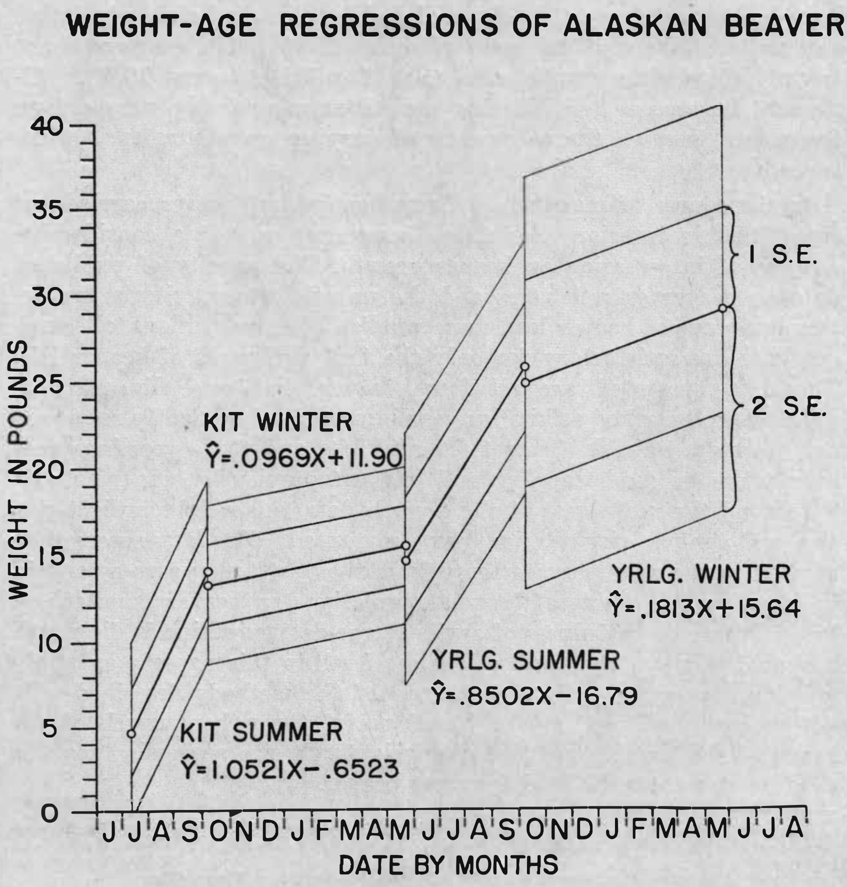

Figure 1. Weight-age regressions of Alaskan beaver.

growth of kits, 93 for summer growth of yearlings, and 18 for winter growth of yearlings. Seven weights that occurred at the beginning or end of the winter season were used in computing both the summer and winter regressions. For purposes of computation, the weights were grouped by 10-day periods, with the date of birth for all beaver assumed to be early June. [Cook and Maunton (1954) have shown that the period of parturition extends from late March to November in New York, but 96.4 per cent of the births occurred from late April to early June. It is probable that the period of parturition in Alaska is more variable than we have considered it, but, because of the more regular and pronounced seasonal changes in Alaska, more restricted than in New York. At any rate, the error introduced by our assumption is not large.] Since the data for the winter periods are few, the regressions of summer and winter growth do not quite join. The limits of one and two standard errors of estimate are shown on Figure 1 and represent respectively the bounds within which approximately 68 and 95 per cent of the weights would be expected to fall.

Our data are too few to permit the computing of regressions for older beaver, but there are indications that growth continues in the same manner until sexual maturity is reached at the age of about 33 months, at which time growth slows down markedly. In three-year-old and older beaver the pattern of seasonal variation of weight is apparently still present, but the amplitude of the fluctuations is much less than in younger beaver and there may be little or no net gain in weight in a 12-month period.

It is evident that growth rates do not change as abruptly as indicated by the regressions of Figure 1. Indeed, the growth probably could be represented best by a sigmoid curve for each year's growth, but our data are too few to permit the necessary computations. Despite the inaccuracies introduced by our method of handling the data, the pronounced alternation in growth between summer and winter is clearly apparent. Obviously beaver in interior Alaska grow much more rapidly during the season of open water (approximately May 15 to October 15) than during the winter months. In fact, during the first two years of life more than three-fourths of the growth takes place during the 10 open-water months. The rates of growth for kits and yearlings respectively, as indicated by the regression coefficients, are 10.9 and 4.7 times as great during the summers as during the winters.

That this pattern is not peculiar to Alaska is demonstrated by regressions of weight on age for 48 weights of beaver from New York.[3] However, the alternation of rates between summer and winter is less pronounced in New York than in Alaska. The regression coefficients show that kits grow 4.1 and yearlings 1.6 times as rapidly during the summers as during the winters. A comparison of the regression coefficients (Table 1) for the Alaskan and New York data reveals that Alaskan beaver grow faster during the summer and slower during the winter than do the New York beaver. Furthermore, Hodgdon and Hunt (1953) present data indicating the same pattern of growth for beaver in Maine. Unfortunately, their data are not published in such form that regressions can be computed and direct comparisons made.

The causes of this pattern of growth, and for its differing intensity between the northeastern United States and Alaska, are not definitely known. It seems probable, however, that the difference in the lengths of the ice-free seasons has a strong influence. In Alaska, beaver begin their food caches in August and complete them by late September; in New York, the caches are not begun until mid-September and often are not completed until late October. Spring "breakup" is in mid-May in Alaska and mid-April in New York. Thus the period during which beaver must subsist on stored food is about two months longer in Alaska than in New York. Furthermore, winter thaws in New York permit beaver to cut fresh food at intervals throughout the winter, whereas beaver in Alaska are seldom able to feed above the ice. Some light is shed on the influence of open water by the history of one colony of beaver located in a "warm water slough" not far from Fairbanks, Alaska. The beaver from this colony could, and did, cut fresh food in the coldest part of the winter when most beaver were completely confined beneath the ice. Furthermore, their food cache was more readily available, because of thinner ice. Two yearling beaver from this colony increased in weight from 24 and 25 pounds in mid-August, 1953, to 36 pounds in early March,

TABLE 1. REGRESSION COEFFICIENTS FOR NEW YORK AND ALASKAN BEAVER GROWTH.

Age and Season		Alaska	New York
Kits	Summer	1.0521	0.7294
Kits	Winter	0.0969	-0.1798
Yearlings	Summer	0.8502	0.4143
Yearlings	Winter	0.1813	0.2570

[3] W. L. Webb of the College of Forestry, State University of New York, kindly provided a number of weights and pelt measurements from the Adirondack region of New York. The senior author also gathered some information from the Adirondack area while he was a graduate student at the same institution.

1954, a net gain of 11 and 12 pounds. The expected gain for this period would be only about 4 pounds.

Other possible influences are differences in amount of light, resulting from both confinement beneath the ice and latitudinal differences in day length; differences in energy requirements due to colder temperatures in Alaska; a greater leaching of nutrients from food stored for longer periods; and unavailability of food caused by formation of thick ice around the food cache. Seasonal changes in growth rates also may be in part genetic, since seasonal growth rhythms of unknown cause occur in several species including humans (Brody, 1945:222-230).

AGE DETERMINATION FROM PELTS

The step-like pattern of growth suggests the possibility of aging beaver by weight. Using all information available, we have established tentative weight-age classes for beaver taken during the trapping season. They are: Kits, 10 to 21 pounds; yearlings, 22 to 31 pounds; two-year-olds, 32 to 40 pounds; and adults, 41 pounds and over. There is little overlap between kits and yearlings; as the animals become older, individual weight variations become greater and doubtless the overlap is greater. In order to have a criterion for estimating ages of beaver from pelt sizes,[4] we computed a curvilinear regression of pelt size on weight based on 93 beaver for which weights and pelt sizes were available (Figure 2). This regression may well be a parameter for most beaver, since the data from New York and Maine agree with it quite well. By projecting the above age-weight classification on the resulting curve, age-pelt size classifications have been established. The ages established from this age-pelt size classification coincide with peaks in the pelt size frequency distribution for the Territory when plotted by one-inch groups, and with pelt sizes for know-age beaver. We have not yet been able to establish the degree of overlap in pelt sizes between ages, but at least in the lower age classes it is probably compensatory. Cook and Maunton (*op. cit.*) are doubtful of the validity of pelt size as a criterion of age in New York beaver, principally because of variability in methods of handling raw beaver pelts. However, in Alaska practically all beaver pelts are handled in the same manner, and we may rest assured that every last one is stretched to its maximum! Moreover, the relatively strict regimen of seasonal change in interior Alaska probably tends to impose a rather uniform growth rate as compared to that in most of the United States.

[4]Beaver pelts are stretched round. The pelt measurement is taken on stretched pelts, and is the sum of the length plus the width, in inches.

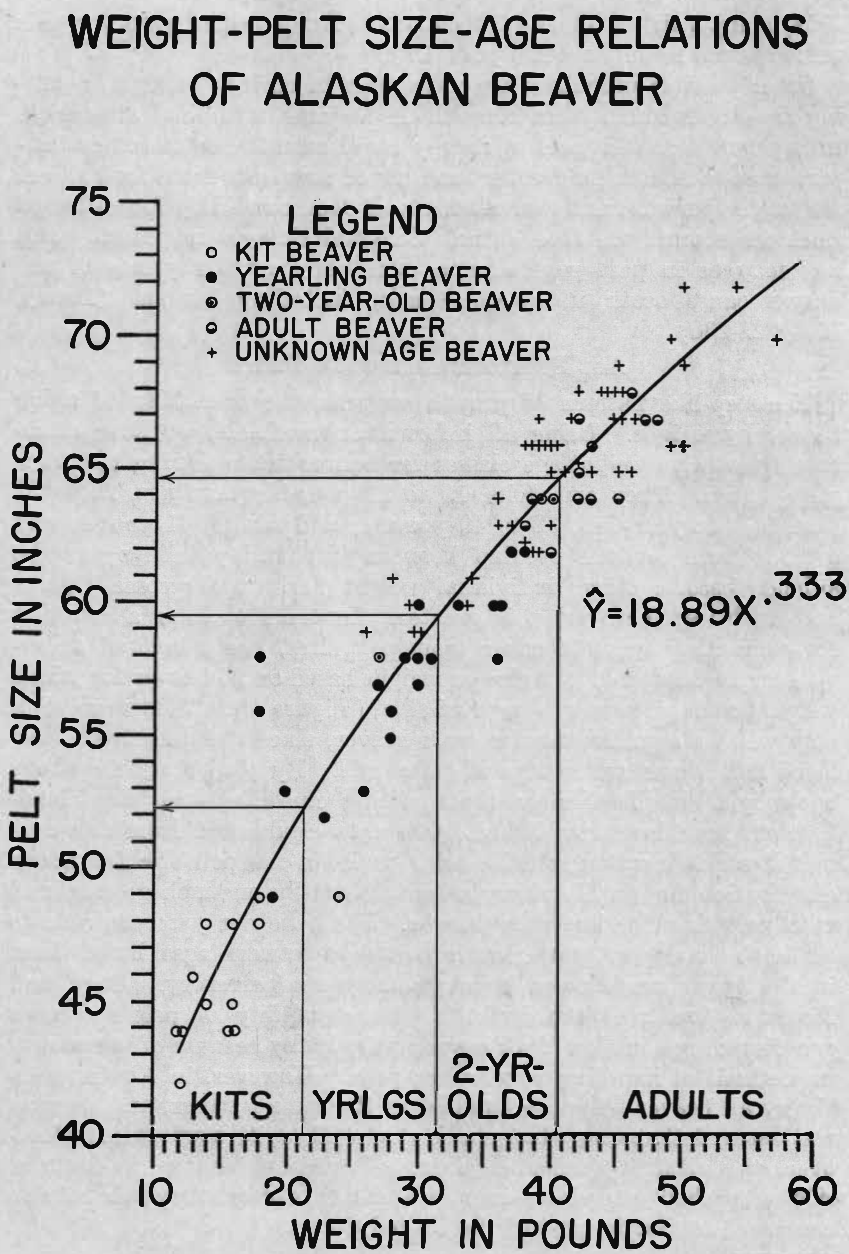

Figure 2. Weight-pelt size-age relations of Alaskan beaver.

APPLICATION OF PELT SIZE-AGE DATA TO MANAGEMENT

Age determination is not an end in itself; however, proper interpretation of age data does provide considerable insight into the condition of a population. The use we have made of our age-determination method is outlined in the following paragraphs.

By obtaining pelt measurements annually we can determine the percentage of the beaver harvest that falls in each age class. This has been done for the past four years for the Koyukuk Fur Management Area (an area in west-central Alaska of 13,300 square miles, roughly equivalent in size to Massachusetts and Connecticut combined). It has also been done for other sections of the Territory and pelt measuring will be continued and expanded. The percentage of pelts in each age class, based on pelt measurements, for the Koyukuk Fur Management Area, are included in Table 2, with data from other areas included for comparison.

The age composition of the beaver harvest as determined from pelt measurements is not in itself sufficient to estimate the condition of the population from which the harvest was drawn. The pelts measured are in no sense a cross section of the beaver population, nor are they even the equivalent of the "catch curve" of fisheries management for two reasons: First, the harvesting is selective for large animals, but there is not the uniform lower limit which a gill net or similar device provides. Second, the smaller beaver are often retained by the trapper for use as parka and moccasin trim without being reported in the harvest; hence, they are not available for measurement. These two variables are further influenced by current fur prices, the economic situation of the area concerned, and the maximum legal limit in effect at the particular time and place.

TABLE 2. AGE CLASSIFICATION IN PER CENT OF BEAVER HARVEST FROM SELECTED AREAS (Based on Pelt Size).

	No. of Pelts Measured	Percentage Kits	Percentage Yearlings	Percentage Two-Year-Olds	Percentage Adults	Percentage Measured of Total Taken in Area
Koyukuk Fur Management Area						
1951	512	28.7	16.0	24.0	31.2	48.5
1952	665	17.9	23.5	25.3	33.4	77.5
1953	1,056	14.7	23.5	25.9	35.9	72.7
1954	913	11.2	15.1	29.0	44.8	66.6
Upper Tanana Fur Management Area						
1951	66	39.4	4.5	18.2	35.3	16.7
1954	125	18.4	16.8	24.8	40.0	49.8
Fur Reporting District 11						
1954	1,130	33.9	25.0	19.3	21.8	83.0

Theoretically an absence of younger age classes in the harvest would indicate a declining population. Under conditions in Alaska it often indicates quite the reverse; a preponderance of young animals usually means that there are not sufficient large beaver available in the area to fill the legal limits of the trappers, so the area is trapped more intensively and a higher percentage of the population is harvested. Thus we find it necessary to have information on the average number of beaver taken per trapper in order to properly interpret the age distribution in the harvest. Table 3 contains the data on trapper success for the Koyukuk Fur Management Area for 1951 through 1954, and for other selected areas in Alaska. By comparing data such as are included in Tables 2 and 3, we can evaluate the beaver population for any area. In general, a combination of a large percentage of the younger age classes in the harvest and a small average number of beaver per trapper indicates overharvesting, and a combination of a large percentage of the harvest being older animals and a high average number of beaver per trapper indicates a healthy situation or possibly underharvesting. The following three examples illustrate the use made of the foregoing data.

TABLE 3. TRAPPING SUCCESS FOR SELECTED AREAS, BASED ON RECORDS OF THE U. S. FISH AND WILDLIFE SERVICE. (Bold face type indicates a legal limit of 15 beaver per trapper, other figures are for areas and years with a limit of 10.)

	No. of Trappers	Number With Limit	Per cent With Limit	No. of Beaver	Average No. of Beaver Per Trapper
Koyukuk Fur Management Area					
1951	120	80	66.7	1,055	8.80
1952	128	100	78.12	1,178	9.20
1953	**120**	**58**	**48.33**	**1,448**	**12.07**
1954	**113**	**53**	**46.90**	**1,370**	**12.12**
Upper Tanana Fur Management Area					
1951	98	21	21.42	499	5.09
1952	98	17	17.35	524	5.34
1953	48	4	8.33	166	3.45
1954	53	2	3.77	212	4.00
Fur Reporting District 11[1]					
1950	223	57	25.5	1,308	5.86
1954	172	79	60.0	1,362	7.42

[1]Closed to beaver trapping during 1951, 1952 and 1953.

In the Koyukuk Fur Management Area, an area known from aerial surveys and other data to have a large population of beaver, the legal limit was increased from 10 to 15 beaver per trapper in 1953. Tables 2 and 3 show that this area has a high average number of beaver per trapper and a low percentage of the pelts in the younger age classes.

The Koyukuk Fur Management Area is in direct contrast to the Upper Tanana Fur Management Area in the vicinity of Tok where the beaver population is so low that the residents requested that the legal limit be reduced from 10 to 5 beaver per trapper. On the strength of this request an aerial reconnaissance was flown by U.S. Fish and Wildlife Service personnel, and the limit was decreased to five for the 1955 season. Tables 2 and 3 show this area to have an extremely low average number of beaver per trapper and a high percentage of the pelts in the younger age classes.

Fur Reporting District 11 also has a relatively low average number of beaver per trapper and a large percentage of the harvest in the younger age classes. This district is one in which the season was closed during 1951, 1952 and 1953, and, from the above evidence, probably will be closed in 1956. A smaller limit of beaver per trapper is recommended to reduce the harvest for this District, because a lower limit would lessen the incentive for trapping and would reduce the take of those that do trap. We could thus more nearly approach a sustained harvest from the area and increase the economic value in the long run.

The knowledge that beaver grow in the manner demonstrated also permits us to place the trapping season at any time the pelts are prime without appreciable loss of pelt size, because practically no increase in pelt size takes place in winter, at least in the far north.

AGE DETERMINATION FROM SKULLS

In the course of searching for reliable criteria for age determination, we have collected a series of 162 skulls, of which 18 are skulls from tagged animals. Measurements of zygomatic breadth, greatest length of skull, and incisor width were taken of all skulls, but these measurements permitted segregation only of the kits from the older skulls. However, qualitative characters of the skulls, including general size, proximity of the temporal ridges, and development of the sagittal crest seemed to separate the skulls taken during the trapping season into four categories, presumed to be kits, yearlings, two-year-olds, and adults (Figure 3).

The skulls of kits are distinctly smaller than those of the other groups, have no sagittal crest, and the temporal ridges are poorly developed and widely separated (15 millimeters or more at the anterior end of the interparietal bone). Yearling skulls have more distinct temporal ridges than kits, and the ridges are nearly parallel though becoming convex at the posterior ends, and are much closer together at the anterior end of the interparietal bone (approximately 11 millimeters); the sagittal crest begins to appear on some skulls. The sagittal crest has started to develop on all two-year-old specimens,

Figure 3. Skulls of kit, yearling, two-year-old, and adult beaver.

the temporal ridges converge toward the rear of the skull and are approximately 5 millimeters apart at the anterior end of the interparietal bone. The temporal ridges on adult skulls meet, or nearly meet, on the anterior process of the interparietal bone or anterior to it, and the sagittal crest is very pronounced. Intermediate stages are present in skulls of beaver taken during the open-water periods.

Cook and Maunton (*op. cit.*) state that they found no clearly defined peaks in any of the skull measurements they took on a series of unknown-age skulls. However, their chart of width of temporal ridges does have poorly defined peaks that may represent the age classes we have found. The more uniform growth of Alaskan beaver may account for the better segregation of ages we have obtained.

We have tried to assess the validity of our skull aging technique by applying it to known-age skulls, but since only 18 skulls are available as yet, we cannot set mathematical bounds of reliability. Of the 18 skulls, six were estimated to be from yearlings, five from two-year-olds, and seven from adults. Tagging records revealed that the six yearlings and the seven adults were aged correctly. Of the five skulls called two-year-olds, tagging records revealed that three were definitely from two-year-olds, one from a yearling, and one from a beaver tagged the previous summer and considered a two-year-old. The latter was definitely not a kit nor yearling, but might possibly have been an adult since our criteria for aging live beaver are not infallible beyond 24 months. The yearling erroneously called a two-year-old was from the "warm water slough" mentioned previously, and thus is considered atypical. It is also of interest to note that one of these two-year-olds was an extremely small specimen weighing only 27.5 pounds but the skull was easily placed in the proper age classification.

SUMMARY

Beaver in Alaska follow a step-like pattern of growth, with rapid growth during the relatively short five-month open-water period, and almost no growth during the rest of the year. The same pattern, but with less marked alternations in rates, occurs in beaver in New York. Regressions of weight on age, based on 164 weights of known-age beaver, are included.

The step-like pattern of growth, with little overlap between ages, at least in the early years, together with the close correlation that exists between weight and pelt size, permits the determination of ages from pelt measurements. The limits established are: Kits, less than 52 inches; yearlings, 53 to 59 inches; two-year-olds, 60 to 64 inches; and adults, over 65 inches. The harvest can be segregated into age classes on the basis of pelt measurements using the above limits, but the age distribution in the harvest is not a true indication of the population from which the harvest is drawn. Information on trapper success is used in conjunction with the information from pelt measurements to evaluate the condition of the beaver population.

A method of age determination based on changes with age in the proximity of the temporal ridges of the skulls is described. Alaskan beaver can be segregated into kits, yearlings, two-year-olds, and adults with an accuracy of about 90 per cent by use of the method.

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DISCUSSION

DISCUSSION LEADER SARGENT: Thank you, Dr. Buckley.

I wonder if you would enlighten us on the desirable size or age as respects the value of the pelt. Does it grow more valuable the larger the animal gets?

DR. BUCKLEY: Yes, there is a marked increase in value as the pelt gets larger, but the evident thing from our work so far indicates that by the time the animal has reached three or four years of age, the chances are, if it is in a good environment, it has reached about as large a size as it will get. We will get some pelts that will be 76", or even one that measured 84". That's a good-sized beaver pelt. I don't know how old the beaver was. By the time an animal has reached three or four years old, its pelt will be a blanket or a super-blanket in the terms of the fur trade, and its value will be doubled or tripled very often.

DISCUSSION LEADER SARGENT: Being associated with the commercial fisheries as I am primarily, we are trying, particularly as respects haddock, to try to get a fish that is the ideal market size, and I am now going to ask an incredibly stupid question. Is there any way of not catching the beaver until it reaches a certain size? I realize they are caught in a trap. I wonder if there has been any consideration given to that. Might they increase the value of the harvest by waiting until they get to a larger size?

DR. BUCKLEY: There has been some work done investigating that in a number of places. In Alaska some of the trapping is done using snares, and if the snare is set with a large enough loop, there is a certain selectivity. Also, the regulations prohibit the setting of the trap closer than 25 feet to a beaver lodge and that means since older beaver usually travel farther than the younger ones under the ice where the trapping takes place, the older beaver are captured first. Thus, in these catch curves which are not catch curves in the sense of fishery management, you will notice that where beaver were abundant, and there was a good population, the older beaver were predominant in the catch. So there is a possibility, but it is not 100 per cent.

DISCUSSION LEADER SARGENT: Thank you very much, Dr. Buckley.

MR. HARRY W. WALTERS [Newfoundland]: I would like to refer to that question. Anyone who has seen the film of the National Film Board on "Beaver in Canada" will note that in the James Bay region there is a selection of beaver by use of nets in trapping. I understand that the nets go down and the small beaver are taken and kept until the trapper take a larger one, and then the smaller ones are released so that there is actual selection of the size of beaver you want.

DISCUSSION LEADER SARGENT: Thank you very much.

Are there any other questions?

MR. A. W. ERICKSON [Michigan]: Dr. Buckley, would it be advisable to attempt to take a larger percentage of the yearlings and kits due to the fact that they are not a producing portion of the population in an area where the population is not at maximum capacity already?

DR. BUCKLEY: As I understand the question, where beaver have not filled all of the possible habitat, you wonder about the advisability of taking the young?

MR. ERICKSON: In an attempt to bring the production up.

DR. BUCKLEY: I think that would be cutting off your nose to spite your face. You would be eliminating a future crop by taking these animals before they had reached the age of breeding. I think that thing for one year might be beneficial. It might assure you of a larger crop right then, but you would not have the amount of replacement stock coming on to produce future beaver. Maybe you and I are talking about two different things.

MR. ERICKSON: I think we are.

DR. BUCKLEY: I think we have a little difference of opinion. I would like to discuss it with you later, if I may.

MR. ERICKSON: I would like to, too.

MR. HARRY W. WALTERS [Newfoundland]: Dr. Buckley, I wonder what you would figure would be the number of beavers per square mile in the population of Alaska.

DR. BUCKLEY: I can't give you that for any large area. We have one small area in the Chatanika River Valley that we follow quite intensively. In one stretch of 10 miles we have marked well over 100 beavers, and their harvest for the last two years has been between 65 and 75 animals a year. This represents a Valley not over a mile and a half wide, most of it narrower than that, so that the harvest has been around 70 beaver for not over a maximum of 15 square miles.

I think you will have to consider that a high one even by your northern Ontario or Quebec standards. We run aerial surveys on one area each fall, and I can give it better in terms of number per mile of stream and things of that sort, but I can't give it to you on a square-mile basis because the habitat is not that uniform.

UTILIZATION OF ATLANTIC HARP SEAL POPULATIONS

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The purpose of this paper is to summarize some of the results of recent research on harp seals (*Phoca groenlandica*) in the west Atlantic, and to indicate desirable steps for management of the resource in the light of present economic utilization.

DISTRIBUTION AND MOVEMENTS

The harp seal ranges from the Canadian eastern Arctic and east coast eastward to Novaya Zemlya and adjacent Soviet areas. There are three major populations which can be referred to as the Eastern, breeding in the White Sea; the Central, breeding in the Jan Mayen Island area; and the Western, breeding off the east coast of Canada (Fig. 1). The Western groups breed in two localities: the Gulf of St. Lawrence and the east coast of Newfoundland. The species is strongly migratory and occupies the southern area of pack ice for spring breeding, moving northward to occupy high Arctic areas of open water during the summer and early autumn.

The recent warming trend in Arctic waters has materially affected the distribution and movements of the harp seal, at least for the Western group. Summer distribution to the east of the Canadian Arctic for example 30 or 40 years ago was concentrated around the southwest portion of Greenland. With the extension of open water areas to the north, and of many species of food animals of the harp seal, it now ranges in summer north to Thule. This has largely contributed to a change-over in the economy of the people of southern

Figure 1. Polar projection showing the approximate range and breeding sites of the three major populations of the harp seal.

Greenland from one based on seal hunting to one based on sheep-farming (Dunbar, 1947). While this northward extension in range has changed the time of arrival of southward migrants at Belle Isle from November to January, it does not appear to have materially affected the timing of the whelping season.

The distribution and times of whelping and mating of the three major groups are compared in Table 1.

The northward movement in all groups begins with the formation of the moulting patches in the pack ice. These are composed at first almost entirely of adult males, which haul out in herds of various size immediately following the mating season. They are gradually joined by the immatures of both sexes and by the adult females, with

TABLE 1. COMPARISON OF WHELPING, MATING AND RANGE IN THE THREE MAJOR BREEDING GROUPS OF THE HARP SEAL.

	Whelping	Mating	Northern range
Western:	Late February-mid March	Mid March-early April	West coast Greenland to Thule; east coast Baffin Island to Devon Island; north Hudson Bay.
Central:	Late March into April	Mid April-early May	West coast Spitzbergen; east coast Greenland.
Eastern:	Mid February-early March	Last three weeks March	Northwest coast Novaya Zemlya; east coast Spitzbergen.

a sudden influx of the latter towards the end of April. The pack drifts south, so to make northward progress the animals periodically enter the water and move north for short distances before reappearing on the pack again. By the end of May, at the latest, all seals from the east coast of Newfoundland are north of Belle Isle.

Suitable ice does not persist long enough in the Gulf of St. Lawrence for complete moulting on ice. There is some evidence that the seals in this area complete the moult in the water, although the possiblity that some of them leave the Gulf to join the moulting groups east of Newfoundland and Labrador has not been eliminated.

EXTENT OF KILL

For over 200 years there has been commercial hunting of harp seals in the northwest Atlantic. It is not possible here to go into detail on its interesting history, which is given quite fully by Chafe (1923) and which is reviewed more recently by Colman (1937, 1949).

The white-coated pup is the chief target of the industry. Before World War II, almost 90 per cent of the catch was of pups. Now many older seals are taken as well, constituting up to 40 per cent of the take. The greatest annual kills took place between 1820 and 1860, when over half a million seals were taken in some years. The largest kill on record is that of 1831, when 687,000 seals were taken by some 300 ships and 10,000 men. Steam vessels had replaced sailing schooners by 1871, largely through the influence of Scottish sealers and whalers from Peterhead and Dundee, who operated in the northwest Atlantic between 1790 and 1910. For the next 40 years from 250,000 to 300,000 seals were taken annually. World War I reduced this kill by half.

World War II took most of the sealing ships out of action. There was relatively very little sealing from 1941 to 1946, none in 1943.

Since the end of the war a Canadian fleet of some 10 or 12 motor vessels has been developed for sealing. Many of these are based in Nova Scotia. Some have been built specifically for the job. In addition a Norwegian fleet of some eight to 11 vessels has joined in the sealing off Newfoundland and in the Gulf of St. Lawrence, largely resulting from Norway's inability to re-obtain from Russia the sealing concessions she formerly held in the White Sea.

The pups are taken on the ice largely in the first half of March. Scattered "beaters" (pups which have moulted the white natal coat and are taking to the water) are taken late in March and in April. Bedlamers (immature harp seals) and adults are shot on the ice while moulting during April and May. Virtually all pelts with adherent blubber, called "sculps," are removed on the ice and dragged or winched to the ship. They are stored in holds with ice. The newer vessels have refrigerated holds.

Of 10,200,000 seals taken between 1895 and 1946, 96 per cent were harp seals. The remainder were hooded seals (*Cystophora cristata*), which at the present time form less than 1 per cent of the catch.

The kill statistics for harps from 1895 on are divided into three

TABLE 2. AVERAGE ANNUAL KILL FOR THREE AGE GROUPS OF HARP SEALS FOR GIVEN PERIODS FROM 1895 TO 1954.

Period	Countries	Ships	Young	Adults and bedlamers	Proportion older seals	Totals
1895-1911	Nfld.	20	228,000	21,000	8%	249,000
1912-1940	Nfld. (Norway 1938-9)	10	134,000	25,000	16%	159,000
1941-1948	Nfld. Norway	9	24,000	22,000	48%	46,000
1949-1954	Canada Norway	22	161,000	68,000	30%	229,000

age groups: "Young" (pups of the year), "bedlamers" (immatures), and "old" (adults). Average annual figures for given periods, with the percentage of older (immature and adult combined) seals, are given in Table 2. These figures represent the kill by vessels on the east coast of Newfoundland. There is in addition an annual shore-based catch of harp seals in West Greenland of about 50,000 and in Labrador, Quebec and Newfoundland of about 15,000, of mixed age groups.

Figures for the kill in the Gulf of St. Lawrence are available from 1949. They are given in Table 3. These include the kill by vessels, by landsmen walking offshore on ice, and by nets taking southward and northward migrants along the North Shore of the Gulf.

ECONOMIC USES

The north Atlantic sealing industry produces three major commodities: oil, fur and leather. The pup is the most valuable source of

TABLE 3. TOTAL KILL OF HARP SEALS IN THE GULF OF ST. LAWRENCE FROM 1949 TO 1953.

Year	Hunting	Young	Immatures and adults	Totals Young	Totals Older	Proportion older seals
1949	Shore	16,000	11,000+			
	Ships	22,000	2,200—	38,000	13,200	26%
1950	Shore	......	12,500			
	Ships	21,500	1,000 ?	21,500	13,500	30%
1951	Shore	11,800	15,000			
	Ships	70,200	1,100 ?	82,000	16,100	16%
1952	Shore	5,000	8,200			
	Ships	51,700	7,100	56,700	15,300	21%
1953	Shore	19,000	14,800			
	Ships	4,000	Nil	23,000	14,800	39%
Average:				46,200	14,600	24%

all three. Values in all of these commodities are unstable, but in general it can be said that the oil represents 50 percent of the industry's total revenue, fur 25 per cent, and leather 25 per cent. It is difficult to indicate a figure for the Canadian sealing industry's worth. The over-all quantitative production varies from year to year, and there are large variations in the production itself, caused by variations in the age groups of animals taken. With such variation from year to year, the total average annual value for the past four years of sealing products produced by the Canadian industry is about \$363,000 for fur, and for all products, \$1,450,000. The value of Norwegian sealing in Canadian offshore waters brings these figures to \$575,000 and \$2,300,000 respectively.

The pup is the basis of the fur stock. A few other fur skins are produced from older animals, but their total value is negligible. The value of the fur of a harp seal pup varies according to whether it is a hairfast "white-coat" or a "beater." Neither category is true fur. The white coat of the pup is foetal hair. It is retained for two to three weeks and is fast only for 10 days or less. Yet the pup should be two or three days old before it is taken, as newborn animals have little value. The "beater" coat is the short, spotted hair coat which grows in under the white coat. It is similar to the hair coat of the older age groups, but is thicker and softer. The market values of both types of pelt are subject to wide fluctuations. The hair-fast white-coat was worth about double the hair-fast beater for the first few years after World War II. The market for hair-fast beaters has improved, however, and today the beater pelt has a slight edge in value on the white-coat pelt.

Important factors in determining the value of pelts, for both fur and leather production, are, of course, the skinning technique and the way in which the skins are handled before and after deblubbering.

Very slight damage indeed causes a 25 per cent reduction in value. The sculps are very susceptible to knife damage and to a peculiar spoilage known as "ice-burn" when the carcass is left to lie on the ice before sculping. It is a matter of minutes rather than hours, which accounts largely for the somewhat hurried nature of operations on the ice. The introduction of machinery for deblubbering after the sculps are landed was a great stride forward in increasing the value of the skins. There is also the problem of oxidation. Clean unoxidized pelts are sometimes worth as much as double the price of the oxidized variety. Much has been accomplished by the industry recently in developing anti-oxidation techniques.

With respect to oil production, the Canadian sealing industry has been at a great disadvantage compared to their Norwegian competitors. Since the end of World War II, fats and oils have been in brisk demand in Europe. This market is closed to the Canadian producer who must sell his oil in dollars. The total quantity of oil produced by Canadian sealing interests represents only an insignificant percentage of the world's total production of edible oils, but the Canadian market is very reluctant to absorb even this small quantity.

BIOLOGICAL RESEARCH

a. *Aerial photographic survey.* Intensive investigation on harp seal life-history on this side of the Atlantic began in 1949. Høst (1951) carried out probably the first tagging of northwest Atlantic harp seals. Rasmussen (1952) carried out investigations for the Norwegian government. The major study on eastern populations is that of Sivertsen (1941). It was considered of prime importance to gain some idea of the abundance of the animals. During March, 1950 and 1951, an aerial photographic survey technique over the whelping patches in the Gulf of St. Lawrence and off the northeast coast of Newfoundland was tried out with some success. A chartered DC3 aircraft was equipped with two vertical aerial reconnaissance cameras; an automatically operated 6-inch type and a 20-inch type for telephotos. After locating a whelping patch, its area was determined by flying the length and breadth at several points, at a known speed. Photography was carried out at from 200 feet to 3,000 feet altitude depending on light conditions, nervousness of the seals, and the regularity of their distribution on the ice. A strip was flown diagonally across the patch, with the automatic camera photographing every few seconds. The area of each exposure is known, and the total area of the exposures was determined. The adults and pups were counted (the latter from telephoto shots at altitudes of over 600 feet) and the density per square mile was calculated. From this and the total area of the patch (usually about 50 square miles east of Newfoundland) the total population of pups could be estimated directly. The pups stayed on the ice but an unknown number of adults were in the water under the pack. The proportion of visible adults to pups, however, serves as an index to the stage of whelping. Immatures are not present at all in the whelping patches. Adult males are nearby and begin to enter the whelping patches at least by mid-March.

Estimates of the total population (*i.e.* of adult and immature males and females) were made from the pup census. Photographic coverage was virtually complete in the Gulf of St. Lawrence. The pup production here was estimated to be 215,000. Photographic coverage on the east coast of Labrador and Newfoundland was only about 50 per cent, and visual estimates were combined with photography. The pup production was estimated to be 430,000. The total annual pup production of northwest Atlantic harp seals therefore is estimated to be about 645,000 These were produced by 645,000 adult females (twinning is rare enough to be disregarded). Reproductive success is about 80 per cent as suggested by net catches of southward migrant adult females (Fisher, 1952). The total number of adult females represented by the whelping patches therefore is 805,750. Since the sex ratio is about equal and there is no evidence of a particularly disproportionate kill of sexes, the estimated total number of adult seals is twice this, or 1,611,500—1,074,000 east of Newfoundland and 537,500 in the Gulf of St. Lawrence. Using age compositions of net catches of southward migrants in the Gulf (Fisher, 1952), and of the kill of adults and immatures from moulting patches on the east coast of Newfoundland, the proportion of adults in the total stock was calculated for the Gulf and the east coast, respectively (Fisher and Sergeant, 1954). This resulted in an estimate of 2,067,000 for the total stock of immatures and adults represented by the breeding population east of Newfoundland, and of 1,200,000 represented by the breeding population in the Gulf. The total stock of northwest Atlantic harp seals thus is estimated to be 3,300,00. The method is open to obvious sources of error, *e.g.* the visual estimates east of Newfoundland, and the fact that the age composition data were several years behind the aerial survey data and the population structure may have changed. Another aerial survey, concentrating entirely on the whelping patches east of Newfoundland, is being carried out at this moment as well as intensive sampling of net catches and of the moulting patches for concurrent age composition data.

b. *Reproductive data.* An accurate method of aging harp seals

(Fisher and Mackenzie, 1954) has greatly facilitated reproductive studies. A study of reproductive tracts of known-age animals has produced data on ages at sexual maturity and length of breeding life (Fisher, 1954). The development of the baculum and of spermatogenesis indicates that breeding maturity in the male does not take place before eight years of age. The males come into breeding condition well ahead of the actual mating season, which begins about March 17 and ends early in April. They go out of breeding condition very quickly after the end of March, which probably is an important factor in controlling the regularity of the whelping season.

A study of the ovaries and corpora lutea has revealed that sexual maturity in the female occurs over a range of ages. First ovulations have been observed in animals from five to nine years of age. The corpus albicans remains visible for up to five years. A study of adult females 10 years of age and under indicates that first ovulation takes place largely between the ages of five and eight, with a peak at six.

The maximum life span is well over 30 years, and the average length of life is over 20. Animals of both sexes in their twenties are sexually active. Very few senile animals have been collected.

Female southward migrants taken in early January do not show follicular development at that season until in the fifth year (though it is evident in younger ages during the mating season or shortly after). Southward migrants of both sexes increase in number by age class until in the fifth year. Apparently the stimulus to migrate to the breeding area is not fully felt until the fifth year, since many immatures, in fewer numbers with each successive year-class, lag behind in the Arctic. In the fifth and succeeding year-classes all seals migrate south.

Implantation is delayed in the harp seal for an unknown length of time but probably for about 11 weeks. The phenomenon in this particular case allows mating to occur at the only time of year when the adults are concentrated, and at a time when normal development would produce pups at an unfavorable time of year.

c. *Weight increase and fur fastness in pups.* A peak in weight of pups is reached 16 to 18 days after birth. During this time their weight increases from about 12 pounds at birth up to a maximum of 100 pounds at weaning time. Almost all of this is blubber, and the peak is followed by a decrease during the period between weaning and active feeding in the water. The white foetal coat remains fast in all pups up to a sculp weight of about 20 pounds, at which time the pup is about five days old. The percentage of fast-furred sculps then decreases steadily with increasing weight until, at a sculp weight of 53 pounds or more (two to three weeks of age) no fast-furred sculps are taken. The white coat moults by degrees, and usually has disappeared completely by March 30. The pups then are in the water a good deal and take the name of "beaters." The short hair coat possessed at this time, as was noted above, is at present a little more valuable than the white natal coat. They are, of course, scattered and harder to hunt at this stage.

MANAGEMENT NEEDS

At present there are no formal laws, international or otherwise, specifically governing the kill of harp seals in the northwest Atlantic. There are, however, annual conferences between government and industry in both Norway and Canada, and informal international conferences between government representatives of these two countries. Each year an informal agreement on starting dates for the hunting is made by them, to which the industry adheres.

Colman (1949), in reviewing the seal fishery up to 1948, concluded that there was little likelihood of danger from overhunting to the Newfoundland seal herds during the next 10 years. Certainly the kill statistics give ample evidence that the population is capable of maintaining itself at a rate of killing comparable to that in the 30 years previous to World War II (*i.e.* 159,000 per year, 16 per cent immatures and adults). In post-war years, however, the kill has increased to a level well over this. Not only is the kill of young more, but the kill of immatures and adults has almost doubled (Table 2). While for almost 20 years previous to 1912 there was an annual average kill of 249,000, only eight per cent of this was of immatures and adults. There has been almost a fourfold increase since then in the killing of these older seals in the moulting patches and there is no indication that it will lessen. This problem is more acute east of Newfoundland than in the Gulf of St. Lawrence where the total kill is relatively light. Most of the take of older seals in the Gulf is by shore-based nets. It is not practicable to reduce this kill since it involves local consumption of the seals by landsmen along the North Shore of the Gulf to whom seal netting is an important factor in the general living economy. It is not an expanding activity.

It must be remembered that the figures for the take of older seals east of Newfoundland represent only those recovered, not those killed. It is difficult to estimate the percentage loss of seals wounded, owing to variations in shooting efficiency, but in the past it has been quite high. A continuation of recent efforts to maintain a high standard of marksmanship with the use of high-powered rifles will minimize the loss of wounded seals.

The industry still stands or falls on its catch of pups, however. The far greater commercial value of the young, and the preference of all operators to secure cargoes of young, point to the obvious desirability of protecting the maturing and breeding stock. The time taken to reach sexual maturity has been found to be long; eight years for the male and from five to eight for the female. Much of the present breeding stock consists of animals born between 1925 and 1943. This stock, which was produced and which matured during periods of relatively light hunting pressure on immatures and adults, is gradually passing out of the picture. It cannot be expected that it will be replaced by a stock of equal size from animals which are under a hunting pressure twice as great during the period of maturation.

Most of the shooting of older seals in the moulting patches takes place during the month of April, although the Norwegian ships often continue shooting on through May. The proportion of adult females, as stated above, becomes high only during the last few days of April. A closing date of April 30 for shooting would allow good hunting during the latter half of April, yet would provide some protection for breeding females.

The present opening dates of March 5 for the Gulf of St. Lawrence and March 10 for the east coast of Newfoundland were set to allow the taking of some fast fur as well as of pup fat. On the whole whelping begins and ends a few days earlier in the Gulf, and we strongly suspect that the two whelping groups are distinct. Fast white-coat fur apparently is still wanted. The opening dates allow a continued heavy take of pups, while they are still concentrated and vulnerable. A closing date under this set-up is desirable to protect the breeding females.

FUTURE OF THE INDUSTRY

The sealing industry recognizes that in terms of the Canadian economy it will never occupy more than a modest place. The necessity of working within very narrow economic limits makes it imperative that the highest possible quality of products be delivered. The great emphasis in the past in harp sealing has been upon quantity rather than quality. The days of the great catches are gone, however. It is the responsibility of the industry to make their sealing captains and and crews realize that quality must come first. The industry is aware of these things and much has recently been accomplished both in Newfoundland and on the mainland. There is still room for improvement in the handling and treatment of sculps from the moment of kill. An awareness of quality can be fostered among the crews by uniformly basing their remuneration not on the quantity of seals brought in but on the market value A carefully sculped, unoxidized pelt would result in a higher remuneration than two damaged oxidized pelts, and the oil, moreover, would be of a higher value.

There remains a possibility of a sustained kill of young seals of the present order only if the industry recognizes that it must protect the stock that produces these young seals, and lavish the utmost care upon what is taken. If *complete* protection were given to maturing and breeding seals, the production of young seals would build up under the present order of kill. But once the breeding stock is seriously down it will take years to recover. It remains to be seen whether this challenge for rational utilization will be adequately met.

ACKNOWLEDGMENTS

I am grateful to members of the industry for information on the economics of the seal industry, particularly to Mr. E. A. Jonas of Christensen Canadian Enterprises, Ltd. Other investigators have shared in the collection of biological information, particiularly Dr. D. E. Sergeant of the Newfoundland Research Station of the Fisheries Research Board of Canada.

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DISCUSSION

DISCUSSION LEADER SARGENT: Thank you, Dr. Dunbar, for a very interesting presentation. When I was in Newfoundland a few years ago, I recall a good deal of discussion about the need at that time for a regulation as respects the harp seal and the difficulty of adopting regulations because of the international aspects that are involved. I wonder if you would care to comment on what regulations you presently have, and what hope there is for some sort of regulation that would perpetuate the seal harvest.

DR. DUNBAR: The only regulation that I am aware of at the moment is that there is a date before which it is illegal to kill the seals. It is in early March. It varies for the two different herds—March 8th or March 12th, or thereabouts, or the 5th or the 8th. It is in Dr. Fisher's paper. Dr. Fisher has recommended a closing date as well, and so far there is nothing down on paper internationally between Denmark, Norway, and so on concerning any other regulation.

There was a meeting in Copenhagen at the time that ICNAF met in 1952, in which the whole matter of the seal was discussed, and the Norwegians showed themselves to be very willing to cooperate. They are much fiercer on the herd than other nationalities are as a rule.

There is a gentlemen's agreement that Norway will no longer go north from the Newfoundland area after April and fill up their holds with migrating seals or with walrus in Davis Strait. The unspoken quid pro quo is that regulations on the actual sealing in Newfoundland will be taken easier for the time being. I think I am right in saying this and not treading on Dr. Kask's toes.

DISCUSSION LEADER SARGENT: The regulation regarding the opening of the season, then, applies only to the Canadians, or is it a gentlemen's agreement among other nations?

DR. DUNBAR: That is on paper. Right? That's the only thing we have on paper? The opening date, and it applies to everybody?

DR. KASK: Yes.

MR. CHARLES FREMONT [Quebec]: I would like to ask if the harp seal carries the *Porrocaecum*.

DR. DUNBAR: I think the answer is no. The *Porrocaecum* is not a common parasite in the harp seal. The harp seal has a very different habit of feeding from the harbor seal. It feeds on macroplankton. We have no evidence of any parasite host relationship between the harp seal and the harbor seal.

DISCUSSION LEADER SARGENT: Are there any further questions? I might ask one more myself. How is the fur of the pup used at the present time in the commercial markets?

DR. DUNBAR: I am on much less sure ground here. I think it is used largely for the trimming of ladies' coats and things, and for fur trimming on slippers, and that sort of thing. It is the most valuable item in the whole seal take, the skin of a white-skinned pup of two and a half to three weeks.

TECHNICAL SESSIONS

Wednesday Morning—March 16

Chairman: Allan G. Watkins
In Charge, Wildlife Management, Southwestern Region, U. S. Forest Service, Albuquerque, New Mexico

Discussion Leader: Randolph L. Peterson
Curator, Division of Mammalogy, Royal Ontario Museum of Zoology, Toronto, Ontario

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BIG GAME RESOURCES

AN AERIAL SURVEY TECHNIQUE FOR NORTHERN BIG GAME

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Introduction

The use of aerial surveys in determining the populations, herd composition, and annual increment of big game animals has become increasingly important in wildlife management. Pioneer work in the development of aerial survey techniques was done by Saugstad (1942) in North Dakota. Other workers have introduced modifications to fit their field conditions. Morse (1946) surveyed moose and deer in Minnesota; Hunter (1945), and Riordan (1948) described techniques for surveying game in mountainous country; and Sumner (1948) surveyed Dall sheep ranges in Alaska. The use of aerial protographs in wildlife management has been well reviewed by Leedy (1948).

Early uses of aircraft by the Department of Northern Affairs and National Resources were in counts of the bison in Wood Buffalo National Park by the Royal Canadian Air Force in 1931, and of musk-oxen in the Thelon Game Sanctuary by Clarke (1940), in 1936. In the latter case Clarke took advantage of an air search for a lost aircraft to observe game on systematic transects.

It was soon realized that aerial survey was the only practical means of obtaining big game population data in the vast regions of northern Canada. It was used extensively during the preliminary barren-ground caribou investigation in 1948 and 1949 (Banfield, 1954). Since that time mammalogists of the Canadian Wildlife Service have flown approximately 2,000 hours on aerial survey work in northern Canada. Northern species studied by the authors include: moose (*Alces alces*), barren-ground caribou (*Rangifer arcticus*), white sheep (*Ovis dalli*), mountain goat (*Oreamnos americanus*), musk-oxen (*Ovibos moschatus*), wolf (*Canis lupus*), and Atlantic walrus (*Odobenus rosmarus*). Fuller (1950) has previously reported an aerial survey of northern bison (*Bison bison*).

During our aerial survey work several innovations have been attempted. We have developed a technique which seems to be well adapted to northern conditions. A report on this work forms the basis for this paper.

FACTORS TO BE CONSIDERED IN AERIAL SURVEYS

The characteristics of aircraft best suited for aerial survey work have been well described by Crissey (1949), and Riordan (*op. cit.*). In order to cover great distances in northern regions it has been found necessary to use heavy aircraft with longer ranges than those of the aircraft generally used in the United States. It is desirable to have a range of five or more hours flying time. Frost shields on the windows are often needed to provide clear vision under winter conditions. It is necessary to carry a good deal of emergency gear. The types of aircraft found most useful for our work include: Cessna 180, De Haviland Beaver, and Noorduyn Norseman. It is also important that a window may be opened to permit the taking of aerial photographs.

The maps most commonly used in northern aerial survey work in Canada are the National Topographic Series, Department of Mines and Technical Surveys, scale eight miles to one inch (1:506,880). These maps are based upon aerial photographs and are available for the whole of Canada. Some areas on them are still relatively blank but most of them present topographic detail. A few units have elevation contours; most give spot elevations.

Generally speaking the best time to undertake aerial surveys is the winter. At that season big game species are concentrated in restricted areas of suitable range. The game is clearly visible against the background of snow and finds little cover among the bare, deciduous trees.

The tracks of various species can frequently be distinguished from the air. Wolf tracks tend to be in straight lines, but those of herbivores meander. Fresh tracks are often sighted before any animals have been observed, and may be used as a guide in locating them. Old trails, feeding holes, and beds in the snow provide proof that the area was previously occupied.

Most of our experience has been gained in working with the barren-ground caribou, which is gregarious and migratory in its habits. The population of this species is not continuous over the whole range, but the herds tend to occupy discrete ranges.

We have found that the best period for caribou surveys is in March and April. At that time the herds are migrating from the winter ranges in closely packed columns and have a tendency to travel on frozen lakes and rivers. This makes it comparatively easy to count them. The hours of daylight are lengthening in that season, and thus there is a fairly long day for flying. Also, the region which the caribou occupy is generally in the polar air mass, and clear, dry, cold weather can be expected. Visibility is good, and "bumpless" flying conditions, desirable for safety in low level flying, may be expected.

Early morning and late afternoon are good times to observe animals feeding. Caribou frequently bed down on snow-covered lakes on sunny afternoons, and they are easily counted from the air at these times.

Unfavorable weather conditions such as strong winds and snowstorms may force big game to take shelter in forests and thus become harder to see from the air.

FLIGHT PATTERNS

The flight track patterns used vary considerably depending on type of job, type of terrain, and species under study. Consideration should be given to the sampling adequacy of the census strips. If population data are required for areas of limited extent, or areas with definite boundaries such as game sanctuaries, then the flight tracks are laid out in a systematic grid to provide adequate coverage. Unfortunately, in aerial surveys of large wilderness areas the economic considerations may restrict the coverage.

In other investigations data are sought on big game populations over vast areas. Here different techniques are needed for the sake of economy. In our caribou surveys reconnaissance flights are made over the range to find areas occupied by herds. When a herd is discovered, the range is quartered to delimit the length and width of the occupied area at the time of the survey.

Moose are difficult animals to census because it is hard to see them from the air in their coniferous forest habitat. Moose populations may be concentrated in favorable winter ranges, such as old brulés and river bottoms. The aerial census of moose has been described by Edwards (1952), and DeVos and Armstrong (1954). Flook found that moose census data have greatest use in the determination of trends. Transect routes should be established over favorable winter ranges. These routes may be flown annually under comparable conditions and estimates of moose per square mile obtained. A comparison of the data over the years may be used to provide an estimate of the population trend. Coupled with kill statistics these data may give close estimates of populations, as demonstrated by Riordan (*op. cit.*).

A special technique is required for the aerial survey of white sheep and mountain goats. These species inhabit discontinuous winter habitats. It is necessary to plan a flight path to cover the occupied ranges of known bands. In this case census consists of enumerating local bands on their winter slopes.

Big game animals are more easily observed on the tundra. This makes it advantageous to try to cover the caribou migration about the time the animals are leaving the timber-line in spring. Musk-oxen, which inhabit the tundra, introduce their own particular problems. When stationary, they are easily confused with rocks. When they run, they bunch up so closely that it is very difficult to count them or identify calves. Because of our present lack of information on winter ranges, musk-oxen are more easily censused in summer when most of the population is concentrated in a few known lake, river, and coastal areas to browse on willows and birch. Here, however, tall shrubs may hide the animals when bedded down.

The altitudes at which the surveys are flown depend upon a number of factors. Generally our work is done between 200 and 1,000 feet above the ground. Within this range the higher altitudes are satisfactory for surveying the larger species and for over tundra and water. Survey of moose in forested areas requires low elevations. Eye strain, an important factor to be considered on prolonged flights, is eased considerably at an elevation of 1,000 feet.

The walrus is a marine animal, leaving no tracks by which its movements may be traced. As it is gregarious and occupies a discontinuous habitat, random sampling cannot be used in obtaining population estimates.

Taking advantage of its habit of hauling out on off-shore islands and peninsulas, Loughrey made ground counts of herds during his investigation of the Atlantic walrus in 1952-54. It was found that the most representative counts were obtained in July, when all age and sex classes are present. At this time the bulls spend much time on land. Cows with calves or yearlings normally stayed on shore when the water was rough. The period from mid-morning to mid-afternoon appeared to be the best for accurate counting, as the walrus fed in the sea in the early morning and late afternoon.

During the ground work, Loughrey collected information for an aerial survey, which would make it possible to census all known hauling-out grounds within a radius of 100 to 200 miles within a few hours. Observation and careful questioning of the Eskimos provided information on the locations where the walrus were known to haul out year after year. These locations can often be spotted at a considerable distance by the darker colour resulting from the deposit of excrement and the accumulation of hair in crevices. It was also found desirable to ascertain the location of the pack ice. The walrus leave the land for the ice when it comes close to the hauling-out grounds, and spread out over a wide area where they are difficult to census.

The hauling-out areas located were marked on the pilot's map, and a flight course was laid out to cover all of them as economically as possible.

The walrus has poor eyesight but appears to react strongly to the noise of low-flying aircraft. The best counts were obtained at 1,000 to 1,500 feet elevation. At these altitudes it was often possible to make a second run before the walrus took to the water. Small herds seemed to take alarm more quickly than large ones.

Considerable experience in ground work is necessary with this species to give the observer confidence and competence before he attempts to estimate from the air closely packed herds which may number 1,000 or more. Aerial photographs, as yet untried, should provide a check on visual estimates. The use of telephoto pictures should permit age and sex group segregation, which was impossible visually.

We have found that aircraft at moderate heights do not generally alarm moose, caribou, and musk-oxen which seldom take alarm until they hear the propeller tip noise as the aircraft passes. Often one has the first glimpse of a moose as it jumps from its bed directly opposite the wings of the aircraft.

Sumner (1948) and Edwards (1954) have tested the accuracy of aerial surveys by ground checks. Both report that aerial counts are about 20 per cent low. Our field experience tends to confirm this figure.

Figures have been given on the cost of aerial census in the United States. Morse (*op. cit.*) reported a cost of $1.09 per square mile in Minnesota. Saugstad (*op. cit.*) reported a cost of 60 cents per square mile. Costs in northern Canada are greater than this because aircraft hire is much more expensive in remote areas. Our survey work in the Northwest Territories costs in the order of $4.00 per square mile for aircraft hire alone.

DETERMINATION OF TRANSECT WIDTH

One of the basic problems in aerial census is the determination of the width of the transect. In the preliminary caribou survey in 1948-1949, Banfield, (*op cit.*) developed a system to estimate transect widths. He plotted the flight course on the map. From time to time he also plotted the locations of the caribou seen farthest from the aircraft. Since the caribou were generally on frozen lakes, it was an easy matter to pin-point them. Later the average transect width was determined from the individual measurements. This system had the advantage that the altitude of the aircraft was not required for the calculation. One of the disadvantages was that some areas surveyed had not been adequately mapped. It was impossible to plot observations when flying over these blank areas.

Caribou and musk-oxen in open country are often visible from the air for distances which allow transect widths far greater than in forest. Accurate estimation of sighting distance is difficult or impossible through map reading because water margins do not stand out sharply at low altitudes except in areas of rugged topography. Kelsall has often checked sighting distances by turning the aircraft to point directly at animals seen. The distance is computed from the known groundspeed and the time required to fly between the point of sighting and the point of sight. This technique has value when it is not possible to measure the distance mechanically.

As another aid to visual estimation of strip widths in open country, sighting tests have been conducted with groups of caribou whose position could be accurately plotted on one-mile-to-the-inch flight maps. It was found that bands of caribou could be seen and recognized at distances up to 3 miles at altitudes from 500 to 1,500 feet, but that individuals and small groups could easily be missed at 1½ miles. Accurate counts could only be secured at distances under 1½ miles and conditions for using the maximum possible sighting distance are rarely found.

After much experimentation it was decided to adopt the system which uses an angle of sight to determine transect width, as originally described by Saugstad (*op. cit.*) and recently used by Edwards (1952). In this method the observations are limited to a definite field, whose outer margin is marked by guides—one on the observer's window and another on the strut of the aircraft. This marks an arbitrarily chosen maximum angle of sight. The transect width is calculated from the tangent of the fixed angle and a fixed altitude.

We have found that this system has several disadvantages. When aircraft hire is expensive and big game sparsely distributed, it goes against the grain to neglect observations beyond the arbitrary angle. It is difficult to find the angle where efficiency in observing big game drops off. This varies with altitude, terrain, visibility, and species. Also it is often difficult to fly at a constant altitude.

A correction should be made for the blind spot under the aircraft in many cases. This is needed if the observers are using only side windows. Even forward vision from the co-pilot's seat may be inadequate to cover this area if the aircraft flies in a nose-up attitude as the Norseman does.

We have found it preferable to record individual angles of sight to the animals observed and use the data to calculate the transect width. A line is drawn on the observer's window with a grease-pencil. Using this reference line, markers are placed on the wing strut to indicate angles of 20, 30, 40, 50, and 60 degrees from the vertical by means of a protractor, Abney level or Brunton transit.

A graph has been prepared which gives ground distances for various angles of sight at several standard altitudes. These data are presented in Figure 1. The graph may be used to quickly obtain effective transect widths under different field conditions. If two observers are used, one on each side, the transect width is doubled in making the subsequent calculations.

Several fundamental considerations in aerial surveys are revealed by a study of this graph. The narrowness of the transect width which may be covered at low level is emphasized. For instance, at an elevation of 100 feet, an angle of sight of 80 degrees (nearly horizontal) covers a transect only 570 feet wide. In order to cover effectively a transect one-half mile wide it would be necessary to fly at an elevation of over 1,000 feet.

It is also evident that at moderately low elevations a difference of a few degrees in sighting causes little change in a transect width. Therefore the recording of the angle of sight need only be approximate. It is advisable to restrict the observations to within an angle of

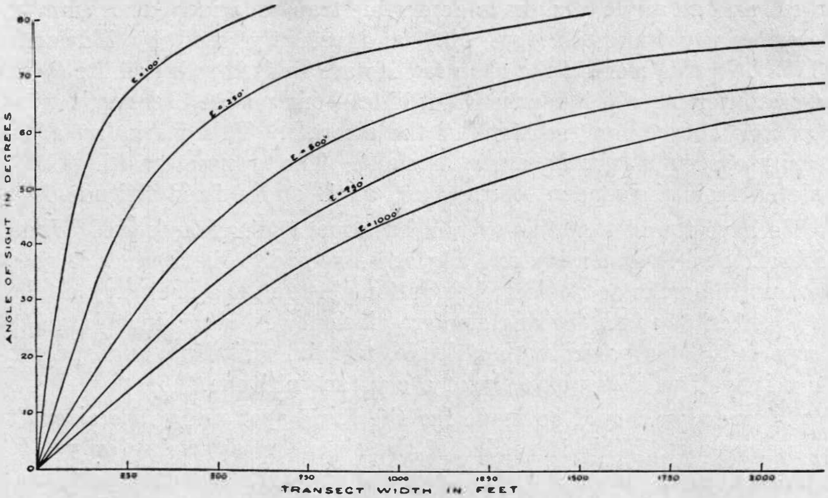

Figure 1. Transect widths for various angles of sight at five standard elevations: 100, 250, 500, 750 and 1,000 feet. Intermediate points may be estimated visually.

approximately 70 degrees, for the transect width increases rapidly to infinity beyond this.

Another practical consideration is the speed of the aircraft at low altitudes. The ground sweeps by so quickly that the sector which may be scanned effectively is greatly restricted.

ANALYSIS OF AERIAL DATA

Field forms have been prepared to record the observations. Flight data recorded includes: flight number, date, time of take-off, base elevation on altimeter, weather conditions, topographical features, time of observation, altitude, angle of sight, number and class of big game observed, and number of photograph if one is taken.

Care should be taken that the aircraft is on level keel when the angle of sight is recorded. This may be verified by glancing at the artificial horizon. If contours are not provided on the map in use, elevations may be obtained by landing and checking the altimeter.

The transect length is obtained by means of a map measure on the plotted route.

The distance of individual animals or groups from a point directly under the aircraft is then calculated as shown in Table 1. If the number of observations is small, full data should be used. If the number is large, a representative number can be used.

In order to ascertain the distances where efficiency in observing

TABLE 1. CALCULATION OF DISTANCES OF OBSERVED CARIBOU FROM PATH OF PLANE. DATA FROM AERIAL SURVEY OF CARIBOU IN NORTHERN QUEBEC, APRIL, 1954.

Altitude feet	Ground Elevation feet	Height feet	Angle of Sight degrees	Transect Width feet
2,700	1,600	1,100	60	1,900
2,700	1,600	1,100	60	1,900
2,400	1,600	800	50	900
2,100	1,600	500	50	600
2,100	1,600	500	50	600
2,100	1,600	500	50	600
2,300	1,600	700	60	1,200
2,600	1,600	1,000	60	1,700
2,600	1,300	1,300	40	1,100

the animals drops off so that animals are missed, the data are re-tabulated according to the distances from the aircraft. An example is shown in Table 2. An arbitrary distance interval may be used to illustrate the grouping of the observations. This method resembles the Kelker system for ground census.

It will be noted that most of the observations fall in the middle distance. Few animals are observed directly under the aircraft and beyond a certain point the number of observations drops off with increasing distance. It may be assumed that the animals were randomly distributed on the ground and that the flight track introduces no special bias. The grouping of observations indicates the efficiency with which the animals on the ground were observed and tallied. A scarcity of observations close to the plane indicates the blind spot, and the falling off of observations with distance indicates decreasing efficiency in counting. The effective transect width is the distance between, where observation appears to be efficient. In the example shown in Table 2, the transect width chosen was 1,600 feet (from 400 feet to 2,000 feet).

Only those observations which fall in the accepted transect width

TABLE 2. CALCULATION OF TRANSECT WIDTH BASED UPON DATA FROM AERIAL SURVEY OF CARIBOU IN NORTHERN QUEBEC, APRIL, 1954.

Distance from Flight Track Interval in feet	Number of Caribou Observations in Interval
0 - 200	1
200 - 400	0
400 - 600	5
600 - 800	6
800 - 1,000	3
1,000 - 1,200	7
1,200 - 1,400	3
1,400 - 1,600	4
1,600 - 1,800	5
1,800 - 2,000	6
2,000 - 2,200	0
2,200 - 2,400	1
2,400 - 2,600	1

are used. First, the average number of animals observed per square mile may be calculated for the transect area. Population estimates for a larger area may be obtained when the per cent coverage of the area is known. In our caribou surveys, herd areas are plotted on maps and then the number of square miles is calculated by planimeter or dot area grid.

Occasionally heavy local concentrations of caribou are observed in an otherwise sparsely distributed herd. In this case it is advisable to obtain an estimate of the concentration numbers by aerial photograph or count and add it to the systematic transect data. Care should be taken to delete the transect mileage across the concentration before calculating the remaining data.

SEGREGATED COUNTS

Valuable data on herd composition may be obtained from the air under favorable conditions. If the animals are distributed in small groups, age and sex classes may be counted on a multiple tally counter.

This type of work calls for familiarity with the age and sex groups on the ground. Advantage may be taken of the seasonal aspects of certain characteristics, for example, the presence or absence of antlers. Caribou with antlers in April and May are either cows or young bulls. Herds of cows and calves may be forced to string out. The aircraft may then fly along the flank and permit a calf count. Calves and yearlings may be distinguished from above by their short dorsal aspects.

Herd composition data, however, are best obtained from aerial photographs.

USE OF AERIAL PHOTOGRAPHS

We have made extensive use of aerial photography in our caribou survey work in northern Canada. Low-angle oblique photographs are taken of groups of animals. We have used the photographs later in the laboratory to determine the age and sex class composition of the herds. After field workers have become familiar with the appearance of the various classes on the ground, they can segregate them without too much difficulty in good photographs.

We use a Fairchild K20 4x5 inch aerial camera in our work. These cameras hold a roll of 50 negatives. The shutter is re-set and the film re-wound with one motion of a crank. The shutter is tripped by a trigger on the crank.

The photographs are studied under a binocular microscope or stereoscope in the laboratory and the animals segregated into classes: bulls, cows, yearlings, and calves, and recorded on multiple tally-counters.

Loughrey undertook a series of experimental counts at Churchill, Manitoba, in early 1954 to test the accuracy of segregated calf counts from aerial photographs. He found close agreement between these and counts obtained visually from the air and on the ground.

Large caribou herds have also been photographed to facilitate accurate counting. The photographs are placed under a glass slide ruled in a grid which aids in counting close groups. Photographs of small herds of known numbers may be used as checks in the visual estimate of numbers of gregarious species.

SUMMARY

Aerial survey has proved to be the only practical method of obtaining data on big game populations, herd composition, and annual increment in the wilderness areas of northern Canada. Different big game species require different handling of data. Moose population data are best treated as trends. Caribou populations are best surveyed in early spring. Caribou populations are best segregated into herds for study. White sheep and walrus may be censused as herds in discontinuous habitats. Musk-oxen are more easily counted in summer along the margins of lakes and rivers.

The following technique is best suited to varied northern conditions. Individual angles of sight to animals observed and altitude are recorded. The effective transect width may be obtained by grouping the distances to the animals in standard intervals. Outer and inner intervals may be discarded when the scarcity of records indicates low efficiency of observation.

Low oblique aerial photographs are used extensively to obtain segregation data, by examining them under binocular microscope or stereoscope in the laboratory.

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DISCUSSION

DISCUSSION LEADER PETERSON: First of all, I'd like to state that I am indeed honored to be asked to come here to act as discussion leader for this Technical Session. I know that there are several people in this room who have had considerable experience with aerial survey work, so I would like to declare this paper open for discussion.

DR. A. DE VOS [Ontario]: I would like to ask Mr. Loughrey whether the Canadian Wildlife Service is using either individual tracks or track concentrations as additional information to establish relative populations of big game.

MR. LOUGHREY: I'm not familiar with the work, especially, that's been done on moose. I imagine there might be a valid index there.

In caribou work we have not found anything in the tracks that we can use to estimate populations, because of their habits of migrating. Particularly in the spring they will string out, and you will see a long trail or several long trails which may indicate 10 animals or several hundred animals. We certainly use them, as you probably do, in locating herds and trying to determine the outer periphery of the main herds.

It is possible, though, that on other species, such as sheep or moose, tracks would be a very useful indicator.

DR. JOHN L. BUCKEY [Alaska]: First I'd like to comment on this very interesting paper. We had some experience quite recently in counting caribou from the air in the Talkeetna area of South Central Alaska. We estimated the population there from earlier aerial surveys taken in November and December this year at between 10,000 and 15,000 animals. Due to the rather large disparity in estimates it was decided we would make a much more complete count, so in late January we acquired five aircraft and twelve men, and counted this area in some detail, using transect lines which were spaced one mile apart; counting within one-quarter mile of each side of the aircraft we covered half of the total area in which caribou were located, and counted 18,700 caribou in half of the area, so our total population was not less than 37,000 animals. Our earlier estimates of 10,000 to 15,000 were obviously way low.

I might say this as a comment. We have been quite dissatisfied with our aerial surveys, particularly where caribou were in wooded areas.

The second thing is in the nature of a question, and I'd like to ask Mr. Loughrey if he has noticed any disparity between the numbers of animals seen by the observer and the pilot, or do you have two separate observers?

MR. LOUGHREY: Normally we like to use two observers, if we can. The pilot is quite busy usually, flying and map reading as he goes, and if he can maintain a fairly constant altitude we're quite satisfied to have him do that.

We have one observer in the co-pilot's seat, and the other just back of the pilot. His vision isn't quite as good as the man who sits in the co-pilot's seat.

We have checked by having two observers sit on the same side, and these checks were astonishing to me, because they were close. I imagine there could be a good deal of individual variation, however.

I'd like to comment on your observations. First, I think that it is very interesting, and I'm sure we should all try a good deal more of that type of survey, especially in large coverage. Fifty per cent coverage is certainly very fine, and if we were all experimenting along these lines we would have a much better basis for our herd estimates.

DR. BUCKLEY: We found on this particular survey that on the left side of the aircraft, the pilot's side, we averaged 10 to 25 per cent less caribou observed than on the observer's side of the aircraft, and that also applied in those few areas where we had two observers in addition to the pilot.

We have made a practice in the past of using the pilot as an observer, but it certainly is much more satisfactory to use two, as most of us now do.

MR. R. C. PASSMORE [Ontario]: I would like to ask Mr. Loughrey whether there has been any consideration given in the case of caribou and moose to the weather conditions available at the time of the survey, and also the influence that might show up because of the hour of the day the survey was made.

MR. LOUGHREY: That matter is discussed a little more fully in the introduction, on aerial techniques. Yes, we have considered that. We tried to do most of our caribou work in late March, early April, and May, and this is a very good time for flying. Daylight hours are long; weather conditions are usually ideal—clear skies and very good sighting. We can almost count on several weeks of continuous sunlight, if we are lucky, and we try to fly our surveys at these times. Certain times of day, especially for caribou, are important. In the afternoons the caribou are often on the lakes lying in open country. Early in the morning they are in the bush and very difficult to census.

I think the question of sighting on days when the weather is not good—certainly this restricts visibility with a wide transect, but when we are flying these narrow transects of half a mile, or a quarter, or even less, the visibility is not too much impaired by a cloudy day. It certainly makes a difference with tracks, though.

MR. PASSMORE: Now, I was thinking of the difficulty of sighting animals, the possibility of seeing them. I was wondering whether that has been correlated with air speed, for instance.

MR. LOUGHREY: Well, there has been no correlation done as such. I think we all have our own feelings on these matters, and we do attempt to try and run them when we think conditions are most favorable.

MR. PASSMORE: Some of my own work has shown great disparity in numbers of animals seen per unit of time over the same range under different conditions, and changes which occur in a fairly short period of time. I am speaking of such things as, perhaps, five moose seen per minute on one day, and a couple of days later you come back and look for moose and perhaps one per five minutes. I'm not sure I have much idea why those differences show up, but I suspect that the weather is a factor.

MR. A. T. CRINGAN [Ontario]: I'd like to ask Mr. Loughrey one question. He says that the Canadian Wildlife Service give the aerial statistics for determining moose. I wonder if you can give me an idea how big a change in the population has to occur before this change becomes obvious, whether it's 20 or 30 per cent or just what size of change can be indicated by this method.

MR. LOUGHREY: As I mentioned earlier, I haven't been associated with the moose survey work with Mr. Flook, and Dr. Banfield has just told me that we don't have any data on that matter at this time.

DR. DE VOS: I have a short question. I would like to find out from Mr. Loughrey whether he ever tried to assess eye fatigue as a disturbing factor in this aerial census work. It has been our experience that eye fatigue starts pretty early, and after about eight hours' work it increases, and it is possible that there is quite a difference between individual observers.

MR. LOUGHREY: We have worried about this subject quite a bit. We came to the conclusion that you have come to, that it does vary so much not only from individual to individual but even from day to day. After you have flown for three days, say, continuously six hours a day, you are lucky the third day if your eyes are even in good shape after an hour or two hours.

We realize that there are times when efficiency drops off, and it varies with conditions of barrenness, whether you are simply looking or recording. I myself feel that I can fly about four hours due to eye fatigue. In the bush I imagine that in a much shorter time you would become fatigued. It's difficult to put down on paper quantitatively, and I believe we all can recognize our deficiencies after we have been doing this kind of work for some time. However, you're not scanning all the time, and we usually try to rest our eyes in between.

DISCUSSION LEADER PETERSON: If I may be permitted, I'd like to make a very short comment before we close the discussion on this paper. I have had a bit of experience in aerial survey work with moose. We found that after considerable experimentation it was virtually impossible to fly a random area and get any reliable census. We found it was necessary to fly only after a fresh snowfall to report both animals and tracks seen, and our experience has indicated that we have never been able to locate any more than half of the animals that are in the transect that we fly.

This is an area of mixed hardwood and bush, so it would not apply to open country.

I would suggest that all of you who are using air survey techniques do more work on standardizing your techniques, and see if we cannot perfect something that will be a reliable index. Certainly I feel myself that it is the only possible way we can cover the waste North country with such thoroughness as to get any idea at all of the number of animals that are involved.

SOIL SURVEYS FOR GAME—RANGE DEVELOPMENT

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Man's struggles against vegetation extend far back beyond history. They have existed on every continent repeatedly. Man has not always won these contests and, when he has, it has too often been only to lose again; for the soil had been in partnership with the objectionable vegetation and on its removal followed it over the brink.

California has a little over 10 million acres of brushland that has been a problem in several aspects. The Mediterranean climate with wet winters and dry summers resulted in the development of some unique vegetation. There were grasslands on the deeper soils at the lower elevations, conifer forests in the cooler and moister mountains and along the humid north coast, but in the rugged terrain between, where the rainfall is moderate to light, a zone of broadleaf but evergreen shrubby vegetation developed. The Spanish gave it its most commonly used name, *chaparral* (Cronemiller, 1942). It can be called a "fire-type" since it was burned repeatedly since time immemorial, and it re-establishes itself quickly by seeds and sprouts following fire. The soils of the zone are usually shallow with little arable land and seldom are they of sufficient depth to support conifer timber.

During the days of the pioneer these brushfields were burned for a number of purposes. First, to facilitate prospecting by the gold seeker, then by the stockman to gain access to intermingled feed areas, to facilitate the handling of his livestock and also for the sprouts and herbaceous forage that grew for a few years following the burn. Occasionally strips and patches were burned to afford protection to his improvements from fire and to the cured grass in his pastures as well.

With the full settlement of the state, the threat of fire to improvements and to other values often deterred many individuals from such burning. The threat of damage suits by neighbors had a similar effect. Fires rushing up into the timber types pushed back the timber line. Laws were enacted governing the use of fire. Extensive watershed damage brought a realization that there were still other values requiring consideration.

Protection of these lands from fire was established and, as the land was reclothed with shrubs and trees, forage values diminished, livestock handling was made more difficult and because of the increase in the amount of fuel, the damage to individual acres from wildfires increased.

There is wide interest in the possibility of managing these lands. Timberland owners, foresters, livestock owners, power companies, irrigationists and municipal water users became interested in their management and, of course, were not all in agreement in what their management should be. Deer were attracted to burns, and such an opening of the dense cover facilitated hunting. Deer, rabbits and quail are important game species over much of the area, diminishing in numbers where the vegetation is too dense for their travel and feeding or where the brush is too tall to have much available forage. Sportsmen became interested.

There is a trite saying in California that "water is king," and there are few that will argue the contrary. There is fair agreement, therefore, that a vegetative cover must be maintained that will preserve watershed values.

The State Division of Forestry has the responsibility for protecting a large part of this zone from fire. In an approach to this problem, it secured an appropriation in 1947 which provided for a soil-vegetation survey that might serve as a guide to the management of these lands by indicating what the various subtypes could or should grow. This supplemented the forest survey started in California by the California Forest and Range Experiment Station of the U. S. Forest Service in 1946.

In cooperation with the University of California, survey methods were developed that would furnish the information needed for the proper development of these wild lands. Vegetation types and soil types were closely allied. The method developed was a combined soil and vegetation survey, however, these features were mapped separately. The vegetation map showed what was growing on the soil. The soil map indicated what should grow upon it, and therefore the cover manipulation that might be applied.

From the study of aerial photographs Timber Stand Maps were prepared for adjoining and intermingled areas of conifer timber. They will not be discussed here. The second series of maps, called Vegetation-Soil Maps, show vegetation and the soil types and were obtained by field survey. These were described by Wieslander and Storie (1952). The broad vegetation types include commercial conifers, non-commercial conifers, hardwoods, chaparral, sagebrush, grass and various mixtures of these. These were subtyped on the basis of the principle components of the vegetation.

Before the work in soil mapping started, it was necessary to determine the important soil characteristics requiring consideration in this type of program. This resulted in soil classifications that would indicate potentialities for timber, grass and water production. The system had to be usable by field men whose primary training had been in forestry rather than in soil science. The standard classification of soil series was found to be the best mapping unit for wildland soil.

Depth of soil is highly significant for wildland management. The mapping program, therefore, delineated for each vegetation unit (1) the prominent soils series and (2) average depth.

The soil series characteristics relating to hydrology and watershed management such as the capacity of the soil to store water, its permeability, erodibility, etc. were also explored.

Following the development of this methodology, an area of about two million acres, the greater part of Mendocino County was surveyed. This is one of the real problem areas. Most of the area was privately owned land, but the resulting work and the maps served as an excellent guide to the owners in developing a land management program. It prevented them from spending money on soils where there was little chance of obtaining the objective they sought. Instead it directed them to areas where there was the best opportunity for success in cover manipulation. For the stockmen it delineated the potential grasslands; for the forester, it delineated the potential timber lands. It aided by justifying certain water conservation programs and within the timber types permitted the forester to develop effective guidelines for the prevention of erosion on roads and skidways in logging operations.

The role of wildlife and much of the effect of cover manipulation upon it was yet to be studied. Game had a very important place in this zone of vegetation and the foresters and game technicians soon got together to face the problem. This resulted when the mapping program was extended into the Mendocino National Forest and an additional area of 250,000 acres was programmed. This was an important big game area and had heavy hunting pressure since it was open to public shooting. The State Department of Fish and Game assigned a wildlife technician to the project whose duties were to determine the game populations, seasons of use of different areas by deer and the preferred forage species.

This game survey showed that there was usually a year-round population of deer within the brush type supplemented by a wintering herd that migrated down from the higher elevations because of snow. The survey also showed large areas almost devoid of game because of the density of the cover and concomitant habitat deficiencies. Quail were absent from areas of continuous shrub cover. The old-growth chaparral was of low value, much of it out of reach of deer and low in rabbit population except where it joined more open areas with herbaceous vegetation. Numerous burns resulting from accidental fires within the zone indicated that burned areas were eagerly sought by game. Contemporary work on a Pittman-Robertson Project showed that game populations could be increased markedly by opening up brush areas by spot burning or by use of farming equipment.

An action program was prepared for a test area of 15,000 acres which would open up and develop the area to the limit of our present knowledge. This included opening up the brush fields with tractor trails for access by both big game animals and by hunters, also breaking up brush fields with tractors to secure an interspersion of low sprouting brush as a new food supply, with high brush for game cover adjoining it. It was also planned to convert some of the brush area to open types of low shrubs and herbaceous vegetation which gave a combination of forage types and made green grass available during most of the winter.

In removing cover temporarily on this rather rugged terrain, it was necessary to give considerable consideration to the effect on the watershed. Naturally the good soils were on the gentle slopes where there was greatest opportunity to control soil movement. The erodibility of each soil type was considered and treatment methods restricted to certain slopes in each. Often the work was conducted in the manner of contour farming. Reseeding with grasses and forbs was conducted on all areas where brush was removed.

For plowing, a heavy tractor was used with a brushland plow, a piece of equipment that had been developed especially for working brushy and rocky areas on the western ranges. In heavier brush the bulldozer was used. This was quite effective since the stems of most chaparral species are stiff and heavy and give way to the bulldozer blade when it is set to a shaving position about 4 inches above the ground. The plant material was windrowed by the bulldozer and subsequently burned. Where it was desirable to reseed with perennial grasses, the ground was thoroughly prepared by disking. Spot-burning was applied to some gentle slopes near ridge tops, making burns from 1 to 5 acres and giving an interspersion of open and dense cover. With the burns kept small, watershed values were maintained.

Burning during the fire season is very hazardous, but a series of techniques have been developed to perform the task safely. Following burning, many of the brush species re-sprout immediately and much of the seed stored in the soil germinates. It has a tendency to develop a denser stand of brush than existed at the time of burning. The arable lands within a burn may be reseeded to perennials. In some cases the seeds of annual grasses (which are available at low cost) are scattered in the ashes and these germinate with the first rain. These grass plantings are usually quite successful on the good soils at least and offer sufficient competition to newly germinating brush seeds that few survive. Many sprouting brush species are highly resistant to chemical sprays with the result that we have a problem of regrowth of brush. This is not yet solved but undoubtedly will be overcome within a few years since the problem is being attacked by several agencies.

The action program is developed entirely in the field. The forest ranger and a representative of the Department of Fish and Game go into the field equipped with the soils-vegetation maps, getting on to a series of points where a view of the country can be secured. They map areas suitable for development from the soils map. Ocular inspection gives indications caused by excessive slopes. By use of the aerial photos, the plan may be developed in still more detail since the soils-vegetation map rarely includes units of less than forty acres. The use of the aerial photos also permits the extension of the plan to small areas not conveniently visible. The final step is to map proposed tractor trails which will provide access for deer and also for hunters.

The action program for the area was financed for the most part by the Department of Fish and Game. It contracted for the tractor and operator on the basis of a daily rate. A Forest Service disc and drill was used and the Forest Service financed a part of the reseeding. Concurrently the California Forest and Range Experiment Station of the Forest Service conducted reseeding trials and cultural methods in similar types and within the area being treated. The tractor work was done under the supervision of a Forest Service or Fish and Game Department representative who furnished more or less continuous guidance to operator. As the various areas were opened up, facilitating access, minor revisions of the plan were made in accordance with the conditions found.

It is, of course, difficult to evaluate the results of any habitat development program. In the first place, knowledge has not reached the point where we can apply a value to hunting and fishing. Secondly, with so many variable factors, it is not possible to measure the effect of a habitat improvement program on game populations without accurate records over a long period of years. This much we do know, forage for deer has increased markedly and the deer, as usual, have responded. Hunting pressure has increased and in spite of this, hunter success has improved. Previously, hunting was most difficult and one might say the herd was practically unshot. Quail numbers have increased where grassy openings have been created, and cottontail rabbits have responded in a great way where there were good soils supporting some succulent vegetation. Rabbits reached a high peak in population in just a few years and apparently are adopting a cyclic population pattern typical of the species in this state.

The costs have been high for each acre treated, yet these treated areas are but a part of the whole new habitat and it is necessary, therefore, to assess a good portion of the costs to the untreated areas. It is, of course, impossible to develop an accurate cost-benefit ratio. Within the 15,000 acres, less than 1,000 were actually treated. This was at a cost of over $25.00 per acre. If assessed evenly to the entire area it would give a cost of $1.50 per acre. However, this still is a poor figure since the work has been on a trial-and-error basis—developing methods as we went, making mistakes and doing some useless work. But actually a usable pattern has been developed. Unit costs can be markedly reduced and the development of new equipment and new herbicides will effect further economies. Neither the Department of Fish and Game nor the Forest Service is ready to argue that a favorable cost-benefit ratio will result in very many areas. On the other hand, the sportsmen and the rest of our people will be insisting that all land be put to its highest use. Much of the ten million acres of chaparral lands in the State of California are valuable chiefly for the production of water and wildlife. The major opportunity for increasing the supply of wildlife is by making the land produce it. We feel, therefore, that monies will be made available for the continuance of the program.

CONCLUSIONS

(1) On agricultural lands the existing vegetation is important only as an aid to soil mapping. On wild lands vegetation type maps are essential to a soil mapping program since the vegetative cover is the crop with which the landowner or land manager must deal.

(2) This paper does not propose that game management agencies proceed to finance soil surveys. Instead it recommends that where there have been soil surveys in good game territory, that the responsible game management or land management agency explore the information they furnish and determine if it would assist in directing land management programs toward improving wildlife habitats.

(3) Where there are soil surveys in progress in good game territory it may be desirable to assign a wildlife technician to the survey. He would not only collect basic information on wildlife but also learn the local soils and their potentialites for cover manipulation as well as for wildlife production.

(4) With a knowledge of the local soils it is possible to develop sound action programs for the manipulation of vegetation types either by direct methods, using farm machinery, fire etc., or indirectly, by adjusting silvicultural practices.

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DISCUSSION

DR. A. DE VOS: As I understand the problem under discussion, Mr. Cronemiller, the ultimate motive is to establish in a sub-clinic of favorable plant species, and I notice he is using mainly one species, and possibly it's better to do it that way than with multiple species. Can you tell us the reasons why you believe that?

MR. CRONEMILLER: The paper indicates that the California Range Experiment

Station is carrying on a whole series of reseeding experiments. The point of using perennial rye or annual grasses for reseeding is that they take very quickly and successfully compete with shrubs that reseed mainly by seed. Perennial rye is not very permanent. In two or three years it will go out, and will change to an annual grass cover.

We certainly want to get into a multiple species process and more perennial species.

MANAGEMENT OF WHITE-TAILED DEER AND PONDEROSA PINE

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America's most popular big game animal, the white-tailed deer (*Odocoileus virginianus macrourus*), near the western edge of its distribution, comes into contact with one of America's most prized lumber trees—the ponderosa pine (*Pinus ponderosa*). The overlapping of ranges of these two great resources sets the stage for some knotty problems for resource management. Conditions under study since the 1930's in northwestern Montana furnish an example of these problems and how they are being met by private, state, and federal agencies.

The purpose of this paper is to describe: (1) an overstocked winter deer range in a ponderosa pine forest, (2) the detrimental effects of overstocking on the forest, and (3) an analysis currently in progress leading to the solution of the problem. A. Starker Leopold (1954), in his appraisal of the Wildlife Conference last year stated that reports of overstocked deer range are no longer newsworthy in big game circles. With this viewpoint we heartily agree, and we wish to emphasize that our first two objectives above are considered of only incidental interest as background for the primary objective of developing a concrete program for solving the problem. We hope that the methods of analyzing and solving our local management problem may be of value as a guide for others facing similar problems elsewhere.

The investigations and analyses reported here are the work of a great many individuals in a number of private, state and federal

[1]In cooperation with the International Forest and Range Experiment Station, Ogden, Utah.

agencies. In addition to those agencies represented by the authors, the U. S. Forest Service carried a very large share of the work.

We wish to acknowledge the fine cooperation we have enjoyed throughout the field work and the preparation of this report from many officials of the Forest Service. We are especially indebted to Mr. Arthur L. Roe and members of his staff at the Missoula Research Center; to Mr. Fred W. Johnson, In Charge of Wildlife, Region One; to Forest Supervisor Howard Ahlskog and his foresters on the Kootenai National Forest; and to Mr. Richard Griffith, Forester, J. Neils Lumber Co.

The winter deer range with which we are concerned in this report lies in the Fisher River drainage, a tributary of the Kootenai River in Western Montana just south of Canada. The deer range is located on the warm south slopes and in the creek bottoms. On these slopes and bottoms the climax tree union is Douglas-fir (*Pseudotsuga menziesii* var. *glauca*) with subordinate vascular unions of pine grass (*Calamagrostis rubescens*) in some localities and snowberry (*Symphoricarpos occidentalis*) in others (Daubenmire, 1953). The area is largely in the sub-climax stage in which ponderosa pine constitutes the dominant tree species.

It is desirable to maintain the forest in the sub-climax stage because of the superior economic value of the pine. Stumpage value of the pine is $28 per thousand board feet compared with $12 for Douglas-fir.

Like much of the ponderosa pine forest type throughout the United States and Canada, the Fisher River forest lacks younger age classes of trees and is composed mostly of mature and over-mature trees (Fig. 1). This means that as the virgin stands are cut there are no younger trees to provide future harvests.

We can roughly calculate the economic loss resulting from the lack of pine reproduction. A well-managed ponderosa pine forest may grow at an average rate of about 100 board feet of lumber per acre each year. At the stumpage value of $28 per thousand board feet, the pine forest is growing at the dollar rate of $2.80 per acre each year. In the Fisher River winter deer range there are about 32,500 acres of the ponderosa pine type. So each year that the winter deer range is not in production of pine regeneration, it is sustaining a loss of $2.80 per acre on 32,500 acres, a total of about $90,000 loss per year. The virgin timber of the Fisher River is currently being cropped so that the need for reproduction is immediate. The many economic and social implications are treated in detail by Kaufman and Kaufman (No date).

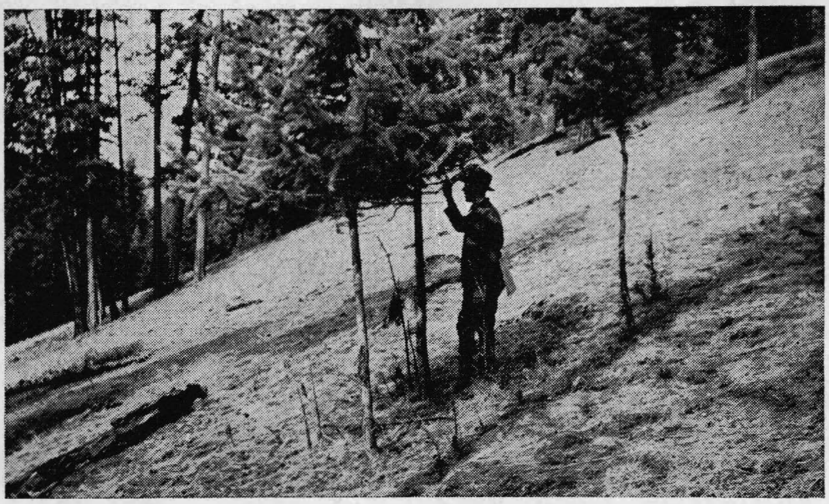

Figure 1. Typical over-browsed winter deer range in the Fisher River ponderosa pine forest. Note that the forest floor is almost barren of brush. Douglas-fir is the only conifer reproducing although the mature trees are predominately pine. There is a sharp browse line on the Douglas-fir and all seedlings below that line are stunted or dead.

There have been many attempts to explain the lack of pine regeneration. Almost every ecological factor that might be limiting has been called into account at one time or another. It has been suggested that the site is too severe, the soil is not right, root competition is too great, seed sources are inadequate, seed is destroyed by rodents, forces of plant succession prevent continued existence of the sub-climax species, and so on and on.

In 1945, as logging plans were initiated for the Fisher River, there was a need to quit speculating about causes and start finding out why there was no pine reproduction. In the 10 years since then the area has been subjected to intensive research by the Forest Service, the State Fish and Game Department of Montana, J. Neils Lumber Company, and the U. S. Fish and Wildlife Service. Before discussing the findings of this research on pine regeneration, let us briefly review the conditions existing in the deer population and the deer range.

DEER POPULATION AND RANGE STUDIES

The ponderosa pine forest of the Fisher River drainage has long been noted as an area where deer herds concentrate in large numbers during the winter. This influx from vast expanses of surrounding summer range has resulted in severe over utilization of the available winter forage (Fig. 1). Although both mule deer (*Odocoileus hemionus hemionus*) and white-tailed deer winter on the area, primary emphasis here is on the latter species, as it is far more abundant. Also its winter range coincides more nearly with the ponderosa pine timber type. Mule deer tend to winter at elevations above suitable pine sites.

We have little information about deer numbers and range condition before the mid-1930's. Indications are that the herds may have been at a low ebb around 1900 (Burgeson, 1942). This decline was attributed to the fact that deer hides were used as legal tender, valued at 50 cents apiece, between 1898 and 1900. Deer numbers apparently increased through the years since then, aided by the establishment of a game preserve on the Fisher River in 1923, the inauguration of a buck law in 1933, and a series of years in which weather conditions favored herd increase. Studies made by the Forest Service in the mid-1930's (Bealy and West, 1935; Drumheller, 1936) give us our first documentation on conditions of the deer herd and the range. It is significant that the reports of these earliest studies mention deterioration of the range. The combined effects of favorable conditions and of the management practices had led to the devolopment of one of the largest winter concentrations of white-tailed deer in Montana.

Deer censuses within recent years have afforded specific evidence regarding animal numbers and trends. Data from these studies in populations and winter mortality (Bealy and West, 1935, Drumheller, 1936, Bergeson, 1943, Zajanc, 1948, Schmautz and Zajanc, 1949a and b, Schmautz, 1950, Blair and Wilson, 1954a and b) indicated that the wintering white-tailed deer herd has been more or less stabilized over the past 10 or 20 years. Annual fluctuations occur as a result of variable winter mortality. However, the population appears to recover within a year or two following such losses.

The most recent studies (Blair and Wilson, 1954a) on the area found the present white-tailed deer winter concentration to be about one animal for 2.4 acres of range. This density ratio was calculated for the lower Fisher River area, where approximately 10,900 animals were found to be utilizing a total land area of about 26,300 acres. No detailed studies have been conducted on the remaining 21,700 acres of the winter range, but the population density was believed to be somewhat lighter here than in the lower Fisher River.

Studies of range conditions were made in 1949 by State Game Biologists Schmautz and Zajanc (1949b). Their range condition survey on sample plots in the deer wintering area showed that browse utilization varied from "heavy" to "very heavy." They noted further that the density of conifer reproduction varied roughly in proportion to the weight of browse produced—where browse was heavily used, conifers were heavily used and vice versa. This indicated that absence of conifer reproduction may be more directly related to deer browsing than to competition with other browse species.

As a result of many studies and recommendations made by the various investigators, a few steps have been taken to alleviate the winter range conditions. The game preserve was abandoned in 1944. Since 1949 there have been limited seasons for hunting either sex of deer. But these measures have not resulted in hunter harvests sufficiently large to affect herd numbers appreciably.

The small hunter take results from several causes. Frequently the onset of winter weather does not occur until after the hunting season, which results in the deer being more scattered and difficult to hunt. The area is remote from any large population centers which could furnish more hunters. Increased deer herds elsewhere in Montana and in adjacent states compete with the Fisher River herd for hunter attention. The non-resident big game license in Montana is $100, which eliminates hunting by most non-residents from nearby Idaho and Washington and other states.

There are several measures that may be used to increase deer reductions. Legislation now pending will, if passed, allow the State Fish and Game Department to sell special permits at $20 to non-residents to hunt in areas overstocked with deer. Good deer management also demands further liberalization of antlerless deer hunting, increased bag limits and extended hunting seasons. However, we should recognize the possibility that even with the complete utilization of all available measures for deer harvest the hunter pressure may not suffice to keep the herd in proper balance with the range.

As frequently happens, the sportsmen favor the excessive deer numbers. This sentiment deters constructive management and will continue to do so until the sportsmen learn to appreciate the use of sound ecological principles in deer management. An educational program for the sportsmen was recently instituted by the State Fish and Game Department and Montana State University under the auspices of the Montana Wildlife Forum (Messelt, 1951).

In anticipation of the possible eventual reduction of herd numbers, there is need for information regarding methods of reestablishing range forage. Some forage species have been killed out over extensive areas. Also logging activities will leave unused logging roads, landings and skid trails, some of which will not be used for new forests and will be available for growing deer forage. To obtain information on proper regeneration measures the State Fish and Game Department instituted browse reseeding studies in 1949 (Schmautz and Zajanc, 1949b, 1950b,c, Peterson, 1953) and browse planting studies in 1953 (Blair and Wilson, 1953b).

It is recognized that artificial regeneration of browse will be of no avail until the existing deer herd is reduced.

PINE REGENERATION STUDIES

In the light of the foregoing description of deer and range conditions, we are now ready to discuss factors that limit pine regeneration. These factors may be grouped for convenience into four categories—site quality, seed supply, seedbed conditions and seedling mortality.

The ponderosa pine areas of the Fisher River are all classified by the Forest Surveys Division as Sites III or IV (Meyers, 1938, Anon., 1941). In addition there are a few areas of Site V which were too small to be classified in the extensive Forest Survey. It is apparent from this that most of the area is well-suited for growing pine.

The number of seedlings occurring in an area is strongly influenced by the number of seed trees on the area. Roe and Squillace (1950) found a correlation of 0.80 between the number of seedlings per acre and the basal area of seed trees per acre on a selectively logged forest near the Fisher River. In harvesting the pine on the Fisher River the J. Neils Lumber Company practices light selective logging which leaves heavy residual stands of mature trees thus maintaining good seed sources.

The presence of an adequate seed source does not insure an adequate seed supply. There are many decimating factors that reduce seed abundance. Squirrels (*Tamiasciurus hudsonicus*) cut cones and fruiting twig tips (Squillace, 1953; Adams, 1955), destroying about 75 per cent of the cone crop. White-footed mice (*Peromyscus maniculatus artemisiae*) and chipmunks (*Eutamias amoenus luteiventris*) eat the seeds after they drop from the trees. Adams (1950) found that these small mammals ate pine seed, and Squillace and Adams (1950) found that about 90 per cent of the seed that fell was destroyed before they had time to germinate. They attributed this loss primarily to the rodents. Other decimating factors such as weather, insects and birds probably take their toll of seed, but have not been studied in this instance.

The effects of seedbed conditions are currently being studied by J. Neils Lumber Company and the Forest Service. The presence of a dense growth of other vegetation reduces germination and induces stagnation of the seedlings that do become established (Roe and Squillace, 1950). The presence of a thick duff layer also curtails germination. Wherever vegetative competition or thick duff occur, scarification or burning is necessary to lay bare the mineral soil.

Studies of seedling mortality were among the first objectives in the current research program. Deer browsing was considered a very probable cause of the lack of seedlings, for the few small trees that did exist were severely browsed and many were dead (Adams, 1949). Bowman (1946) reported in his management plan for the Kootenai National Forest, which includes the Fisher River, that, "the presence of deer in large numbers on open south slopes is inimical to the production of ponderosa pine since they browse and kill practically all the young trees."

In 1946 the Forest Service and the Montana Fish and Game Department built seven deer exclosures, 16 by 32 feet in size, within the winter deer range. Both artificial and natural regeneration have been studied on these exclosures in comparison with paired control plots nearby (Adams, 1951a). First, nursery stock was planted on the exclosures and control plots in the fall of 1946. The next spring deer had killed or severly damaged 60 per cent of the plants outside the exclosures. The browsed plants were replaced with new stock that same year. Three years later, in 1950, 99 per cent of the unprotected plants had been killed or severly damaged by browsing. Plants inside the exclosures have thrived except in two cases where deer have accidentally gained access by breaking through the wire.

Meanwhile natural seedlings began to appear in the exclosures greatly aided by a particularly heavy seed crop in 1948. Adams (1951b) reported that by 1951 there were 114 ponderosa pine seedlings on the fenced plots, 18 seedlings on the control plots. In 1953 (Adams, Unpublished Progress Report) there were 142 seedlings in the exclosures, 23 on the control plots.

A one-acre exclosure was constructed near the mouth of Wolf Creek in 1948 by the Montana Fish and Game Department. Conifer regeneration is abundant in this exclosure which corroborates the findings just reported. No complete account of seedlings has been made on this exclosure, but in the summer of 1954 they were counted on one-half of it. Seedlings counted on that half-acre were 599 ponderosa pine, 278 lodgepole pine, 89 Douglas-fir, and 75 larch. No count was made on the check plot but cursory surveys there reveal only a few seedlings—probably fewer than 100 of all species on the entire acre.

Concurrently with the exclosure studies a survey of the winter deer range was made to determine the amount of conifer regeneration existing there, the extent to which it was browsed and the successional relationship of pine and douglas-fir. With regard to the pine—Douglas-fir relationship it was found (Adams, 1949) that, although the overstory was 66 per cent pine and 34 per cent Douglas-fir, the understory (3 inches d.b.h. and smaller) was 33 per cent pine and 67 per cent Douglas-fir. This suggests a strong successional trend toward Douglas-fir. Inadequacy of restocking is indicated by the fact that only 21 per cent of 704 mechanically located 4-milacre plots were stocked with pine reproduction and only 43 per cent bore Douglas-fir reproduction. (An 80-per cent stocking rate is considered adequate.) The study showed finally that deer were feeding heavily on both species of conifers and that they showed marked preference for the ponderosa pine, thus favoring the successional trend toward Douglas-fir. This survey did not show how drastically the deer were destroying small seedlings leaving no trace of their former existence, a fact revealed later by the exclosure studies already described.

DISCUSSION

From the studies described above it is apparent that the pine's life is fraught with many dangers. From flower anlage to saw log, the individual tree runs a gamut of limiting factors, any one of which may eliminate the tree from the forest population. In developing a rational program for management of ponderosa pine we are now prepared to analyze the primary current problem—how to obtain adequate pine regeneration. This involves the evaluation of those limiting factors which are preventing regeneration as a basis for deciding which factors may best be controlled.

Experimental deer exclosure studies have shown that adequate pine regeneration occurs where deer are excluded. Where the deer are present in great concentrations, as they are on their winter range, pine seedlings do not survive. The questions arises whether we could overcome this effect of deer if we eliminated other adverse factors such as rodents, duff, root competition, etc., thereby producing such large numbers of seedlings that the deer could not eat all of them. While this particular point has not been fully explored experimentally (such an experiment is in progress), it is apparent that such treatment would not be successful. It seems unlikely that we could create such a quick and abundant surge of conifer growth that we could with conifer browse alone saturate and exceed the great demand for deer forage.

It is almost certain that the deer could absorb this added forage resource as fast as it is generated in either increased consumption per deer or in increased numbers of deer resulting from decrease in numbers of deaths caused by starvation. We must therefore conclude that conifer regeneration can be accomplished only if deer numbers are greatly curtailed. Before discussing methods of accomplishing such reduction, we need to consider another aspect of the problem of limiting factors in pine regeneration.

Our experimental exclosures have produced adequate stocking of pine regeneration. Does this mean that we should expect to obtain adequate restocking if we simply eliminated deer on a broad practical scale? There is good evidence that this might not necessarily be the case. There is considerable variation in the stocking rate on the different experimental exclosures, depending upon many limiting factors other than deer. The lightest stocking rate on one of the deer exclosures is 400 trees per acre, which is barely adequate for seedlings. Almost certainly there will be need in some situations for supplementary treatments in addition to deer control. Which treatments to be used will have to be determined by local analyses on the areas where management is being practiced. For such analyses, criteria are being developed from the experimental studies to be used in evaluating the need for various treatments. For example, the need for ground scarification will be judged on the basis of the amount of mineral soil shown to be naturally exposed in a set of point-intercept plots. The supplementary control measures which appear at present to be most likely to be needed are: insurance of adequate seed source by continuing to restrict the present cutting cycle to a light selective harvest, and possibly scarification, at least on those areas not badly denuded by heavy deer use. Censuses of mice and squirrels may show the need for their control also.

We are now ready to consider methods for reducing the influence of deer on pine regeneration. Obviously the best method of attaining such reduction is through increased cropping of deer by hunters. We have already mentioned the difficulty of achieving adequate hunter pressure because of the isolation of the Fisher River and the abundance of deer herds elsewhere which compete for hunter attention.

What alternative methods are available for eliminating deer damage? Johnson and Adams (in press) have suggested fencing as a silvicultural method under such circumstances. The primary obstacle to the use of this method is the cost. Johnson and Adams estimated that deer-proof fence would cost about $20 per acre if 160 acres were fenced, less for larger acreages. In the present instance we estimate much larger cost because of the steep, rocky terrain and higher labor costs. Our estimate for the Fisher River is about $45 to $50 per acre for 160 acres.

Can forestry afford such costs for pine culture? Here we enter the realm of forest economics. Within the limits of the present paper we cannot give a detailed analysis of all the implications of this problem in economics. We will present only the general considerations involved in answering such a question.

At present foresters quite commonly spend from $35 to $50 per acre, or even more, for planting seedlings where this is necessary to obtain regeneration. That such expenditures are of the same order of magnitude as the estimated fencing cost is empirical evidence that the fencing cost may not be prohibitive.

Another kind of evaluation consists of estimating expected monetary returns from timber growth in relation to the amount invested, including interest. Such an evaluation is presently in process by expert counsel and will be used by the J. Neils Lumber Co. in deciding whether fencing is the best method.

Beyond these analyses, the Company's Directors will weight the cost-benefit ratio of fencing against the desirability of alternative investments that could be made with the same capital in improving other holdings or in acquiring other forests less costly to administer. These matters are currently under consideration by the Company's management.

SUMMARY AND CONCLUSIONS

Several investigations of white-tailed deer and ponderosa pine in northwestern Montana have shown that browsing by deer is a primary limiting factor in pine regeneration. Heavy browsing results from an overstocking of deer on their winter range. This range roughly coincides with the best pine producing areas.

Since the deer are effectively blocking the regeneration of pine, the private and public agencies concerned with pine culture are confronted with the alternative of either controlling deer damage or discontinuing pine forestry following the harvesting of the present stand of virgin timber. The choice between these two courses of action will be determined by the practicability of reducing deer browsing to tolerable limits.

At present the two methods of limiting deer damage that appear most promising are reduction of deer numbers by intensified hunting, and fencing tracts of pine forest to exclude deer during periods of forest regeneration.

Deer reduction by hunting is the more desirable solution to the problem since it entails the proper management and utilization of the recreational resource. However, if this method is to be effective, the hunting pressure must be greatly increased. Hunting pressure can be intensified by several means: hunting both sexes of deer equally, extending hunting seasons, adjusting hunting seasons dates to weather conditions favorable for hunting, keeping access roads passable during hunting seasons, increasing bag limits, reducing out-of-state hunting license fees, and the like.

It is possible, however, that adequate hunting pressure will not be attainable even with all these measures. In that event, fencing appears to be a feasible method of locally controlling deer damage.

Whether fencing is the most practicable method is a problem in forest economics to be analyzed by those who manage the forests. In the event that fencing is considered a good tool, it is recommended that a pilot-project fence be established first to obtain more exact cost estimates and to test our experimental findings in their large-scale application.

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DISCUSSION

DR. DAVID E. DAVIS [Baltimore, Maryland]: I'd like to know a little more about the details of the fencing idea. Would the fence last for 10 years or 20 years? Would you extirpate the deer within the fence?

MR. ADAMS: We would expect to eliminate the deer within the fence. We would expect the fence to last 20 years. That's an approximation.

DR. E. L. CHEATHAM [New York]: I would like to know what per cent of the ponderosa pine area is of interest to the lumber company and is directly affected by this deer problem, in order to get perspective on that.

MR. ADAMS: The winter deer range and the ponderosa pine are practically superimposed on each other. Does that answer your question?

DR. CHEATHAM: In other words, it's a 100 per cent problem as far as distribution is concerned?

MR. ADAMS: That's right.

DR. CHEATHAM: I would like to make one more point in connection with that, and that is that in the Northeast a great deal of economic importance is attached to the spruce, and some lumber companies actually encourage and want heavy populations of deer in order to keep down other growths which compete with the establishment of spruce. Of course, spruce is not food for deer, but I think there is a certain amount of short-sightedness there.

MR. ADAMS: I wish we had the time to go into that point. I don't want to leave the impression here from my talk that we have an irreconcilable conflict between deer and pine. We certainly do not.

One of the things we are proudest of is the fact that we are synchronizing and meshing pine management with deer management. I could show you that the two are going forward simultaneously. When we build a fence and exclude the deer, we are not excluding deer culture by any means.

I wish I could go into details, but that's the case.

BARREN-GROUND CARIBOU MOVEMENTS IN THE CANADIAN ARCTIC[1]

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INTRODUCTION

Since 1948 a continuing study of barren-ground caribou, *Rangifer arcticus arcticus* (Richardson), has been carried out by the Canadian Wildlife Service. During the first two years of study the investigation covered the whole of the mainland ranges and added much to our general and specific knowledge of the animals (Banfield, 1954). In 1950, for economic and practical reasons, the study area was reduced to include only certain of the western ranges and a permanent operating base was established at Yellowknife in the Northwest Territories. Studies on many phases of caribou ecology and management have been continuous since that time under the direction of the author.

For management purposes a thorough understanding of the seasonal movements and distribution of the barren-ground caribou is essential. They travel extensively each year between summer ranges on the barren grounds and winter ranges often deep within the northern coniferous forests. Human utilization is high and has been estimated by Banfield (1954) at 100,000 caribou per year. The success or failure of the harvest depends to some extent on the initiative of the hunters and other factors, but to a greater extent on the movements of the animals. If the caribou do not come, there is no harvest. The attendant hardship on the native population has been graphically described by Back (1836) and other early explorers. If the animals come in unusual numbers, or after an absence of some years, there is a tendency

[1]In the absence of the author, this paper was read by Dr. A. W. F. Banfield.

among the more primitive hunters to take far more than they can use, and also more than the caribou herds can stand.

Caribou movements, distribution and population numbers have been studied on a continuing basis in order to discover what conditions might be counted on as usual or constant, over a long period of time. The present exposition is intended to illustrate some of the variable conditions thus far encountered, and to show the effects they may have on management plans.

AREA OF STUDY

The area involved in the continuing caribou study is shown in Figure 1. It includes forested winter ranges, west to the Mackenzie River between Great Slave and Great Bear Lakes, and north of Great Bear Lake. It also includes barren-ground ranges northward to the Arctic coast and east to the Bathurst Inlet area. The total area of the region under study is approximately 200,000 square miles.

Fig. 1. Map showing the area covered by the barren-ground caribou investigation during the past five years.

Greatest emphasis has been placed on studies of the forested winter ranges between the north side of Great Slave Lake and the south side of Great Bear Lake—an area of 50,000 square miles. Caribou range throughout this region in winter, as far as the Franklin Mountains and the Horn Mountains, which lie just east of the Mackenzie River.

METHODS

Data on caribou movements and distribution have been gathered largely from aerial surveying. A great deal of material has been gathered during the course of extensive winter flying for the purpose of assessing population numbers. Commercial aircraft operate extensively out of Yellowknife, and it is frequently possible to keep track of caribou for long periods by correlating pilots' reports.

Because of a need for economical operation, and because animals are easier to see against a snow background, much of the Canadian Wildlife Service's aerial work has been done from December to May, when the animals are on their winter range. This work has been supplemented to some extent by summer and winter work on the ground.

GENERAL MOVEMENTS AND DISTRIBUTION

Most major groups of caribou make seasonal migrations between summer ranges on the barren grounds and winter ranges within tree-line. The spring movement to the barrens is generally direct and rapid. The late summer and autumn movement into the forest is often leisurely and accompanied by much wandering. It is usual for the animals to pause in late August at, or within, tree-line and reverse migration briefly to the edge of the barrens, where the rut takes place, before they once more proceed to their winter ranges. Movements are not necessarily along a north-south axis; at least one major group of caribou moves almost directly east and west between its seasonal ranges.

Exceptions to this general movement pattern are known. Certain caribou herds spend the year round in the barrens. Even these, however, generally make seasonal movements between summer and winter range areas. Historical evidence of caribou wintering on the barren grounds has been presented by Clarke (1940), who also defined some of the more usual areas of concentration. Subsequent studies have shown barren-ground wintering areas to be much less consistent than those within tree-line; within a single year the wintering area of a given group of caribou may shift 100 miles or more. This would be expected, as barren-ground wintering areas do not provide grazing sufficient for as prolonged a period as the better ranges within tree-line.

Major anomalies in the basic movement patterns have long been recognized. For example, during the first Canadian Government caribou investigation, Hoare (1927) found animals wintering on the barrens in great numbers between Bathurst Inlet and the upper Snare River, but not in the forests southwest of there, toward Fort Rae. From the numbers which he encountered it must be surmised that many animals which normally winter in the forests chose in that year to remain on the barrens. A comparable occurrence has not taken place in recent years.

Besides being migratory, caribou are gregarious and tend to congregate in large herds. Clarke (1940) tentatively proposed a breakdown of the mainland barren-ground caribou into 11 herds, or concentrated groups, each with a reasonably predictable summer and winter range. Banfield (1954) proposed 19 herds which appeared to have discrete winter and summer ranges, and migration routes, during his period of study.

CONTINUED STUDY

The author's study has been confined to an area in which five of Banfield's proposed herds are found. The most important part of the study region, because of numbers of caribou involved and extent of human utilization, is the wintering area between Great Bear and Great Slave Lakes. The Radium herd moves into that area each autumn from the east and northeast, around the south side of Great Bear Lake, and frequently penetrates as far west as Fort Franklin and the Great Bear River. The Rae herd also moves into the Southern part of the area from the east and northeast. Both herds move out in an easterly direction in the spring. The history of this area as a winter range is long and consistent. During a trip between Great Slave and Great Bear Lakes in 1903 Preble (1908) learned that large numbers of caribou habitually crosses his route along lines still used by the Rae herd. Farther north he found well-worn trails along the southern shore of Great Bear Lake which would correspond to the routes still used by the Radium herd.

In each of the winters since 1950-51 the northern elements of the Rae herd and the southern elements of the Radium herd have mingled to some extent. In 1952 they moved onto the winter ranges on a continuous broad front and during several years the eastward movement in spring has also been on a continuous broad front until the animals neared the barrens. The two herds might well be considered one except that they appear to use separate summer ranges. The Radium herd uses ranges extending east over the barrens to the west end of Contwoyto Lake and from Point Lake in the south to the lower reaches of the Coppermine River in the north. The Rae herd generally moves east on the barrens south of Point Lake and then turns north towards the eastern part of Contwoyto Lake and southern Bathurst Inlet.

WINTER DISTRIBUTION OF THE RAE AND RADIUM CARIBOU HERDS

Distribution on winter ranges has shown great variation although there are a few areas of easy access and choice feed which have had wintering caribou in each year since 1950. During the winter of 1950-51 the Rae and Radium herds occupied a continuous area of only about 10,000 square miles at a density of about 14 animals per square mile. The eastern limit, except for a few scattered and insignificant groups, was Keller Lake— a lake which often approximates the western limit. The western limit was the Franklin Mountains and various mountain passes which extend as far as the Mackenzie River. During that winter caribou were found farther west than they ever had been in the memory of living natives. Their maximum penetration of the forest extended at least 300 miles from the edge of the barrens.

The following winter the same herds, fewer in number, occupied a continuous range about 25,000 square miles in extent (Loughrey, 1952) which included only the more easterly portions of the range used the winter before. In 1952-53 and 1953-54 the occupied winter range areas were much broken, and groups numbering from the thousands to the tens of thousands were found in widely separated pockets.

During this five-year period there was a sharp and continuous reduction in the number of animals on the winter ranges, which will be discussed below. There was, however, no corresponding reduction in the size of the utilized ranges. In both 1951-52 and 1952-53 the utilized ranges were more than twice as large as when the maximum population was present in 1950-51.

Another example of variability in winter range use may be drawn from observations made during annual late winter or spring flights along the north shore of Great Bear Lake. The north shore is largely forested and constitutes excellent winter range for the Great Bear herd which numbers about 30,000 animals. In 1950 and 1954 no caribou were seen in the forests along the north shore, and the absence of trails in the snow showed clearly that they had not been there. In 1951, 1952, and 1953, caribou were present in numbers and utilized forests along the whole shoreline at one time or another in each winter.

INTERREGIONAL MOVEMENTS

The literature makes mention of the possibility of two or more caribou herds mingling together. Both Clarke (1940) and Banfield (1954) suggest that this must occur occasionally. It has already been mentioned above, that elements of the Rae and Redium herds have been seen to mingle annually on their winter ranges. For management purposes this makes little difference, because the whole region, and the two herds within it, can be treated as a unit. Annual increment and permissible human utilization especially, can be conveniently calculated if the two herds are considered as one.

Quite another matter, however, is the occasionally observed mingling of the Radium and Great Bear Lake herds which has been observed in a few winters near Fort Franklin and the Bear River. The Great Bear herd moves into this area from the north and it is the only herd in the study area that is consistently under-utilized. The Radium herd moves in from the east and, together with the Rae Herd, it is in great danger of over-utilization. Mingling of these two herds have great implications in management, if a significant exchange of animals took place, leaving one herd reduced and one increased.

Winter observations in 1952-53 showed that this latter event may indeed occur. Several thousand animals of the Radium herd moved from the barrens westward in comparative isolation and took up winter quarters on the Bear River near Fort Franklin, having passed south of Great Bear Lake. During the winter animals from the Great Bear herd to the north pressed farther south than usual and reached the Bear River, where they met the Radium animals. In the spring, instead of taking their usual eastward course, the elements of the Radium herd in contact with the Great Bear herd followed the latter due north, presumably to summer ranges north of Great Bear Lake. It is hardly possible to imagine a more complete or sudden change of movement pattern than this. Whatever ranges the Radium animals ultimately reached, they could not have been less than 300 miles distant from those on which they originated.

Instances such as this have been frequently suspected during the continuing study but until an efficient method is devised for marking large numbers of animals they will be difficult to prove.

POPULATION SHIFTS

A perplexing phenomenon, referred to as a population shift for want of a better name, has been noted during the past five years on the winter ranges of the Rae and Radium herds.

In 1949 Banfield estimated the wintering population of the Rae and Radium herds at 215,000 animals. The next estimate, made by the author in the spring of 1951, put the combined total at no more than 147,000 animals. Conditions were not satisfactory for confident estimates during the following winters, but in the spring of 1953 it was evident, without any sort of mathematical calculation, that the population was even further reduced. It was estimated that there were no more than 60,000 animals in the area. Some of these had come in from south of Great Slave Lake, which would lead to the belief that they were not necessarily members of the Rae and Radium herds proper. In 1954 the ranges were given more thorough coverage than ever before and no more than 35,000 caribou were found, including, again, animals which might not normally be considered part of the usual wintering populations.

This reduction from over 200,000 to less than 40,000 in a period of five years is not accountable for in terms of over-utilization or disease. During the period in question, kill statistics have indicated a higher utilization in some years than is desirable, but not exceeding 10,000 animals per year at most. Disease conditions prevalent enough to bring about such a progressive decrease would surely have been noticed. The answer to this problem appears to lie in a shift of the caribou population from west to east. There is no other direction in which they could have gone without detection. The shift has apparently taken them out of the area under study. Occasional reports of unusual caribou numbers to the east have been received; for example in the spring of 1953 the movement of caribou out of the winter range area directly south of Great Slave Lake was larger, and of longer duration, than has been seen in many years.

An even more striking movement occurred on the barren grounds in 1952-53. In most years there is a heavy eastward movement of caribou in spring from winter ranges between Bathurst Inlet and the Coppermine River to calving areas eastward of Bathurst Inlet. After calving, the animals make their way back westward around the inlet, or across it by swimming from island to island. This condition has prevailed since at least 1950. Sometimes the Rae herd also reaches Bathurst Inlet at calving time. In the spring of 1952 there was a heavy eastward movement through southern Bathurst Inlet, in which barren-ground wintering groups certainly, and some of the Rae herd possibly, were involved. After the calving period the westward recrossing of Bathurst Inlet was unusually light, and only scattered caribou in small numbers were reported. The main body of animals apparently continued east and spent the following winter 300 to 400 miles east of the ranges which they had occupied for the previous several years. In doing this they re-populated, in a spectacular manner, areas which had been nearly devoid of caribou for fifteen years and more. Eskimo groups which for years had been supplied with imported skins to make clothing, took more than 9,000 animals in a few months.

Discussion and Conclusions

At the commencement of the continuing study it was felt that there might be certain consistent and basic movement and distribution patterns displayed by major caribou herds which would aid considerably in management. Historical references, mentioning major movements which are still taking place with regularity, and the findings of the initial studies in 1948 and 1949, indicated that such might be the case.

Human utilization of barren-ground caribou varies from place to place, being consistently light or heavy in some areas, and unpredictable in other areas. If, as suspected, movements, distribution and especially population numbers of caribou were relatively stable within even large areas then regional management could be applied.

Continuing observation of these matters, on a large study area, has shown that there are major inconstancies, some of which are presently inexplicable. Caribou distribution on important winter ranges in the study area has shown variation within a five-year period. It appears that a winter range can only be defined in terms of a few choice areas which are regularly used, and in terms of broad boundaries which observation over a period of years has shown to be the limits which a herd may reach. The potential winter range may be at least six times as large as would come under actual usage in any one winter. This fact, of course, is not necessarily inconsistent with regional management.

The mingling of major caribou herds on certain boundary areas has been observed frequently. In at least one recent case, however, an actual exchange of animals has been known to take place, with animals from one herd moving off winter ranges in an entirely new direction with animals from a second herd. In the observed instance the numbers involved were insignificant. If, however, major numbers of animals should move from one region to another under such circumstances, regional management could be difficult. The size and remoteness of many of the caribou ranges would make it possible for such an exchange to go unnoticed for years.

Of even greater significance in management has been a progressive shift of major numbers of caribou from the major wintering area under study. This has resulted in a reduction in the wintering population on that area to one-fifth its original number in about five years, and has involved over 150,000 caribou. It is only explicable in terms of an eastward movement to regions outside the area of continuing study. The fact that it has been progressive in nature, with fewer and fewer animals showing up on the same winter ranges in successive years, seems to suggest broad underlying causes not readily explained in terms of commonly recognized abormalities in caribou movement and distribution. Continued observation in the study area is indicated in order to ascertain when, and if, the larger population begins to re-occupy the area.

It is interesting to note that a few flights over the study area during the course of predator control work of the current winter, appear to indicate a heavier caribou population than in the preceding winter. Possibly a shift back to the area has already begun.

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DISCUSSION

MR. D. CLEGHORN [McGill University]: Does the Canadian Wildlife Service visualize extending this work to include Peary's form in the North and the Ungava form in the East?

DR. BANFIELD: At present we are entering the second year of a cooperative investigation of the Ungava area of Northern Quebec with the Quebec Department of Game and Fisheries. I have no information to give at this time, but a final report will be presented in some form when this investigation has been completed.

At the moment we do not contemplate any special investigation of the caribou of the Archipelago. We are collecting data each year through the operation of our field officers in the high Arctic, and through cooperation with the Royal Canadian Air Force we have been able to undertake air reconnaissance for three years. This reconnaissance will be continued this year also.

DR. JOHN BUCKLEY [Alaska]: I wanted to ask if there was any evidence of increased rates of movement as herds of caribou increased in size. I realize that this last paper has presented evidence that herds were decreasing in size, but I wondered if other observations of that sort were available.

DR. BANFIELD: We seem to have some places where that appears, and yet we're not sure whether it actually exists.

We have evidence of that condition from the reverse. As the caribou herds become decimated, they give up their large movements. I could cite several examples in the Eastern Arctic mainly, but it's the feeling of all of us who have worked with barren ground caribou that this is a reality, this restriction of movement with decimation of population.

INCREASED NATALITY RESULTING FROM LOWERED POPULATION DENSITY AMONG ELK IN SOUTHEASTERN WASHINGTON

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During the early November elk season in 1952 and 1953, the junior author (Swanson, 1952) noted that a number of females two and one-half years of age examined by hunters were lactating, indicating that these animals had become pregnant as yearlings. These findings were corroborated by the senior author during a special session (January 31 to February 22, 1954) when yearling females were examined for the presence of fetuses and during the fall of 1954 when information on lactation was again obtained from hunters. The interpretation and possible significance of this unusually high incidence of pregnancy among yearling elk in a natural population is the subject of this paper.

EVIDENCE OF PREGNANCY AMONG YEARLING ELK

Blue Mountains, southeastern Washington. Evidence of pregnancy among yearlings based on location in females two and one-half years of age in the Blue Mountains exclusive of the Mill Creek Watershed (southwestern portion of Blue Mountains of Washington) is presented in Table 1. The increase in percentage of pregnancy among

TABLE 1. EVIDENCE OF INCREASE IN PERCENTAGE OF PREGNANT YEARLINGS

	Blue Mountains Exclusive of Mill Creek Watershed		
Year	Number 2½-year-old Females Examined	Number Lactating	Per cent Lactating
1952	14	3	21
1953	12	5	42
1954	19	11	58

yearlings between 1952 and 1954 shown by these data probably reflects actual changes, although one may argue that the number of animals examined is too small.

Reliability of information from hunters may also be questioned. Hunters were first asked whether they examined for lactation and the presence of fetuses, and then for their findings. Data on lactation are probably accurate, but the figures on festuses in uteri (Table 2) may be erroneous. On one occasion, for example, a uterus containing a 2.8-pound fetus was found to have melted 3 feet into the snow where it was discovered after a 20-minute search, yet the hunter had reported this cow barren. Data on occurrence of embryos therefore must be considered as minimum numbers.

The high percentage (54 per cent) of fetuses *in uteri* (Table 2) in the Mill Creek Watershed corresponds closely with the percentage of pregnancy indicated by lactation for the same area (60 per cent) and for elsewhere in the Blue Mountains (58 per cent) during 1954. If these data represent actual conditions in the population, they indicate that a high percentage of females are still lactating in early November and that lactation at this time of year is a reliable index to pregnancy during the previous gestation period.

TABLE 2. EVIDENCE OF YEARLING ELK PREGNANCIES

	Yearling Females		2½-Year-Old Females		
Location	Number Examined	Per cent Pregnant	Number Examined	Per cent Lactating	Per cent with-corpora albicantia
Mill Creek Watershed southeastern Washington	35	54	5	60	
Blue Mountains, exclusive of Mill Creek Watershed....			45	42	
Wichita Mountains National Wildlife Refuge, Cache, Oklahoma	4	50	10		0
Yakima-Wenas Area, central Washington..	6	17			
Rocky Mountain National Park	6	17			
Nebo Elk Herd, central Utah	63	10	54		17
Yellowstone National Park	14	7	14		14

Elsewhere in the United States. Information on pregnancy among yearling elk from several other localities in the United States is presented in Table 2. Data were obtained from the following sources: (1) Yakima-Wenas and Wichita Mountains National Wildlife Refuge—unpublished records in the files of the senior author, (2) Yellowstone National Park—Cheatum and Gaab (1952), (3) the Nebo herd in Utah—Rognrud (1953), and (4) Rocky Mountain National Park—Coffin and Remington (1953). Graphic illustration is provided in Figure 1. With the exception of the Wichita Mountains Wildlife

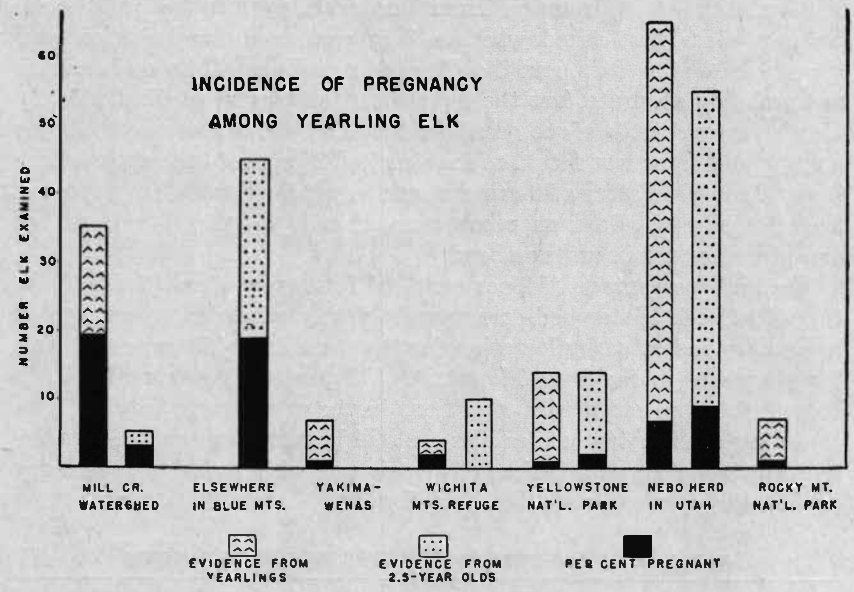

Figure 1

Refuge where only four yearling cow elk were examined for pregnancy in late November and early December, pregnancy among yearlings varied from 7 to 17 per cent. In comparison with the localities listed, the Blue Mountains stand out in conspicuous contrast with respect to the incidence of pregnancy yearlings.

HARVEST HISTORY

An historical background of harvests is essential for an interpretation of the above data. Elk are endemic to the Blue Mountains of Washington and Oregon, but were so low in population during the first decade of the Twentieth Century that enthusiastic sportsmen accomplished three introductions from Yellowstone National Park, as follows (Couch, 1935):

Pomeroy, Washington ....104 in 1911
Walla Walla, Washington .... 50 in 1919
Dayton, Washington .... 26 in 1931

In all probability the elk population would have increased to high levels without introductions. A photograph taken by Perry Foster in 1905 at Elk Horn Springs on the north side of the Grande Ronde River, Near Grouse Flats, shows 11 single antlers, some of which may represent pairs, and one pair gathered from the ground where they were cast on this wintering area. The area was opened to the hunting of branched-antlered bulls in 1927. Hunting of cows and calves in addition to bulls began in 1943 to exercise more effective control over the increasing population.

The harvest history from 1943 to 1954 is presented in Table 3. In 1949 the first significant herd reduction occurred when 1,500 permits for hunting either sex were issued. During the regular November season 1,411 elk were harvested; in addition, 502 were reported through the checking station during the period November 15 to March 1 when heavy snowfalls drove elk out of the Mill Creek Watershed to private land where they were heavily harvested. Actually, circumstances were such that probably close to 700 were harvested from this area, many of the animals not being reported. In January, 1949, when census conditions were excellent, 1,003 were counted from the ground and 821 from the air (Buechner, Buss, Bryan, 1951, p. 83). Further reductions in the entire Blue Mountain elk population resulted from a general open either-sex season in 1950 when 1,590 were har-

TABLE 3. ELK HARVEST STATISTICS, 1943-1954

Year	Bulls	Cows	Calves	Not Classified	Total
1943	99	317	152	123	691
1944	256	185	109	135	685
1945	459	274	89	......	822
1946	502	342	148	......	992
1947	342	87	52	69	550
1948	410	197	74	......	681
1949	594 (170)1	923 (184)	396 (148)	......	1913 (502)
1950	314 (26)	768 (27)	309 (28)	......	1391 (81)
1951	299 (63)	217 (56)	131 (46)	......	647 (165)
1952	372 (6)	142 (3)	70 (1)	...... (15)	599 (25)
1953	420 (87)	226 (120)	150 (83)	......	796 (290)
1954	388	154	86	9	628

1Special Mill Creek season in parenthesis; other figures are for the entire Blue Mountain area, including the Mill Creek drainage. Mill Creek figures recorded for 1953 represent elk harvested from January 31 to Febuary 22, 1954.

vested, 81 of which were killed in the Mill Creek Watershed during the late season. From 1951 to 1954 the seasons remained the same in the area exclusive of Mill Creek, bulls being legal over the entire area and cows and calves legal only on private land on the periphery of the mountains outside the Umatilla National Forest. Over the entire Blue Mountain area the elk population following the 1949 and 1950 reductions was probably about 2,500; in 1954 it was perhaps closer to 3,000. Because of mild winters no reliable census data are available and these figures should be used with caution.

The harvest data in Table 3 and the age-composition data in Table 4 indicate that the Blue Mountain elk herd represents a young, vigorously increasing population in which the average annual harvest by hunters is near the maximum. A consistent annual harvest above

TABLE 4. AGE COMPOSITION OF BLUE MOUNTAIN ELK POPULATIONS (BASED ON HARVEST)[1]

Approximate Age (years)	Mill Creek Watershed (Jan. 31-Feb. 22, 1954) Bulls	Cows	Elsewhere in Blue Mountains (Nov. 1-11, 1953) Bulls	Cows	(Oct. 31-Nov. 7, 1954) Bulls	Cows
0 - 1	47	37	27	26	27	27
1 - 2	38	41	88	27	83	24
2 - 3	10	19	36	21	29	17
3 - 4	11	19	18	16	19	14
4 - 5	5	12	14	5	3	11
5 - 6	5	4	14	4	4	1
6 - 7	3	5	5	4	0	5
7 - 8	3	2	1	5	0	4
8 - 9	0	4	1	2	0	2
9 - 10	0	3	0	6	0	2
10 - 14	2	2	0	2	1	9
14 - 20+	0	0	0	0	0	4

[1]These data should not be ragarded as highly precise, particularly for the age classes 5-6 to 7-8.

approximately 700 would probably increase the total annual population mortality and result in a decline in population. Apparently the population is now being held in the inflection portion of a sigmoid population growth curve, and both natality and survival are maximal.

DISCUSSION

The high incidence of pregnancies among yearling elk in the Blue Mountains seems to have resulted from lowered population density. Unfortunately no data on yearling pregnancies are available for the time when the elk population was at its peak during the mid-decade period between 1940 and 1950. On the basis of merely occasional pregnancies among yearlings in most natural populations (Table 2), it is probable that fewer yearlings were pregnant in the Blue Mountain herd when density was high. So unusual are yearling pregnancies among elk that special notations have appeared in the literature (Mills, 1936; Coffin and Remington, 1953). According to Murie (1951, p. 123) female elk breed for the first time at an average age of 2 years and 4 months. He states further that although a large series of so-called yearling elk was examined in winter for disease in the Jackson Hole area of Wyoming, one with calf was never found.

The increase in natality with lowered population density suggested by the data from the Blue Mountains conforms to the principle of population gain. That population gains tend to be inversely related to population density was possibly recognized in 1798 by Malthus and supported by recent studies (see Allee, *et al.*, 1949, pp. 25, 335, 349-352; Errington, 1945). Since no figures on yearling pregnancy for Blue Mountain elk are available prior to 1952, data in the present paper cannot be taken as proof that natality was increased when density of the elk population was lowered in southeastern Washington. However, this seems to be a logical assumption when the data from the Blue Mountains are compared with those obtained from other elk herds in several different localities, the former population apparently differing from most of the others primarily in that it has been subject to heavier mortality through hunter harvest over a longer period of time.

An interesting aspect of the high natality among yearling elk in the Mill Creek Watershed portion of the Blue Mountains is that the vegetation, particularly grasslands of bluebunch wheatgrass (*Agropyron spicatum*) and Sandberg bluegrass (*Poa secunda*), on arid south-facing slopes is in relatively poor condition (Buechner and Whittaker, 1954). Cattle and sheep were grazed in the watershed from the last two decades of the nineteenth century to 1924, at which time livestock was excluded by the U. S. Forest Service in cooperation with the city of Walla Walla to guarantee a continuous and pure municipal water supply. Soon after livestock was excluded elk began to increase, but they were not abundant until about 1940. Only in the most mesic sites along the main valley of Mill Creek was vegetation well-developed and in good condition after more than a quarter of a century of protection from cattle grazing. Choice browse plants were heavily utilized on both summer and winter ranges. *A. spicatum,* which provides 90 per cent or more of the winter diet of elk in much of the Blue Mountain area, was being produced at the rate of approximately 245 to 310 pounds (air dry) per acre. On similar sites elsewhere within the same mountain range, but where cattle had not grazed in any significant amount, if at all, production of *A. spicatum* was 1,478 pounds per acre. Vegetation of the Mill Creek drainage is, however, better developed than in areas such as the adjacent Blue Creek drainage, where the vegetation is grazed by cattle. Spring runoff from Blue Creek is heavily loaded with silt, whereas water from Mill Creek is at the same time almost perfectly clear.

Despite the slow recovery of the vegetation in the Mill Creek drainage since livestock were removed and the present relatively low forage production, elk populations increased rapidly over a period of two decades and, in all probability, exhibited an increase in natality among yearling females following recent reductions in population density. The presumed increase in natality among yearling females could have resulted from the greater availability of forage per individual following reduction of the herd by approximately one-half in the 1949-1950 hunting season. Better nutrition probably is the principal underlying factor responsible for higher percentages of pregnancies among yearlings. Higher natality among cattle and sheep under better conditions of vegetation has been experimentally demonstrated in several studies (Stoddardt and Smith, 1943, p. 477). In southern and western New York pregnancies among 36 per cent of white-tailed deer fawns was attributed to better nutrition (Morton and Cheatum, 1946). Only 4 per cent of the fawns were pregnant in the central Adirondack area where the vegetation consists more of mature forests and the population density of the deer was much higher in relation to the available forage than in the agricultural area of western New York. Cheatum and Severinghaus (1950) found an average of 1.71 embryos and 1.97 corpora lutea per doe in western New York and 1.06 embryos and 1.11 corpora lutea per doe in the central Adirondack area, indicating both higher fertility and fecundity where population density was lower and forage production higher.

Reduction of the elk herd in the Mill Creek Watershed approximately doubled available forage per individual. Although detailed studies of the vegetation are not available for the Blue Mountains outside the Mill Creek Watershed, it is likely that the vegetation improved little in production over the past five to six years and that increases in natality resulted from better nutrition at lower population density.

SUMMARY AND CONCLUSIONS

Although the available data do not prove that an increase in natality resulted from lowered population density, the evidence of more than 50 per cent pregnancies among yearling elk in the Blue Mountains of Washington strongly suggests this possibility. Harvest and age-composition data indicate a young, vigorously increasing population. Presuming nutrition to be the principal underlying factor responsible for increased natality, it is significant that the vegetation has not changed appreciably in recent years. Apparently the increase in natality reflects better nutrition through greater availability of forage per individual rather than improvement in the condition of the vegetation.

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DISCUSSION

DICUSSION LEADER PETERSON: Dr. Buechner has presented still more evidence, I think, of the basic importance of nutrition, particularly with respect to this species, and we are convinced that this is a very fundamental idea which we must understand before we can appreciate the dynamics of big game populations. It undoubtedly applies to a lot of other species as well.

FACTORS INFLUENCING MULE DEER ON ARIZONA BRUSHLANDS[1]

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This report deals with the ecology of mule deer (*Odocoileus hemionus*) and the prospects for their management on Arizona brushlands. Two representative areas were selected for intensive study. The Three Bar study area is located in the Mazatzal Mountains, 7 miles northwest of Roosevelt, Arizona, and contains about 7 square miles. McCulloch has worked there from November, 1952, to February, 1955. The other study area is located 10 miles west of Prescott in the edge of the Bradshaw Mountains and contains about 8½ square miles. Hanson worked there from June, 1953, to February, 1955. The study areas are approximately 100 miles apart. In addition to the intensive investigations on the study areas, reconnaissance has been done in many other localities.

It is not known what subspecies of mule deer is present in the Arizona chaparral. The problem is under study by Dr. Donald F. Hoffmeister of the University of Illinois, with the aid of materials sent him from here.

We gratefully acknowledge the extensive assistance of Dr. Wendell G. Swank, leader of the project. For several kinds of assistance we acknowledge the help of many persons in the Arizona Game and Fish Department: Messrs. John M. Hall, P. M. Cosper, O. N. Arrington, Theodore Knipe, Homer Erling, Steve Gallizioli, numerous game rangers, and others. An early version of the manuscript was read by Mr. C. Roger Hungerford, University of Arizona; and Dr. Lyle K. Sowls, Arizona Cooperative Wildlife Research Unit. Mr. Denis J. Illige, now of the New Mexico Game and Fish Department, and employees of the U. S. Forest Service have aided in several ways.

DESCRIPTION OF THE REGION

The chaparral-covered region we are concerned with runs through central Arizona diagonally from northwest to southeast and is located north of the latitude of the Gila River. South of the Gila River there are isolated mountains which also contain chaparral vegetation. Those brushlands are populated principally by white-tailed deer (*Odocoileus virginianus*) and are not considered in any respect in this report. According to Nichol (1943), there are approximately 9,100 square

[1]Contribution from Federal Aid in Wildlife Restoration Project, Arizona W-71-R.

miles of Arizona which contain chaparral vegetation. The majority of this would lie in the region considered by us.

The climate of the present brushland region is given by Smith (1945). The chaparral of Arizona occurs in a climate which has mild, short winters and long, hot summers. As much as 30 inches of snow may fall annually at some stations, but the snow usually melts rapidly. Annual precipitation averages near 15 to 18 inches, and most of it falls in the periods of December through March and July through September.

The outstanding weather problem for deer is drought, rather than snow or cold. Reynolds (1954) has discussed drought on southern Arizona rangelands. He considers "drought" of various classes as precipitation which is of varying amounts below average. In this report we call any year which had 75 per cent or less of the long-time average precipitation a year of "drought." At four representative weather stations with long records (from 34 to 88 years), "droughts" of this severity have occurred on the average every 4¼ years. Some of the droughts were unbroken for several years.

The topography of most of the region is very rough and characterized by canyons and steep slopes. In general, the brush vegetation is found between about 3,000 and 7,000 feet elevation, depending on exposure. The soils are generally very coarse textured and infertile. The brush may be found on numerous kinds of parent material, but most of it is on decomposed granite.

Most of the shrubs are of the "evergreen" broad-leaved type; a few may become small trees. Grasses and forbs are scarce, and few of the herbs grow in winter. The ground surface is, thus, typically bare of cover between the clumps of shrubs. Although more than 20 species of shrubs and small trees may characterize a mountain range, only two or three are generally abundant in any local area. Those probably most abundant over the region as a whole are shrub live oak (*Quercus turbinella*), several kinds of mountain-mahogany (*Cercocarpus* spp.), skunk-bush (*Rhus trilobata*), manzanita (*Arctostaphylos pungens*), and Emory oak (*Q. emoryi*). The vegetation has been briefly discussed by Nichol (1943).

METHODS

Most of the information contained in this report was gathered by repeated, intensive observations on the study areas. In addition, reconnaissance was made in a number of localities.

Information on the movements and the home range of mule deer was gathered from 30 animals which were naturally recognizable and seen more than once, and from observations of other deer while moving about.

The condition of more than 700 mule deer was examined at hunter checking stations. Twenty-five deer were collected in closed season for analysis of stomach contents and to secure information on physical condition. An intensive search was made for sick or dead deer on the study areas. All coyote scats found there were analyzed for possible deer hair.

Estimates of deer density were based on: (1) the number of non-duplicate bucks present in relation to buck:doe:fawn ratios, (2) deer watering at different sites, (3) deer flushed within strips of known size, and (4) deer seen feeding on areas of known size.

In the fall of 1954 the groups of old deer pellets were cleared from 30 transects, each 300 feet by 20 feet, selected at random on the Prescott area. The number of groups originally present gave an index to deer distribution and relative abundance in different vegetative types.

To give immediately an estimate of browse use, very careful observations were made on 144 plants, selected at random, of five species on the Prescott study area and of 370 plants of 12 species on the Three Bar area. Many other, more general observations were made of browse utilization on the study areas and elsewhere. Twigs were marked and measured on different shrubs (in August, 1954). Remeasurements of the twigs will later give more detailed data. The present vigor of one important shrub species was investigated from careful examination of 150 plants selected at random on the Prescott area, regardless of whether the plants were dead or alive.

To learn the time of growth, a small branch was tagged on 90 plants of 12 species, and the length of the twigs was measured at monthly or shorter intervals.

DISTRIBUTION, MOVEMENTS, AND HOME RANGE

Mule deer are distributed from the higher altitudes of the north, across the brushlands, and down onto the hot deserts farther south. White-tailed deer are frequent also in the southeastern half of the region considered by us (that is, the region north of the Gila River), but we have little information on the whitetails.

On the Prescott pellet transects, there were 2.25 times as many old pellet groups where some of the palatable plants (described later) were present as where they were absent. The difference was highly significant statistically. (The standard error of the difference equalled 2.95. P was less than 0.01.) This fitted well with field observations which showed that deer spent much more time in the vegetative types containing shrubs which we consider palatable.

We have been able to secure little information on the movements or home ranges of mule deer because we have been unable to mark the animals satisfactorily. A number of automatic markers described by Clover (1954), and others devised by us, were tried out. We have had no success with them for the reason that the dyes would not become fixed on the hair of the deer. Numerous types of dyes and solvents were propelled onto deer but without leaving a persistent mark. Efforts to capture deer in box traps for ear tagging were unsuccessful, too, although many kinds of baits were used.

It appeared that the home range of the recognizable deer averaged about 2 miles in diameter during the period the individuals were distinguished. Daily cruising radii averaged nearly 1 mile. Watering sites seemed to be the center of the home range when the site was surrounded by suitable habitat.

Deer were frequently seen to move one-half mile during a morning feeding period, up to 1 mile in flight after being alarmed, and up to 1 mile to water while being observed. Deer are distributed quite uniformly through areas up to 1¼ miles from available water. Beyond this distance their numbers usually decline, and we have not frequently found them more than 1½ miles from water. Because of that and the observations of where known animals fed and watered, we assume tentatively that 1½ miles is the average maximum distance which the animals will regularly move to water. The terrain and other factors, of course, affect the distance. It is very important that more information be gathered on this so that we can say definitely where water becomes deficient. When waterholes go dry, the deer may of course emigrate long distances to water. We were not able to learn the frequency with which deer watered. Weather, succulence of the food, and several other factors affect the frequency of watering (Linsdale and Tomich, 1953, p. 328; Clark, 1953, p. 92). Clark noted pregnant does watering up to four times daily in midsummer.

The chaparral vegetation is used as winter range by many deer which summer in coniferous forests adjoining chaparral. But in most of the chaparral region the deer are resident throughout the year.

The deer had larger home ranges and made longer daily movements than did some other populations of the mule deer. For example, in some California herds it was found that the deer seldom moved more than one-quarter mile from a given spot in one day, or even during longer periods (Leopold *et al.*, 1951; Cronemiller and Bartholomew, 1950). The mule deer of Tucson Mountain in Arizona, however, had a mean home range 2.3 miles in diameter, which was similar in size to the home range of our deer (Clark, 1953, p. 85). It is doubtful if our deer prefer to move daily the long distances they do, or prefer to have such large home ranges. Their lengthy movements may be required by sparsely distributed food and water.

POPULATION CHARACTERISTICS

Density. The density of mule deer in October, 1954, was about 10 per square mile on the Prescott study area and was about 12 per square mile on the Three Bar study area. The different ways of estimate gave good agreement with each other. Probably all of the estimates were uniformly conservative.

Where few palatable shrubs were present on the Prescott tract, the number of deer was about 5 to 7 per square mile. On the rest of the area the number was about 15 per square mile.

Last year for the first time, deer of any sex and age were shot on Mingus Mountain, 329 square miles in size and predominantly brushland. From various formulas and tables (Kelker, 1940 and 1952; Petrides, 1954; Lauckhart, 1950), it was estimated that an average of about 8 mule deer were present per square mile. Where the carrying capacity appeared highest, the density of deer was undoubtdly two or three times this.

After comparing the three areas where density was arrived at to numerous other chaparral areas, we judged that the mule deer population may average near 10 per square mile over the brushlands region as a whole. Over any extensive region there is generally great variation in deer numbers. That is true here also. Some localities appear to have two or three times this density of deer, whereas other localities appear to have only one-half this density or less.

According to Trippensee (1948, p. 207), mule deer are seldom found in a density greater than 20 deer per square mile. According to Wing (1951, p. 31), 25 deer per square mile might be considered a high density. The mean number per square mile of habitat in California was estimated at 13 deer (Longhurst *et al.*, 1952).

We think, therefore, there is not necessarily a shortage of mule deer on Arizona brushlands, contrary to what superficial examination may indicate. Deer numbers are often underestimated, because it is very difficult to observe them in the dense brush.

Sex ratios. A total of 856 observations of classifiable deer have been made on the Prescott area, and 908 on the Three Bar area. Sex ratios are shown in Table 1. The only hunting permitted for a num-

TABLE 1. BUCKS AND DOES OBSERVED ON STUDY AREAS

Three Bar			Prescott		
Period	Total adults	Bucks per 100 does	Period	Total adults	Bucks per 100 does
Jan.-Dec., 1953	173	83	July-Sept., 1953	142	53
			Dec., 1953-Mar., 1954	77	38
Jan.-Dec., 1954	383	83	July-Sept., 1954	157	51
			Nov., 1954-Jan., 1955	49	36

ber of years in these areas has been of bucks with more than one point, except in the Mingus Mountain unit. Because of the accessibility of the Prescott study area, heavy hunting, by Arizona standards, has taken place. Nevertheless, the post-season sex ratios indicated that numerous bucks remained after hunting—although many of these were bucks with spike antlers.

The Three Bar area has had no deer hunting for four years. (The area was set aside for research purposes by cooperative agreement with the U. S. Forest Service.) Consequently the proportion of bucks has been rising there but is still less than that of does.

Seasonal differences in the behavior of the sexes made it difficult to learn the true sex ratios. This is reflected in the widely fluctuating, apparent sex ratios on the Three Bar area (Figure 1). The observations of several months or more were combined to approach what we think were the true ratios during a given period (Table 1).

Graphing of the monthly data showed that similar trends were found in each of the years on both study areas when monthly sample size was more nearly adequate. The Three Bar proportions of bucks are shown in Figure 1 because they were the longer series of measurements. In some months sample size was very small. Where so, two months were lumped and the bar (Figure 1) was placed between the months. In some cases the sample was still less than 30 adults, and the sample was considered inadequate in size. The observed proportions of bucks, in general, were too high while the does were secluded in the fawning season; too low following the period of velvet shedding; and too high during the main part of the breeding season. For example, 117 adult deer were observed on the Prescott study area, in October, 1954. Hunting has begun here near the end of October. Velvet shedding is usually completed by about October 10. The sex ratio of these deer was 41 bucks:100 does. This compared with a ratio of 51 bucks:100 does for the period of July through September, 1954.

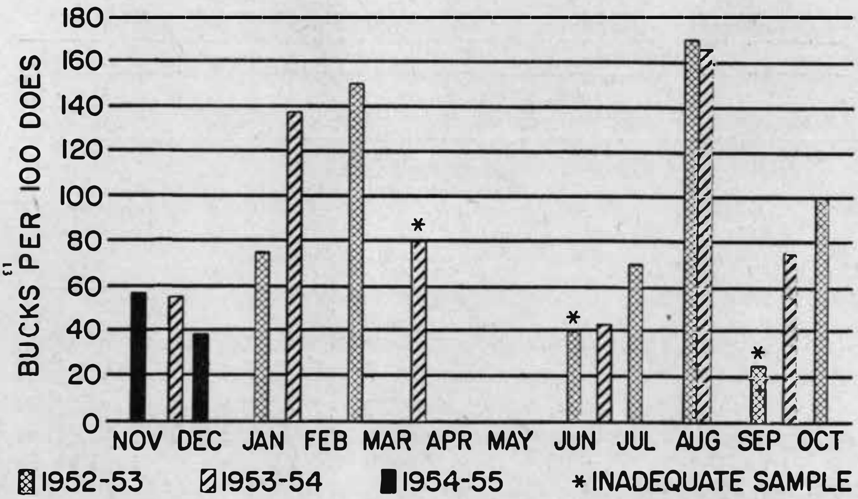

Figure 1. Bucks Per 100 Does Three Bar Study Area.

The difference between the proportions was highly significant according to the chi-square test. (P equalled about 0.01.) This means that it is difficult to make accurate surveys just prior to the hunting season and that the hunters have an unusually hard time finding bucks when hunting begins.

The increased elusiveness of bucks during the hunting season seems to be caused only partly by hunting. The dropping trend observed in bucks began several weeks before hunting started on the Prescott areas and was found on the Three Bar where no hunting took place (Figure 1). Linsdale and Tomich (1953, p. 151) also found that the wariness of bucks on their area increased as the antlers matured and that it reached a peak about the time the velvet was shed.

Fawn crops. Mule deer, on fair or better range, have usually had a fawn crop of 75 per cent or more as of when the fawns were 5 or 6 months old (Tolman, 1950; Biswell *et al.,* 1952; Cronemiller and Bartholomew, 1950; Rasmussen and Doman, 1947).

If the average fawn crop is taken as 75 per cent, then on both study areas the fawn crops were average or below in two years and above average one year each (Table 2). Our fawn crops are figured as the ratio of fawns to all adult does. On most of the regular winter surveys in central and northern Arizona, covering various types of vegetation, fawn crops have varied from 35 per cent to about 75 per cent for several years. (The Kaibab North is here excluded because it has

TABLE 2. FAWN CROPS OBSERVED ON THE STUDY AREAS

Period included	Three Bar Fawns:does	Three Bar Per cent fawn crop	Prescott Fawns:does	Prescott Per cent fawn crop
11/52-3/53	62:49	126	39:30	77
11/53-3/54	37:68	54	89:105	85
11/54-12/54	29:75	39	15:22	68

recently had a heavier harvest and a larger fawn crop.) The average of the fawn crops observed on the January surveys of 1950 through 1955 in the parts of Arizona just mentioned was about 53 per cent, or very deficient. (This was based on 7,186 does and 3,799 fawns observed.)

On the study areas, fawn crops have gone up and down roughly in proportion to the precipitation during the three years we know about. This short series of observations does not prove that lowered precipitation causes a lowered fawn crop. But the results tentatively indicate that. Our information is not detailed enough to show whether winter drought would be more effective in lowering the fawn crops than summer drought or vice versa. Surveys of various Arizona deer herds in recent years also seem to suggest this effect from precipitation. The effect is often not clear-cut because of various difficulties, including inadequate size of sample in the surveys. But if such a connection exists, it must indicate that present food and water resources are inadequate for the number of deer present.

Perhaps if deer were stocked well below carrying capacity, drought would have little effect on fawn crops. The further the deer herd is stocked above the carrying capacity level, the greater should be the effect from drought. The Santa Maria Mountains are predominantly covered with brushland. The stocking rate of deer in relation to carrying capacity is there probably the highest of any chaparral unit for which there are separate surveys. The fairly close relation between fawn crops and precipitation in these mountains is shown in Figure 2. In some other places, as the Three Bar area, where browse seems adequate, lowered fawn crops may be the result of drought-lowered supplies of herbaceous forage.

Productivity. According to Leopold (1933, p. 22), "Productivity may be defined as the rate at which mature breeding stock produces other mature stock, or mature removable crop." The ratio of yearling deer to deer older than yearlings gives an index to productivity. No reliable figures are available on the productivity of brushlands deer, because of the scarcity of age data from hunter checking sta-

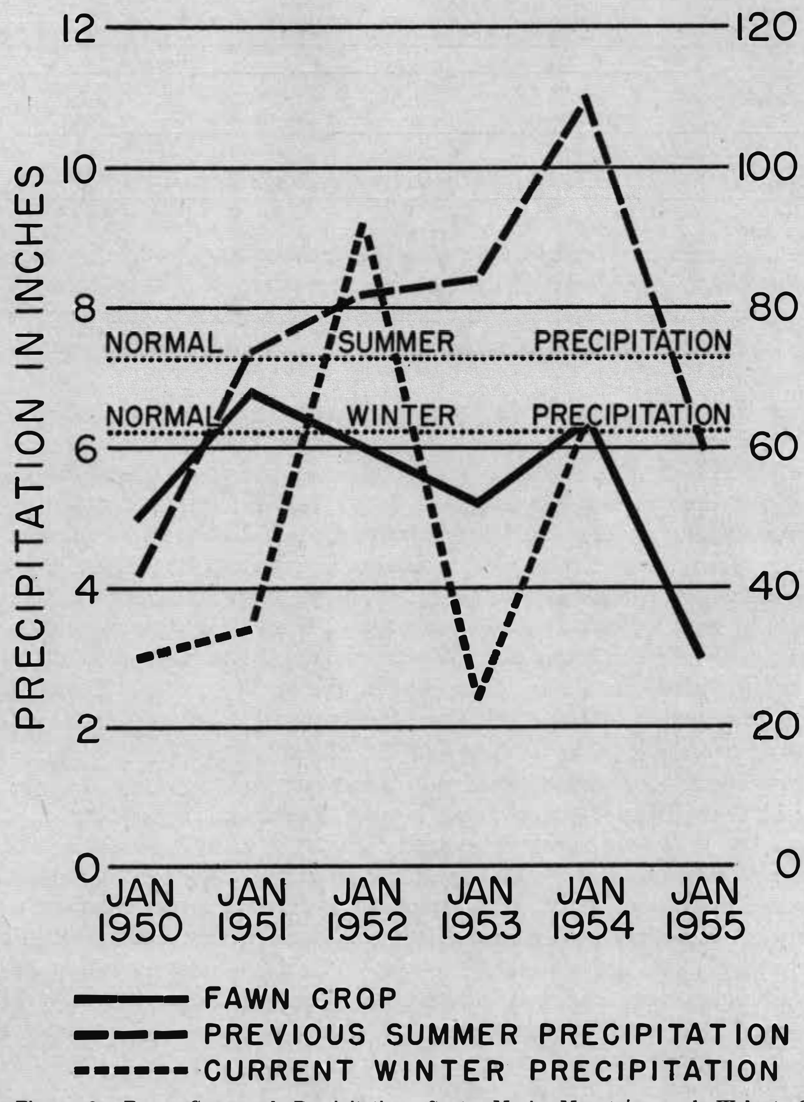

Figure 2. Fawn Crop and Precipitation, Santa Maria Mountains and Walnut Creek Weather Station.

tions. In the Mingus Mountain unit, where any deer could be shot in 1954, bias of the sample made it impossible to estimate productivity.

A productivity of 25 per cent or more has been found in most herds of mule deer living where range conditions were satisfactory

(Longhurst *et al.*, 1952; Robinette and Olsen, 1944; Rasmussen and Doman, 1947; Tolman, 1950; and other authors).

On the study areas, from repeated observations, we attempted to classify yearling bucks (based on antler size and shape, and body conformation). We then assumed an equal proportion of yearling does. On this basis, productivity was about 30 per cent and 24 per cent, respectively, for the two years on the Prescott area. Productivity at the Three Bar area was about 22 per cent and 19 per cent, respectively, for the same two years.

Based on the fawn crops observed (Table 2) and on the yearlings believed present, productivity was about average on the Prescott area and below average on the Three Bar area. Little can be definitely said about the productivity of Arizona chaparral mule deer off the study areas. In a number of other localities it must have been low, or in some cases barely average, judging by the fawn crops observed on the regular winter surveys. In no brushland area examined was there evidence of high productivity. This was probably because the range generally seemed stocked at or above carrying capacity.

TIME OF FAWNING AND BREEDING

Mule deer in the Arizona chaparral region breed and drop fawns late in the year as compared to other regions (Cahalane, 1947, p. 36; Trippensee, 1948, p. 183). The peak of the breeding season for the brushland deer appears to be around the first of January. Fawns may be dropped all through July, August, and into early September, indicating a prolonged breeding season. However, the majority of the fawns seem to be born during the last week of July and first week of August.

NATURAL MORTALITY

Disease and parasites. No evidence was found that infectious disease or parasites caused important mortality on the study areas. Despite careful search, no sick deer were seen, and on each area only one carcass was found of deer which might have died from disease. Not enough remains were found to account for all normal mortality. Some loss from disease or parasites may therefore have been overlooked. If losses had been heavy, we should have found more carcasses,

Examination of 25 deer collected during closed season, of more than 700 hunter-killed deer at checking stations, and of a few other miscellaneous deer showed no serious pathological condition. Lice, nose bots, ticks, a roundworm (*Elaeophora schneideri*), and cysts of a bladderworm (*Cysticercus tenuicollis*) were common in or on the chaparral deer examined, but the deer did not seem adversely affected. A few infectious diseases were present but causing apparently little loss.

It has been reported (Longhurst *et al.*, 1952; Taylor and Hahn, 1947) that disease as opposed to direct starvation is often the factor that finally kills many of the deer in the hotter climates. However, it is recognized by them and others (Cheatum, 1951; Whitlock, 1939; Van Volkenberg and Nicholson, 1943) that loss from diseases or parasites is usually a symptom of poor range conditions. Following severe drought, malnutrition might become critical and lead to die-offs from disease on the brushlands.

Predation. No evidence was found that the present populations of predators were limiting deer on the study areas. Coyotes and bobcats are found on both study areas, and mountain lions are found on the Three Bar area. Only one definite instance of coyote predation was noted, and one other instance was suspected. Only two mountain lion kills (both white-tailed deer) were found on the Three Bar.

Coyote and bobcat populations seem low on the Prescott tract and on various parts of the brushlands as compared to certain regions of the West where thriving deer herds are found. A program of predator control has been carried out in much of the state for some years. On the study areas this has made it difficult to learn if predators would normally be limiting to deer. In spite of intensive poisoning and trapping on the Three Bar area, coyotes and bobcats are more abundant there than on the Prescott area, but even so they have had no adverse effect on the Three Bar deer herd.

Almost every coyote scat seen on the study tracts was collected for analysis. Only 137 scats were found during the entire study in the Prescott vicinity, indicating a low coyote population. Of this number of scats, only five (or 3.65 per cent) contained deer hair. Of about 200 coyote scats collected on the Three Bar during the latter half of 1954, only 15 (less than 7 per cent) contained deer hair. There is of course no way of telling from scats whether the deer meat was eaten as carrion. Leopold *et al.* (1951) found that deer hair occurred in coyote scats with frequencies of 33 to 44 per cent on areas where predation on fawns was considerable. By these standards our coyote scats contained little deer hair.

Other natural mortality. No direct field evidence of any unusual amount of mortality was found in spite of intensive search on the study areas and reconnaissance elsewhere. A few deaths from accidents, and other miscellaneous causes, were noted but these were rare.

Based on field observations, on about 400 deer handled last year at the Mingus Mountain checking station, and on reports from hunters, the physical condition of the deer both on and off the study areas has recently appeared good in general.

Drought and carrying capacity. In Arizona carrying capacity fluctuates radically, and the long term capacity may be much different from the short term capacity. Severe droughts often occur. Probably such a drought would reduce the deer herd wherever it is stocked above, to, or nearly to carrying capacity of the environment. Drought would increase mortality and reduce natality (1) by decreasing the number of green forbs for food, (2) by reducing the amount of stem growth and increasing defoliation on shrubs, and (3) by reducing the amount of available drinking water. A loss of watering sites would cause the deer to abandon many areas and to concentrate in other areas where water remained. These other areas would often be stocked to carrying capacity already. The concentrating of deer would also tend to make easier the spread of communicable diseases and the capture of deer by predators.

Favorable years following a drought period would likely permit favorable productivity, because carrying capacity would be much higher than it had been during the drought. The herd would be recovering during normal precipitation. Study at that time would not reveal the effects of drought. Our work has been done during a period of nearly normal precipitation. So we have had little chance of finding heavy mortality from this cause. Although we have had no opportunity to see herd reduction by severe drought, it seems reasonable to believe that it would happen because the range is frequently stocked to carrying capacity or above during even normal precipitation.

According to Department personnel who are familiar with the southern Arizona chaparral, the frequent but limited die-offs of white-tailed deer in that region have usually coincided with the normally dry season of May and June. And recent short droughts (where only one of the two yearly wet seasons was dry) coincided with lowered fawn crops of the mule deer on the study areas and elsewhere (Figure 2). The last, very extended drought occurred during World War II when the Department staff was greatly reduced. Hence, no field information was secured on the effects to deer.

Finding deer mortality. If the deer are stocked to the long-term carrying capacity of the range or above, as they often seem to be, why have we not found more evidence of die-off? The losses may well have occurred, and may occur in the future, but they are not likely to be conspicuous. There are several reasons which bear on this.

First, no severe drought has recently occurred.

Next, it is extremely difficult to find a stationary deer in the thick brush. Our densities do not seem to be much more than one deer per 43 acres. The chances of finding one dead or sick deer on 43 acres of the brush present would be quite small.

Then, in a relatively hot climate such as ours, there will rarely be a sudden, drastic die-off within a short period. The carcasses instead of piling up in a short time will more likely be dispersed over a much longer time. We are accustomed to thinking of die-off during the winter season when the snow is deep and the weather cold. In this climate the main mortality is not likely to be restricted to the winter and may occur more gradually throughout the year.

HARVEST

The difference between the pre-hunt and post-hunt proportions of bucks gives an estimate of harvest when only bucks are killed. On the Prescott study area, this indicated that about 6 per cent of the total pre-hunt population was harvested each year. According to Kelker's (1952) yield table, about 7-8 per cent of the total population is killed when the sex and fawn:doe ratios fit conditions on the Arizona brushlands. There were no "any deer" hunts in recent times in chaparral, except for the one already mentioned on Mingus Mountain. This particular unit (329 square miles) last year received a harvest of about 16 per cent. Another unit of mixed chaparral and coniferous forest apparently received last year a harvest of about 17 per cent of all sexes and ages.

It is obvious that the harvest has been very conservative here, even allowing for normal crippling loss and poaching. The harvest could not have taken more than a small part of the yearly increment of new animals. Even in the two units where there was a harvest of any sex and age, the populations were probably not stabilized in view of the average productivity of mule deer. In spite of light harvests, the deer and cattle seem stocked to or above the long term carrying capacity of the environment in a number of areas.

Hunter success is low in the Arizona chaparral. Hunting is difficult at best because of thick brush, rough terrain, and a scarcity of roads. Few of the hunters care to hunt more than one-half mile, or at most 1 mile, from a passable road. Because of these difficulties, hunters tend to dislike the brushlands.

Hunter success, where only bucks with forking horns could be shot, has here run from about 24 per cent down to about 12 per cent or less, according to various data. On Mingus Mountain, where any sex or age of deer could be shot last year for the first time, hunter success jumped to 80 per cent compared to the 15 per cent of the previous year. The more remote areas may give a much higher success, although lighter harvest, because hunting pressure would be even lighter. Many of the remote areas see very few hunters during the open season and, even if opened to "any deer" hunting, would still tend to serve as natural refuges.

WATER SUPPLIES

Numerous undeveloped springs are present in the brushlands region, except in some limestone and lava areas. In addition, supplies of drinking water for deer and livestock have been artificially much increased in recent years. Coincident with the improvement of water supplies, there has been predator control, a reduced rate of cattle stocking, and an increase in deer numbers.

The estimates of U. S. Forest Service personnel indicate that deer in the Prescott Ranger District (about 417 square miles) of the Prescott National Forest increased greatly in numbers between 1926 and 1935. Then, according to the trend figures, the deer population approximately doubled between 1935 and 1949. Since that year, the deer were presumed to remain fairly stable in numbers. Perhaps the improvement in deer numbers up to 1949, and the stagnation in numbers since then, were influenced by the improvement in food and water up to 1949 and the lack of improvement in food and water since then. Although this refers to a small part of the brush region, we believe the same effects were present in other parts.

Deer and cattle will regularly travel 1 mile or a little more to water. The deer do not seem inclined to travel more than 1½ miles to water and tend to desert areas where they must travel farther, as shown by our preliminary observations. Water did not become limiting to deer on the study areas because water was available within the cruising radius of the deer.

In numerous other localities it is more than 1½ miles to the more persistent waterholes, according to Forest Service maps. In those places it is probable that water is ordinarily a limiting factor. It is true that a number of the waterholes are not shown on the maps. During the normally dry season of May and June in each year, however, many of the mapped waterholes do go dry, and many more would during longer drought.

For example, one period of eight months was nearly a drought on the Prescott study area. Of 14 deer watering sites on and near the study area, nine (or more than two-thirds) then dried up.

Without having seen it happen, we can speculate that during a long drought surely water would become a limiting factor in large areas. Even during normal precipitation, it seems to be in some places.

FOOD

Abundance and utilization of browse. It appears that deer are presently stocked to carrying capacity or above in much of the region considered by us, and that food is the factor most frequently limiting. We base this conclusion on the following:

1. Most of the brush is not palatable to deer or cattle.
2. Where the palatable kinds of brush are present, deer numbers are much higher than where they are absent.
3. The palatable shrubs show heavy use by deer or cattle or both.
4. Seedlings of palatable shrubs are seldom found.
5. We do not have a shortage of deer and the numbers of deer compare favorably with many other parts of the West.
6. Fawn crops on the study areas, and elsewhere on the brushlands, tend to go up and down with precipitation.
7. Deer numbers seem not to be increasing in most parts of the region; where deer numbers are increasing most rapidly, the browse appears to be in the best shape.

There is not space here to describe at length the composition of the vegetative communities nor of browse utilization on the many shrubs present. The symptoms of overbrowsing are similar to those reported for many other regions stocked to or above carrying capacity.

The tremendous stands of shrub live oak, skunk-bush, and manzanita furnish little food because the deer and cattle do not like to eat these plants. Locally or at certain times deer and cattle may eat the plants to a greater degree, but generally they are unpalatable. There are numerous other shrubs and small trees which are also unpalatable to deer and cattle. Usually, the unpalatable shrubs make up a large proportion of the total shrub cover (about 80-100 per cent depending on site, based on limited sampling and numerous observations).

Desert ceanothus (*Ceanothus greggii*), mountain-mahogany, and cliff-rose (*Cowania mexicana* var. *stansburiana*) bear by far the majority of the browsing in the northwestern half of the region. They are palatable, and they are also relative frequent as compared to other palatable shrubs. Locally other shrubs, such as hollyleaf buck-thorn (*Rhamnus crocea*), Wright's silk-tassel (*Garrya wrightii*), and others, may furnish some browse in the northwestern half of the region. But these latter plants are too scarce to be often important.

In the southeastern half of the brush country, palatabilities are similar, except that mountain-mahogany is unpalatable. Apparently the mahogany there is of a different species or variety; so far, we have not got positive identification of our mahogany plants. Cliff-rose is virtually absent, but some of the palatable species which are very scarce to the northwest (holly-leaf buckthorn and others) are more abundant to the southeast, and some additional palatable species are present. These other palatable species may do little more than fill the void created by the unpalatability of mountain-mahogany in the southeastern areas.

Cattle here seem to prefer the same shrubs which the mule deer do, except that in part of the region cattle may eat little desert ceanothus. Comparatively few grasses or forbs are present in the brushfields. This greatly complicates the food situation, because it deprives mule deer of the forbs, and it throws much cattle feeding on browse. As a result, there is frequently considerable competition between the mule deer and the cattle. We did not secure from any source information which would show the actual amount, but it is surely considerable wherever grasses and forbs are scanty. Cattle are ranged almost throughout these brushlands. Typical rates of stocking for cattle, on lands managed by the U. S. Forest Service, may run from about 4-15 cows per square mile of brushland, depending on the composition of the vegetation, etc. On brushlands not managed by the Forest Service, in areas with which we are most familiar, the stocking rates for cattle may run considerably higher, and the browse utilization then reflects this.

The degree of browse use varies greatly over this extensive region. The same has been found elsewhere since "ranges may be misused and yet have excellent feed in local areas" (Stoddart and Rasmussen, 1945). Let us take the Prescott study area as an example of a fairly typical degree of use.

Roughly one-half of the mountain-mahogany plants appeared too heavily browsed. The safe rate of use of mountain-mahogany growing in the hot, dry climate of Arizona is not known. For the present, we assume that 60 per cent utilization of the current annual growth is optimum. The rate of browsing on mahogany is such on most parts of the study area that the plants are frequently weakened or unthrifty. Not many of the plants are dead because they can and normally do grow out of reach of browsing animals.

The situation with regard to desert ceanothus, cliff-rose, and other palatable shrubs is more uniform. That is, almost every ceanothus or cliff-rose plant seen on this study area has been excessively browsed. The utilization of these shrubs is nearly always 80-100 per cent of current annual growth, and more often approaches 100 per cent.

Worse than the current heavy use of desert ceanothus is that many of the plants are nearly or completely dead. Of 150 plants selected in an objective manner on the Prescott area, 58 per cent were dead or nearly dead. And the plants were not counted as "nearly dead" unless 85 per cent or more of the crown on a given plant was actually dead. In reality, the plants probably die after much less than 85 per cent of the crown becomes dead, and so these estimates are probably very conservative.

Cliff-rose and the other palatable shrubs can and normally do grow out of reach of deer and cattle and thereby survive. In spite of this, where cliff-rose stands occur, many dead plants are usually found, and the cause appears mostly to be overbrowsing.

Browse utilization as described on the Prescott study area is typical of many other areas. However, in a number of places, browse use is considerably heavier. There, even shrub live oak, Utah juniper (*Juniperus utahensis*), and other unpalatable plants may show heavy use.

Then to take the other condition, there are a number of places where utilization is lighter than on the Prescott study area. We think that many of the places are deficient in drinking water. Also, in some areas, the deer herd may have been reduced by past drought and may not yet have recovered to the point where it is causing heavy use of browse. Doubtless there are other places where neither food nor water have been limiting and our conclusions do not apply.

The Three Bar study area is an example of an area where present use is light on palatable shrubs. Even on the most heavily used species, desert ceanothus, less than 25 per cent of the twigs had been browsed. Briefly, the history of the area is as follows: From about 1908-37 the area was very heavily used by cattle. From 1937-45 cattle were absent. Then from 1945-47 cattle were stocked again in lighter numbers. Food was thus greatly reduced. During World War II a small military camp was located nearby, and poaching was reported to be heavy. In 1947 more than half of the chaparral vegetation of the area was burned by wildfires. Consequently the deer population had been greatly reduced by these various causes. Browse (and water) are not now limiting, and the deer are increasing.

Perhaps there are other areas where food is not limiting because the deer herd was previously reduced by similar circumstances.

No controlled burning has been done on the state's brushlands where the effects to game have been specifically investigated. Cooperative experiments with different kinds of brush control are now being started, and various effects, including to game, will be studied. Our observations on wildfire burns show that for the first four years after burning most of the normally unpalatable species are very palatable. For up to seven years deer prefer the sprouts from burned shrubs to the same species where not burned. On the recent wildfire burns investigated, more deer and cattle were being supportd than in similar non-burned brush (up to two or three times more on the areas we knew most about).

A tract of brush was broken off on the Prescott study area by a bulldozer pulling a heavy drag made of train rails. About 80 per cent (roughly estimated) of the brush was broken off, but the sprouting species then sent up shoots as they would have from a fire. The new shoots were more palatable to deer than before treatment, but less palatable than after burning. More time must elapse before we can determine the value of railing. The cost was about $1.50 per acre, but it could be lowered by more extensive operations.

Shrub growth. From our tagged plants and general observations, we found that most of the shrub growth occurred in April. A smaller amount of growth occurred following the summer rains if the rains were heavy. Desert ceanothus grew almost as much in early winter as in spring, when conditions were favorable. This may be one reason why the shrub is so valuable to deer. All of our shrubs, like shrubs on the hot deserts, are well adapted to growing whenever weather conditions are favorable, and some growth may occur during any month.

Not only may the shrubs grow at any time, they may go into dormancy at any time. During drought periods even the so-called evergreen species defoliate to a greater or lesser degree, thus reducing the number of green leaves for deer food.

Succession. Seedlings or sprouts of the palatable plants were seldom found. The majority seen where browsing pressure was about average were growing under the protection of the disliked shrubs. Dead plants of desert ceanothus and cliff-rose were common. As a result, all of the palatable shrubs will tend to diminish slowly in abundance.

The unpalatable shrubs are sending up new seedlings and sprouts and increasing the size of their existing clumps wherever growth conditions permit. This is putting the palatable shrubs under increased competition. As the openings close up between clumps of unpalatable brush, deer and cattle are excluded. Succession is thus tending toward a community of lower carrying capacity.

CONCLUSIONS

The Arizona brushlands, in general, appear to be stocked to carrying capacity or above by deer or cattle or both. There often appear to be shortages of palatable browse, of forbs, and of water. Mule deer will probably decline in abundance whenever severe drought occurs. Carrying capacity is of a sharply fluctuating nature on the Arizona brushlands because the frequent droughts reduce the supplies of both food and water. Between periods of drought when precipitation is average or above, mule deer may not appear to be lacking in food and water if the population has been much reduced by previous drought. The limiting factors of food and water are frequently less apparent than elsewhere because deer die-offs are hard to detect as compared to colder climates. Regardless of drought or rain, the long-term trend in mule deer numbers is downward because too heavy browsing is converting the vegetation to one of lower carrying capacity. Possible management alternatives are to reduce temporarily the numbers of deer or cattle or both to permit forage recovery, to find ways to make unpalatable shrubs palatable, and to increase watering sites where needed. This study is continuing, and later information may change or clarify some of the conclusions given here.

SUMMARY

1. Study for two years has been made of the ecology of mule deer in the main part of the Arizona brushlands and of the outlook for increasing the deer numbers.
2. The deer have a home range about 2 miles in diameter, a daily cruising radius of about 1 mile, and seem not to travel on the average more than 1½ miles to water.
3. The density of deer appears to average at least 10 per square mile.
4. On most areas sex ratios vary from 33 to 50 bucks per 100 does. It is difficult to learn the true sex ratios during several periods of the year.
5. Most of the fawn crops have been average to low, and the number of fawns produced seems to vary with precipitation.
6. No reliable figures were secured on productivity, but it seemed to be average (25 per cent) to low where estimated.

7. Various parasites and diseases were found present but not limiting deer numbers. Predators were not found limiting under their present densities.
8. It is difficult to find deer die-off in the Arizona chaparral because brush is dense, droughts occur intermittently, and die-off tends to occur steadily through the year.
9. Harvest has been very light because only bucks are generally shot. Hunting is difficult and the hunters prefer to hunt in other types of habitat if possible.
10. There are still areas where there is no available drinking water during normal precipitation. During drought there would be even more of these areas.
11. Palatable shrubs make up only a small part of the brush vegetation. These plants are generally heavily browsed by both deer and cattle, and many of several species have been killed as a re-as a result. Mule deer appear generally to be stocked to or above carrying capacity for these and other reasons.
12. Carrying capacity is decreasing because palatable shrubs are being replaced by unpalatable ones on many large areas.
13. Carrying capacity is here of a fluctuating nature because of drought. Presumably when severe drought strikes again, deer numbers will be reduced; this has happened in some places in the past.
14. Several kinds of brush control offer much promise for habitat improvement and are being investigated.
15. Some management alternatives are to reduce temporarily the numbers of deer or cattle or both to permit forage recovery, to increase watering sites, and to make unpalatable browse palatable, as by dragging and burning.

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THE CONCEPT OF CARRYING CAPACITY

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The term "carrying capacity" is well-established in the vocabulary of wildlife biologists as well as of many ecologists working in other fields. It is one of those terms often employed without strict consideration of exact meaning which is used to describe a general conception rather than to express an exact idea. As such it is useful, but as our knowledge of factors affecting the survival of animals grows it is wise to examine periodically the terms we use and to consider their adequacy.

In preparing this paper we set out to examine the term "carrying capacity" to determine its meaning and to assess its usefulness in wildlife management. If the term as currently used proved inadequate we hoped to redefine it to give it more useful precision. We have tried not to confine our thinking to herbivores, ungulates or to any particular group of animals, for it should be possible to develop a general concept which can be applied to a wide range of animals. If the term is to have value in wildlife management, it should recognize broad criteria within which must fit particular conditions pertaining to particular species.

Our conclusions can be briefly stated. We find that most definitions of carrying capacity are vague and that some are almost meaningless. There is a difference of opinion in that many have used, and are using, the term as if it applied to food alone, while others use it to denote more than limitation of food and include other factors. We find that carrying capacity is often considered a stable characteristic of environment despite the fact that nearly all limiting factors are known to vary constantly in their influence on populations.

PAST AND PRESENT USE OF THE TERM

In analyzing our thoughts as well as those of associates with whom we have discussed the matter, we have found that the most basic differences of opinion stem from whether carrying capacity has meaning only with reference to food supply or whether its meaning is broader. Most people have assumed the former meaning, especially those working with ungulates. A few have maintained that this is the only meaning possible and have cited Leopold (1948) as the basic reference on this point for wildlife management. His definition states (p. 450) that carrying capacity is:

> "The maximum density of wild game which a particular range is capable of carrying."

Here food is not mentioned but elsewhere, in speaking of ungulates, he says (p.54):

> ". . . . there is so far no visible evidence of any density limit except the carrying capacity of the food."

A somewhat different meaning is implied in another passage (p.135) where he brings the importance of cover and "edge" into the definition. Although Leopold does not provide us with a specific definition it is apparent that he considered food as only one factor determining carrying capacity. This is illustrated by his use of the term "range" which, to him, included a variety of facilities in the environment which are utilized by animals. He says (p.135):

> "A range is habitable for a given species when it furnishes places suitable for it to feed, hide, rest, sleep, play, and breed, all within the reach of its cruising radius. Deficiencies in such places are usually seasonal."

The last statement is significant because it shows that Leopold recognized the variability of environment in terms of variations in factors such as weather, season and plant succession, and that dynamic environment is reflected in fluctuations in populations of animals.

A number of authors have emphasized food in considering carrying capacity. Hadwen and Palmer (1922), speaking of reindeer (*Rangifer tarandus*) say that grazing or carrying capacity is (p.29):

> ". . . . the number of stock which range will support for a definite period of grazing without injury to the range."

Here it is clear that food is the limiting factor under consideration. Trippensee (1948) also emphasizes the importance of food when referring to deer (*Odocoïleus*), saying (p.196):

> "The carrying capacity of a range, measured in terms of food availability, depends upon two factors; stand age and stand composition."

Elsewhere in his book he avoids the term. Allee *et al.* (1949: 706) also stress food in discussing carrying capacity. Fowle (1950) in reviewing factors controlling deer populations says (p.57):

> "The environmental factor which has received the most attention in deer studies is food. Indeed, our concept of carrying capacity for deer centers around the adequacy of the food supply while our criteria of overuse have their basis in the rate of depletion of food supply."

Dasmann (1945:400) regards carrying capacity as:

> ". . . the maximum number of grazing animals of a given class that can be maintained in good flesh year after year on a grazing unit without injury to the range forage growing stock or to the basic soil resource."

The same author modified this definition in a paper in 1948 (p.189) by changing the words "grazing animals" to "foraging animals" and replacing "grazing unit" with "range unit." This definition, confined to foraging animals, represents the modern trend of confining the use of "carrying capacity" to ungulates and other herbivores.

Dasmann's definition adds two concepts to that of Leopold; first, the quality of the animals should be considered, and second, deterioration of range cannot be allowed. Because standards have been established, quality is relatively easy to deal with in domestic stock, but with wildlife species it is more difficult. Perhaps it has yet to receive the attention it deserves in wildlife studies. Fisheries biologists have employed the idea with some success in cases where an environment is found capable of producing a certain aggregate weight of fish (biomass) made up of either a few large fish or many small ones. Reference to the "maximum number" that the waters will hold is meaningless unless the quality (size) of fish required is stipulated. Similarly, a unit of environment may support a large number of deer living at a minimum subsistence level, or a lesser number of healtheir animals.

If a population is sufficiently large to deplete the food supply faster than it is being produced, or injures the environment in some way, the size of future populations will be affected. This is an important concept in Dasmann's definition which is usually assumed in considering carrying capacity. In theory it is simple, recognizing that the animals should be in such numbers that they eat only the annual interest from food plants and none of the principal. Hence the environment should be stocked for perpetual maximum benefit rather than for a short period at its full capacity.

That Dasmann (1948) considers carrying capacity to be a dynamic concept is further illustrated when he says (p.189):

"The number of animals that will take no more than the forage crop in all but poorest growth years is the maximum number a range unit will support on a sustained basis. Since range is dynamic, changing continually with fluctuations in precipitation, temperature, evaporation, and varying use-patterns, no rate of stocking can be considered final."

Leopold (1948:51) is careful to distinguish between "carrying capacity" and "saturation point." The former he regards as "a property of a unit of range" and the latter "a property of a species." However, Graham (1944) refers to both concepts as if they applied to range. He says (p. 60):

"Closely related to cruising radius is the idea of *carrying capacity* of the land, whether it relates to wild species or to domestic animals such as sheep and cattle. Many factors influence the number of animals an area can maintain. The ancients had this in mind when they remarked that 'one hill will not carry two tigers'. A knowledge of the number of animals a habitat can reasonably be expected to support—its *saturation point*—is useful to the land manager."

This statement and the discussion which follows show that Graham was considering all factors, including social intolerances, which can limit the number of animals in an area.

In this connection the definitions of Errington and Hamerstrom (1936:308) are of interest. Speaking of bobwhite quail (*Colinus virginianus*) in winter they say:

"In its simplest form carrying capacity may be said to denote the upper limit of survival possible in a given covey territory as it exists under the most favorable conditions."

Later they state (309):

"The definition of carrying capacity may perhaps be restated as the level beyond which simple predation upon adult birds, their own territorial tolerances, and their tendencies to depart from coverts overcrowded with their own or some other species, do not permit continued maintenance of population."

Reference to "upper limit of survival . . . . under the most favorable conditions" suggests Leopold's phrase "maximum density." Inclusion of territorial tolerance and predation in the second definition gives it the wider meaning inferred in Graham's (1944) statement cited above.

The great importance of social interactions in limiting the number of animals within an area has been illustrated by work on rats (*Rattus norvigecus*) (Calhoun, 1949, 1950, 1952). Hodgdon and Hunt (1953) in defining carrying capacity for beaver also emphasize social tolerances when they say (p.7 3):

> "Carrying capacity, then, is a matter of available food, available water and degree of tolerance one beaver family has for another."

Allen (1954) gives no definition but uses the term frequently.
He states (p.44):

> "Within limits a trained observer can make a fair-or-better estimate of what is likely to be a productive area for species he has worked with, but the final proof is *what it is actually supporting.* The biologist's term for this is 'carrying capacity'."

Again he states (p.132):

> ". . . . in the North the carrying capacity of a land unit usually declines during the cold season . . . ."

Also (p. 259):

> "In a given unit of range there probably will be an optimum density level where the population of prey animals will be reasonably safe from predation (i. e., the carrying capacity phenomenon.) But variable factors, such as weather, may alter the condition of this range unit, for better or worse, in a given year."

On page 138 he states:

> "Carrying capacity of deer country usually depends upon available food on the winter range."

It is clear that Allen's concept of carrying capacity is broad. He regards predation in relation to carrying capacity much as do Errington and Hamerstrom (1936). While food is acknowledged as the main limiting factor for deer, he recognizes that a number of factors may determine carrying capacity for other species. Of special interest perhaps is the statement from page 44 suggesting that the final proof of carrying capacity is what the area supports. He follows this statement by noting that an area with one quail per acre has a high carrying capacity for quail, but the same area may have a low carrying capacity for turkeys.

These examples will illustrate the general conception of carrying capacity as it has been developed in the literature. There is agreement that for various reasons it is impossible to crowd more than a finite number of animals into any unit of environment. This number will vary with the conditions for life existing in the unit concerned. It is not clear, however, whether all or only some of the many factors tending to limit animal populations should be included as factors determining carrying capacity. Some have stressed the importance of "quality" of the animals. It has been recognized that the ability of environment to support population varies from time to time, leading to fluctuations in population. The fluctuating nature of carrying capacity has not, however, been universally recognized, particularly by some earlier writers who inferred that it was a more or less stable attribute of environment.

WHAT IS CARRYING CAPACITY?

Our review of the development of the concept of carrying capacity has shown that the original ideas were related to the role of such factors as climate and food-supply in controlling populations. Gradually, however, there has been a shifting of emphasis from a point of view holding that single factors, such as food, are the main controlling factors to a more comprehensive view in which it is recognized that: "The relationships of a population are with the whole ecosystem (which includes itself) rather than with the environment only." (Solomon, 1949:31; Tansley, 1935). The fact that populations do not grow beyond finite limits is the result of the limited capacity of the ecosystem to support organisms.

In view of the complexity of factors influencing populations it is impossible to make a generalization as to the factors which determine carrying capacity since these will vary with time, place and the species concerned. If, however, we include all the factors in the ecosystem which tend to limit populations as factors determining carrying capacity we are led to an apparent paradox; namely that the number of animals present at a certain time in a unit of environment within the main geographic range of the species concerned is in itself a measure of carrying capacity at that time or at a previous time. For example, the number of quail in an area in spring may be a measure of the carrying capacity of the area during the most critical period of the previous winter. Conditions in spring may be favorable to the maintenance of more quail than are present, but in the absence of reproduction or immigration the population remains below carrying capacity.

Hence carrying capacity may be expressed simply by the number of animals in a unit of environment, except when time has been insufficient to enable increase when it is possible and except where the distribution of the animals is such as to leave some parts of inhabitable environment vacant. The *desired* population may be smaller or larger, and it is the business of management to establish the desired levels through manipulation of the appropriate manageable factors in the ecosystem. Our approach leads us to conclude that management must deal with carrying capacities in order to control populations and not, as sometimes thought, with populations in relation to one carrying capacity.

For a given population in a unit of environment there are a number of factors and processes which are potentially capable of placing an upper limit on its size. Several of these may be acting simultaneously as, for example, in a population which is increasing. As the population density increases the food-supply may be gradually reduced, competition for space with associated intraspecific fighting will become keener, and there may be an increase in disease and predation. Some animals will be eliminated in fights, some may die as they are forced out into inhospitable habitat, some may starve, disease may kill some, and predators may take an increasing proportion beyond a certain threshold of density of prey. Severe weather may eliminate others. The net result will be to limit the population, but no one factor may be responsible.

On the other hand, the various factors may act separately in the sense that one may become critical in the manner described by Liebig's Law of the Minimum. Taylor (1934) suggested a somewhat similar idea when he restated the law to apply to environmental factors generally as follows (p.378):

> "The growth and functioning of an organism is dependent upon the amount of the essential environmental factor presented to it in minimal quantity during the most critical season of the year, or during the most critical year or years of a climatic cycle."

The role of factors operating at a minimum are also considered by Hubbs and Eschmeyer (1938) who, in speaking of management procedures say (p.21):

> "All the essentials of a large fish yield should be provided; none may be omitted, because the lack of any one is sufficient to hold down production."

Later they say (p. 23):

> "When the least developed or most limiting factor has been built up and has increased the fish production, some other factor

may then become under-developed in terms of the increased population."

Even here, however, it is virtually impossible to think of a factor acting independently of other factors or processes in the ecosystem. Indeed, as Rübel (1935) points out, factors operating at a minimum will have varying effects depending upon the other factors operating with them.

However, for purposes of management it is useful to approach the concept of carrying capacity from the point of view that populations are, in the last analysis, limited by some factor operating at a minimum. This approach focuses attention upon more or less measurable and manageable factors instead of complex environments regarded as entities. The latter can lead management into useless activity, improving environmental conditions which could be limiting, but which are not limiting in the situation at hand. For example, if 200 deer attempt to live in an area in a year when there is winter food for 100 and shelter at the height of winter for 300, and if we assume further that hunters will harvest 50, it is obvious that improving shelter or restricting hunting will neither maintain nor increase the herd. This can be done only by increasing the food. One detrimental factor alone can limit a population to a low level in what is otherwise superior habitat. Moreover, if the 200 animals are consuming their food supply faster than it is being produced, and the food supply cannot be increased, the aim of management should be to lower the level of critical operation of some other potentially limiting factor so that the herd is reduced to a number in balance with the food supply. Increase of the harvest to 100 would reduce the population to the desired level.

But what of a definition of carrying capacity? It is clear that populations are maintained by periodic recruitment of new organisms through reproduction or immigration. In the intervals between periods of recruitment there is a tendency for populations to decline because of many decimating factors which are operating against them at all times. These vary in the intensity of their effects with both time and density of population. For example, a population of ring-necked pheasants (*Phasianus colchicus*) may be reduced to half its size in winter because of shortage of food and shelter aggravated by severe weather. Again, a dense population of muskrats (*Ondatra zibethica*) may develop in summer only to be reduced later by a reduction in water-level. Here water-level is the critical factor acting in accordance with Liebig's Law. These examples demonstrate that the most critical conditions for survival occur at intervals—in the cases cited, once a year. Thus, there may be definite periods of time in the life of a population in which the most critical controlling factors operate and it is in these critical periods that the population is reduced to minimum.

Hence, for practical purposes we may regard carrying capacity as represented by the maximum number of animals of given species and quaility that can in a given ecosystem survive through the least favorable environmental conditions occurring within a stated time interval. For practical purposes this time interval is usually one year. This number of animals is an expression of the interaction of the properties of the species concerned and the total environment in which it lives. There is, of course, a maximum number of animals that can survive in the unit of environment under conditions existing at any given instant which might be called the current carrying capacity, but this does not seem to be as useful a concept as that for carrying capacity as we have defined it.

Perhaps a definition is not important in itself. The really important thing is to recognize that carrying capacity is not a stable property of a unit of environment but the expression of the interaction of the organisms concerned and their environment. Moreover, the carrying capacity of an ecosystem may fluctuate in response to the ebb and flow of interactions going on within it. The concept developed here requires management to determine, within the framework of local conditions, critical factors and periods in which they operate to limit populations, and to direct its energies into channels which will actually affect the operation of these critical factors.

SUMMARY

A review of the term "carrying capacity" as used in the past and at present reveals serious differences of opinion as to its exact meaning. Some definitions are almost meaningless, some confine the term to food while others include whole environments, and some, particularly earlier workers, consider it as a static attribute while it is known to be dynamic.

We conclude that carrying capacity is determined by the whole environment, and that, with some reservations, the number of animals upon a unit of range is in itself a measure of the carrying capacity of that area.

Factors in the environment should be considered separately. These usually operate as in Liebig's "Law of the Minimum," the most critical factor being the major control on population. The role of management is to recognize and work with critical factors to produce

desired populations. "Our approach leads us to conclude that management must deal with carrying capacities in order to control populations and not, as sometimes thought, with populations in relation to one carrying capacity."

ACKNOWLEDGMENTS

For contributing to discussion essential to the evolution of this paper and in some cases for commenting on the paper in its various revisions, we are indebted to: P. J. Bandy, I. McT. Cowan, F. H. Fay, C. J. Guiguet, R. L. Hepburn, H. Lumsden, D. J. Robinson, R. C. Passmore, R. O. Standfield, and M. D. Udvardy. Since our critics did not always agree with us, the views expressed in this paper are, of course, the responsibility of the authors.

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DISCUSSION

DISCUSSION LEADER PETERSON: It seems entirely appropriate that we should end this big game session of this Twentieth North American Wildlife Conference with a paper calling for a re-examination of a concept that many of us have used, unfortunately, rather loosely.

The paper is now open for discussion.

MR. PIMLOTT: It has been proposed that this carrying capacity definition be adopted. I feel that would be essential and necessary to adopt new terminology to describe more adequately the relationships of big game populations to their range. In the case of forest big game species, if we cover only the survival of the animal, we have no method of describing whether the animal is damaging the forest reproduction in any area. This is possible under Dasmann's definition of carrying capacity.

I wonder if it would not be more desirable to confine the term more narrowly, rather than to give it such a wide definition that it would have no practical use.

DR. FOWLE: I think I would agree at least in some measure with Mr. Pimlott. Our attempt here, as we indicated, was to try and find some general common ground. We feel that the important thing is that you have to specify what environment you are talking about and what quality of animals you are talking about.

I am sure that everyone here knows that when you say that animals have exceeded their carrying capacity, it's a rather paradoxical statement. If there is a carrying capacity that limits the number of animals, and then you have a whole lot more animals—well, it's a rather absurd conception.

What you really have is another kind of environment. After the supposed carrying capacity has been exceeded, you move to a deteriorated environment which is different from what you had before, and that's why we feel that a dynamic view is necessary.

I would suggest that in the case of big game and forest species generally that it might be desirable to consider desirable rates of stocking and not look at the matter in terms of carrying capacity at all.

DR. IAN MCTAGGART COWAN [British Columbia]: The matter that Dr. Fowle just brought before us, that of the peculiar situation you get into if you define carrying capacity and say that you have an excessive population only arises if you depart from the initial idea that capacity was involved in food. However, I can see considerable merit in the suggestion that Fowle and Edwards have made, but I also suggest that if we use carrying capacity as it is suggested, we need to have something else as well. We need another term that we may call, let's say, ultimate capacity. We need an ideal to shoot at in terms of management, because if you say that this population represents the capacity, and this animal is at capacity, then that immediately removes your ideals, aims, and objectives.

In other words, when you are attempting to manipulate your environment to increase the number of animals, you are shooting at an ideal which you must set as the capacity of this range to support animals when you have done everything humanly possible to improve the range's ability to support those animals.

I would suggest that if we use this, that we must immediately adopt this other idea of an ultimate capacity that can be reached by a good many of us.

DISCUSSION LEADER PETERSON: Perhaps we might employ the word ''population optimum.'' That's equally difficut to define, however.

MR. LOUIS A. KRUMHOLZ [Lerner Marine Laboratory, Bimini, Bahamas]: I hesitated to come here simply because we have batted this thing around in fisheries for quite some time, and this was pretty thoroughly discussed back in the late 1940's when we were talking about carrying capacities in the fish population.

Now I agree, and I think that Mr. Fowle has a good definition of carrying capacity when he says that it should be the population that an environment is capable of maintaining at the most critical time of the year; but I do not believe that carrying capacity fluctuates as he says it does. I would rather say that carrying capacity is pretty much of a stable thing, and the standing crop of animals in the population will fluctuate up and down depending upon the season of the year.

To use a silly example, a quart milk bottle will hold a quart of milk, but if you raise the temperature of that bottle it will hold more than a quart. If you lower it, it will hold less than a quart. The capacity is a quart, but the amount in the bottle will fluctuate as the temperature varies.

Now, food will do the same thing in our area, and I'm firmly convinced from our studies on a good many small lakes that we do know that the number of fish, whether it's total weight in pounds per acre, or however you measure it—is smaller in a lake in the spring of the year, and this applies virtually to all the fresh-water lakes in the world, I imagine—than there are in the fall of the year.

The reason for that is that the fish do not recruit to the bottom of the lake during the winter, but they do in the summer, and this is a change in the size of the standing crop, and not in the carrying capacity of the lake.

DR. FOWLE: I'll have to go and look at a quart of milk, I guess, after that.

That's a very interesting comment, and I thing it brings out something that perhaps we do not emphasize sufficiently. I think Edwards and I would feel that the fish in the pond, if they were fewer in the fall than in the spring—which is certainly the case—the reason for that was that factors had operated against them during the year, and that had there been no mortality factors the numbers of fish would have remained the same.

In other words, we are trying to include everything in the environment, including social interactions of the animals, and perhaps even some of the conceptions that were described here yesterday by Dr. Christisen, and some of the other people who were working with lemmings, and so on. However, I feel that the comments of Dr. Krumholz are well taken. This matter isn't cleared up at all.

DISCUSSION LEADER PETERSON: I think we'll all agree that if he has done nothing else he has stimulated some interest here and some thought, and I see no reason for cutting off the discussion. Do we have a little more?

MR. JAMES BRUCE FALLS [University of Toronto]: I wonder if this new definition is so broad that it includes hunting pressure, and if we are going to adopt such a broad definition whether perhaps this invades the field of already established terms like ''standing crop,'' and ''population.''

I think that perhaps the main idea of the old usage of carrying capacity was standing potential, and perhaps it should be limited to food potential.

DR. FOWLE: We're going to get into a lot more definitions here before we're finished, but I think Dr. Falls has brought out a good point. It simply is a matter of deciding what the definition is going to be. If you don't want to include hunting, you have to think of another term to deal with that.

I still feel that the conception of desirable stocking is one which management should look to. I suggest that there is a slight difference between the definition that we have suggested, in that it perhaps has real biological basis, and it is not perhaps developed as a result of some desire on the part of management; whereas the concept of desirable stocking is something which you make up your mind about and develop procedures which will give you the stocking which you want. It may be much less than the actual number of animals which a segment of the environment will hold, but nevertheless it's what you want, and your program should be geared to that.

DISCUSSION LEADER PETERSON: Who has the next question?

MR. ELSON [St. Andrews]: I'm another one of these fish men. I'd like to make a brief reference to a paper which is going to be given at the forthcoming meeting of the Northeastern Branch of the American Fisheries Society in Atlantic City.

A gentleman from our station in St. Andrews is going to put forth something on the carrying capacity of the small trout streams there. I'm not sure of his definition in terms of words, but I think his practical definition is a very useful one. It will be the mean over a period of eight years of the number of fish which were encountered within the area each year.

It seems to me that these things like carrying capacity are useful particularly if we think of them as a broad band, rather than as a solid line. We have something then that we can work with.

My own particular interest has been with Atlantic salmon. We have had a lot of population work done within a relatively short period of time—I talked a little about it yesterday morning—and there the stream seemed to have a certain carrying capacity for young fish under what might be called natural conditions.

We were able to change that not by raising temperatures or anything like that, but by applying predator control, and then the thing that I think we might call carrying capacity increased by about eight times, so that it is a fluid sort of thing, and I think we should perhaps guard against too hard and fast word definitions to keep these terms useful.

At the high end of the scale I think we do need something like the ultimate carrying capacity that Dr. Cowan mentioned.

DISCUSSION LEADER PETERSON: Any further questions?

DR. W. H. ELDER [Missouri]: I should like to have us remind ourselves in regard to this definition that there is a need to reflect that carrying capacity is the number which a piece of range or a stream will support without deterioration of that range due to the population itself.

MR. KRUMHOLZ: Two years ago I was placed on the Terminology Committee for the Society, and we have had a great deal of difficulty with this term "carrying capacity," as well as "standing crop," "productivity," and so forth. Right now I feel that it's up to us to define this term so that it is pretty much of a standard term and can be used by everybody to mean the same thing. If we have different people believing that it means different things, we might as well redefine the words in the dictionary.

If we adhere to one definition, I think it ought to be that it is the most that any environment will support during the most critical time of the year. If we do that, then we know that we can rely on that environment to support that year after year, unless we have something like a tremendously heavy snow or a tremendous freeze-up in the lake, or there is a heavy ice-cover leading to a diminution of the oxygen or the introduction of undesirable cannibals in the water. Then I think we can say that the carrying capacity went haywire this particular year because of the conditions this particular winter; but we must have a stable definition of the word, and then use another term to definite the population itself, whether its standing crop or something else. I don't care about that, but the term carrying capacity is no more than a unit of measurement which the environment is capable of supporting at the most critical time of the year.

DISCUSSION LEADER PETERSON: Do we have further comments?

MR. CHARLES R. HUNGERFORD [Arizona]: Along with Dr. Cowan's point on the difference between carrying capacity and ultimate carrying capacity, I might ask Dr. Fowle if he includes in the definition the number of animals on an area per unit of area at any given time, how he might define density—if that is the case.

DR. FOWLE: Well, I'm not sure that I get the exact gist of this question. Do you want a definition of "density"?

DR. HUNGERFORD: Yes.

DR. FOWLE: Well, I'll have to look at this definition to see if I'm stuck here. At first glance, I wouldn't say there is anything the matter with the old mathematical definition of density, being the number of animals per unit of area or volume or whatever it is you are dealing with. Is there a difficulty here that I'm too dense to see?

DR. HUNGERFORD: I believe you included that in your definition. You said any unit at any one time. Your definition starts with, "if at any one time the numbers per unit vary."

DR. FOWLE: No, the definition is that carrying capacity is represented by the maximum number of animals of a given species and quality that can in a given eco-system survive through the least favorable conditions occuring in a stated time interval.

DISCUSSION LEADER PETERSON: Any further comments?

MR. FRANKLIN C. DAIBER [Delaware]: I don't have any proposed definition for carrying capacity. I would like to make a couple of comments, however.

There have been a couple of ideas expressed here that somewhat confused me as to what is meant by carrying capacity, and one of them is this. We talk about limiting factors, controlling factors, and so forth. We talk about temperature; we talk about nutrition, and so on.

I would just like to call attention to the fact that regardless of how we name it, whether it be temperature or food supply, as long as it has the same effect on the organism to control, to depress, or increase the individual species' population, that it should be considered as a limiting factor.

Now, a controlling factor is something else again. As long as it has some effect on the population, regardless of how it works, I think it should be considered as a limiting factor.

Then in terms of carrying capacity, there was an expression from the man from New Brunswick, and I have heard it also in several fish papers, being a fish man. We talk about the carrying capacity of a fish pond in terms of bluegills, or the carrying capacity of some bay in terms of the number of flounder that are present; but I'm not so sure that that is what we want, because if we are talking about removing predators to increase the size of the flounders population in the bay, we are increasing one population at the expense of the other, and I think we can think of all kinds of examples of control of competitors, or predator control, or some means of direct action.

I think we have to consider the total and not just one population or species.

DISCUSSION LEADER PETERSON: Do we have any further comments or questions? [There were none.]

I think this paper has been highly successful, and I'm extremely pleased that we got as much comment and thought on it as we have.

CHAIRMAN WATKINS: I think the fact that nearly 100 of you so long delayed your lunch is rather good testimony that at least this last paper was an interesting one.

I share Dr. Peterson's feeling in that, so I think if there are no further questions, we can consider ourselves adjourned.

TECHNICAL SESSIONS

Wednesday Morning—March 16

Chairman: DAN SAULTS
Chief, Information Section, Missouri Conservation Commission, Jefferson City, Missouri

Discussion Leader: BUD JACKSON
Director, Field Activities, National Wildlife Federation, Springfield, Missouri

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CONSERVATION INFORMATION OPPORTUNITIES

LET'S USE RADIO

WILBUR D. STITES
Department of Conservation, Springfield, Illinois

Mr. Chairman, ladies and gentlemen. It is always a privilege to take part in any discussion on conservation education. I am especially honored to have been selected as a panelist to present to this group some ideas about radio as a medium for spreading the conservation message. In Illinois, we have achieved a considerable measure of success with our weekly radio program, Outdoors in Illinois, and I hope that my experience in conducting this program has equipped me to pass along to you some ideas which perhaps will benefit you in putting radio to advantageous use in your own state or province.

These days, in any discussion about radio programs, it seems that the most common remark you are likely to hear is, "I've got television now and I never listen to the radio any more."

This may give rise to the conclusion that radio is a dying medium and that it is just a matter of time until the radio set will go the way of the Model T Ford. However, this is far from true as a look at a few facts will show.

For instance, the ratio of radio stations to TV stations in the country right now is better than seven to one in favor of radio. What's more, the number of radio stations is growing constantly. Prior to

World War II there were 900 radio stations in the entire country. Today there are more than 2,500, and new ones are springing up right along. These stations are broadcasting to 111,000,000 home radios and 27,000,000 car radio sets throughout the country.

The fact that people still spend a good portion of their time listening to radio despite the growing popularity of television is shown in statistics compiled by the various poll takers. According to these polls, the average home set owner listens to radio at least three hours a day. And this is the case even in homes which have both television and radio sets.

One big advantage that radio has over TV in the battle for the public's attention is that radio is available practically anywhere you go. Today two out of three automobiles are radio equipped. Certainly, automobile television is not yet available. And considering our present highway accident rate, I might well add, "Lord help us if they ever do put TV in cars."

I know you've all heard the favorite and oft-repeated plug of the radio industry—"wherever you go there's radio." And with home, car, and portable sets available, this phrase surely sums up the picture accurately.

Now all this is not to condemn television. I would be very foolish indeed not to recognize the popularity and value of television as a medium. In fact, we are now in the process of preparing to put on film a series of weekly TV shows which will follow the pattern of our radio program. But the point I want to make here is that, even with TV at their elbows, people still love and listen to radio.

The important thing to us is that radio time is available for our use. The Federal Communications Commission, which issues the necessary permits under which radio stations operate, stipulates that each station donate a certain portion of its time to public service. The 46 stations now carrying our program, Outdoors in Illinois, devote an average of 25 per cent of their time to public service. Non-commercial educational programs are classed as public service, and under this classification time is available to conservation agencies.

The best part about it is that this time is free. All we have to do is use it. And by simply using this free time, we can benefit at the rate of thousands of dollars worth of free publicity a year. Our Outdoors In Illinois program is a weekly 15-minute feature. Computed at a rate of a dollar a minute, which is considered a standard average rate, we are realizing an annual total of $35,880 worth of free publicity on the 46 stations carrying our program.

Any of you can do the same thing in your own state or province. If you don't do it you're simply wasting thousands of dollars of valuable radio time every year.

Contrary to what you might believe, producing a weekly program is not a highly involved project requiring a host of professional personnel. One man can do the job easily. I know this from experience because I have been conducting our program in Illinois alone for more than two years and I still find time to get some other work done besides.

There are many ways in which you can produce a program. I shall list three possibilities here.

One method is to mail prepared scripts to the radio stations to be read by their own staff announcers. This method is little different from the regular news release service which many of you now conduct for distribution to the newspapers in your state or province. A second method is to send members of your own field force to the radio stations in their areas to read prepared material or to be interviewed by station announcers. The third method is to build up a network of stations whom you regularly service with a complete transcribed program on tape or disc.

The first method is of course the easiest and simplest to use, but it also falls the farthest short of getting the kind of job done that you would like to accomplish. Its greatest drawback is that the script runs the risk of being edited or condensed to such an extent that the import of the message you wish to convey may be lost. In fact, if the press of other news is particularly heavy your entire script may wind up in the wastebasket instead of on the wave-length.

The second method has some decided advantages but it also has some disadvantages. On the credit side of this method is the fact that it gives your program a local appeal to the listening audience since the person presenting the material is your own representative in that particular area. In some cases you probably would find that your field agents would be able to substitute actual local specific hunting and fishing news in place of the rather general material in the prepared script which you furnished them. On the other hand, this method stands a good chance of missing the mark mainly because it depends on too many widely distributed people to get the job done. In any case of this kind there are bound to be slips.

The third method listed is the one we use in Illinois and, naturally, it is the one which I most highly recommend. Under this method a man working out of the central office is responsible for producing and conducting the program. By having a single individual presenting the program via transcription on every station in your network you are assured of getting a uniform message across to all your listeners on each and every program. This means that, when necessary, programs can be audited before release thereby preventing any possibility of misinterpretation of material relating to policy or other important phases of your particular department or commission administration. In other words, with this method every station in your network is carrying your message in the same way.

Also, the fact that the program is being conducted by a representative from your central office tends to lend it a note of greater authenticity. Then too, the voice familiarity which is built up by having the same person conduct each program is an important aid in attracting and holding a large listening audience.

To give you an idea of the number of people you can reach with a radio program of this kind, our estimated total listening audience on the 46 stations carrying Outdoors In Illinois is 5 million. When you consider that the population of the State of Illinois is slightly more than 9 million, this figure of 5 million listeners becomes rather impressive.

Production costs under this method are, of course, greater than with the other methods listed since the recording equipment needed to produce a program of this kind is rather expensive. However, after the initial outlay for equipment is completed, production costs become negligible in comparison to the net worth of the program publicity-wise. Use of magnetic tapes for distribution of the programs helps to hold costs down since these tapes can be used and re-used indefinitely. We insist that our stations return the tapes to us as soon as they have used them on the air so that we will have them available for recording future programs. We pay all postage charges.

Personnel costs are of no consequence since it is not necessary to employ professional radio writers or announcers to conduct a program of this kind. Anyone who can write an acceptable news story and speak reasonably correctly and distinctly can do the job. Pronunciation and enunciation are important and voice or speech training certainly is an asset to the person who plans to conduct the program. But, I feel sure that every state or province has within its department or commission staff at least one man who can fill the bill.

If you are to attract and hold an audience you must give the listeners the kind of program they want to hear. Fortunately, conservation offers a wealth of varied material which has an almost universal appeal. You'll find that your listeners are eager to hear anything you have to tell them about conservation in your area. Actually, the very kind of conservation program you are trying to sell will, without doubt, be exactly what they want to hear. More specifically, they want to be told how they can improve their skills or techniques in the field or on lake or stream, and what their state or province department or commission is doing to improve outdoor recration.

It is equally important to satisfy the stations in your network since the success of the whole project hinges entirely on getting the stations to air your programs. Remember that there are many national, state, and local organizations and agencies competing for the free time which the radio stations devote to public service.

Probably the most important thing to the stations is timing. Radio stations operate on rigid schedules which are timed to the second, and a program director or station manager has small patience with a program which fails to arrive in time for the spot he has set aside for it in his carefully planned log. It is essential that you set up a regular schedule for release of your programs. It is also essential that each program in the series conform to a definite time length. Our Outdoors in Illinois programs are timed at 13 minutes, 45 seconds. This enables the stations to fill a 15 minute spot and still allow time for a station break, weather report, or commercial at the beginning and end of each program.

Stations and listeners both want programs which are entertaining and informative. These two qualities must be incorporated into each and every program if you are to build and hold a listening audience. And it is only by building and holding a substantial listening audience that you can expect to keep your program on the air.

DISCUSSION

MR. HOWARD SHELLEY [Michigan]: I enjoyed very much the comments of the speaker. I should like to add this bit to it. We in Pontiac have used the same system you have prescribed, not from the state-wide standpoint, but just our own local sportsmen's club. We spread the good gospel of conservation in our own local area much the same as you do state-wide. We find it worked very, very well.

DISCUSSION LEADER JACKSON: Thank you, Howard.

MR. ROBERT DAVIS [Ohio State University]: First, I would like to know just what you are aiming at in this radio series. That may be a little on the academic side, but I would like to know what the specific objectives are in getting this information out, and if that has a bearing on the type of information you disseminate.

MR. STITES: Our objective is to disseminate information about our Conservation Department to as many people as we can. Through this radio program we feel we are getting that job done. Actually, in Illinois we have just about reached the saturation point in the number of hunting and fishing licenses we can sell.

So now we are trying to educate those people who buy those licenses, how they can best use them to get the most enjoyment out of them. We are telling them what we are doing in conservation to make hunting and fishing better for them.

MR. DAVIS: What time spots do you find your show occupying over the various stations?

MR. STITES: You make me very happy by asking me that question. We have stations carrying our program on Class A commercial time which is very tough to get. I don't know of any educational service in Illinois which is being given that kind of time. Other stations which are not carrying our program on Class A are giving it the spot they feel is the best one to reach the kind of audience that we are aiming at.

MR. K. THOMPSON [Montana]: I would like to know if you have any problem in areas where you get competitive stations with just one program; that is, stations running the same or similar programs. Do you run into that?

MR. STITES: Yes. But, really, that is an asset rather than a problem because the competitive program is building a listening audience, too, and thus people are anxious to hear more about hunting and fishing and about conservation than our competition can give them. There is no one outside of a state department or a commission agency who can try to develop the themes that the people in that agency can give them. So these competitive programs are actually helping to build our radio audience, because they hear what our competitor has to say, and they want to hear more.

MR. THOMPSON: But are you in a position occasionally of being asked to produce two programs—two networks asking for a conservation program and you are currently only supplying one? Do you run into the situation where a competitive network asks for your conservation program?

MR. STITES: Oh. yes, we have that. We have three stations in Springfield that carry our program, two in Peoria, and so forth, and that is very fine.

MR. THOMPSON: You give them the same program?

MR. STITES: Yes, the very same. They welcome that. Even though they are a competitive station they welcome the program. They are very receptive to it.

MR. HENRY DAVIS [Connecticut]: I would like to ask if your material stays with conservation education or is it entertainment type of material?

MR. STITES: Henry, I am glad you asked that question. If I had not been so slow, I would have gotten to that. It seems if you are going to build and hold a listening audience, you have got to give them the kind of material they want to hear. Unless you can hold your audience, the station is not going to want to carry your program. We have got to temper education with entertainment, and that's what I try to do. I try to make my programs interesting, entertaining and informative. I think those qualities fit together rather well with conservation and can be used with each other and that's the way to work it.

MR. FARLEY TUBBS [Michigan]: Do you have any difficulty—or what is your policy—regarding sponsorship of the program? For instance, finding a place in the station's network within the program's schedule, and then do the stations come to you with a request such as this: "We would like to put this on with a sponsor who would like to sponsor this program." Do you have that problem at all?

MR. STITES: No. we have never had anyone come and want to sponsor that amount of time. We cut each tape 13 minutes and 45 seconds to fill a 15-minute spot which still allows time for a commercial, station break, a weather report, or whatever they want to use at the beginning or the end or both. They can use the 15-second spot or 30-second spot at the start or end of the program. They can sell it to a beer company, a sporting ammunition or fishing tackle company, or whatever. They can do just as they like with that as far as we are concerned.

DISCUSSION LEADER JACKSON: If you will permit me, I would like to offer the observation, as a former radio man, that is one problem that a radio man is seldom faced with. He has few opportunities to turn down a sponsor, at least in the field of conservation, in my experience.

I mentioned a moment or two ago this business of costs. We haven't gotten into it, and I don't know whether we have time to get into it, but if you grant me the privilege of asking a question myself, I would like to ask Wilbur to tell us a little something about this business of comparative costs.

He made mention of the fact that he could produce 52 radio programs at approximately the same cost as two issues of a magazine. I know that is challenging to Dan Saults over here and some of you people, too.

MR. STITES: First of all, I will just break it down a little. You've got to get these tapes to the stations, and then get them back if you want to re-use them, so it requires postage for that. We pay all postage costs, outgoing and return, so we'll be sure to get the tapes back. It costs us $1,291.68 a year—$24.84 a week—for that. We have to have an engineer to make a duplicate copy of the master I record in the field. We pay standard union rates to him, $2.89 an hour. That figures out to about $20 a week or $1,040 a year. The tapes, as I said, cost about $3.50 each. You need about three sets, depending upon the number of stations you are going to service.

You need one set to be at the station being played; you need another set for recording, and the third for a buffer. That costs us for 46 stations about a total of $625. It will cost you less than $3,000 to do the job we are doing. You will get a yearly total of somewhere around $5,000.

MR. DAVIS: Yes, but you haven't said anything about talent.

MR. STITES: We don't pay anybody to appear on the show. [Laughter] My salary is being paid whether I do radio or whether I don't. I also do magazine work, and so forth. You have someone in your department who can do the same thing. He can do radio as a sideline and still make it a full-time job, too.

LET'S USE TELEVISION

JAMES R. HARLAN

State Conservation Commission, Des Moines, Iowa

What would be the impact of the Sermon on the Mount if it had been filmed in sound and televised into the homes of the earth each Christmas for 1900 years?

What has been the effect of Bishop Sheen, Senator McCarthy, "I Love Lucy" and Captain Video as they pour their stories out of the television tube into the living rooms of 60 million American homes day after day?

Church, industry, the armed forces, and many others have accepted this phenomenon for what it is, the world's newest and fastest growing sales medium. They are telling their stories effectively to millions of receptive Americans each week. Conservation, on the other hand, is approaching this near perfect mass medium with extreme timidity.

Conservation has some very heavy artillery in the TV battle for American minds and support. We have uniquely available to us superb TV props in fish, wildlife, trees and waters. These strike a responsive chord in every individual. We have the trained people who work with and understand these photogenic natural resources.

Combined, they would, on television, tell the conservation story as it has never before been told.

The Iowa Conservation Commission is using television extensively. Beginning in April, 1954, we offered to all of the stations within the state and those outside of the state with major Iowa audiences, the first of 13 black-and-white quarter-hour motion pictures especially designed for television.

In our first offer we asked the 13 stations to pay $10 per program for film rental or $130 for the series. About half the stations agreed to the charge and secured sponsors. In order to secure complete coverage, the rental charge was withdrawn before the series went on the air and every station then accepted the programs for television showing. Each week the films were made available for simultaneous release at 6:00 p.m. Monday, but because of staggered schedules 10 prints of each film were sufficient to meet commitments.

The impact on the viewers was felt immediately. At the conclusion of the spring series the television stations were unanimous in their request for additional programs. A fall series of 10 new films were promptly accepted when made available.

It is impossible to tell how many people saw each of the 23 shows. The area blanketed contained well over 6 million people. Nationwide, 60 per cent of the homes have one or more television sets. We had good time on most stations and, because of staggered schedules and overlapping of station ranges, our programs had from two to four shots at the television audiences. In addition, the films are now being used for sportsman's clubs and other groups. Recently the Army secured these films for overseas TV use. Those that are not "dated" may be used again on domestic television.

Our format is simple. We have a standard studio opening, a two or three-minute interview with the department expert in the field to be presented, followed by the outdoor movie subject matter described or narrated by the expert, then a standard studio close and credits.

The interview and the expert's voice explaining the motion pictures, are recorded on 16-millimeter magnetic tape and re-recorded on film. The film sound track is synchronized with the picture negatives. Any number of high-quality prints can be made for release to the stations. This is known as double system sound and is far superior to single system sound.

The subject matter for our shows has been selected from a list of more than 100 titles submitted by personnel. Pictorial quality and limitations of photography must be kept in mind when selecting subjects. Unity of time, place and action are very desirable in shooting these newsreel-type movies for they may be on the air within 10 days. Because of the time factor the action takes place in a limited area.

It must be kept in mind that we are in hot competition for the television audience when our films are being shown. Most viewers have choice of several programs and will turn their dial at the beginning of the show if the opening indicates dullness or amateurishness.

If we, in conservation, build our programs around birds, animals, outdoor recreation, and the other spectacular props under our control, we can have great audience appeal. *We must keep the viewer entertained as our first objective, or we will be talking to empty air.* We must keep our conservation sales messages interwoven with interesting, fast-moving outdoor drama. In hundreds of shots we can show habitat development, interdependence of wildlife, good soil practices, wise water use, and other basic phases of conservation, all without preaching a gospel of doom. If we do, we can and will reach an audience of millions that we have never reached before.

The following titles to a few of our shows indicate the range of subject matter and the pictorial possibilities. They also reflect the philosophy "entertain the public first." The Spring Goose Flight, The Warden At Work, Water Safety, Our Common Reptiles and Amphibians, Fly Fishing the Farm Pond, King Catfish, The Man in the Park, Gun Safety, The Conservation Commission in Action, Our Furbearing Animals. In all films, *the viewer is presented some basic conservation fact or concept or informed of a Conservation Commission program or problem.* But in making the shows entertainment is foremost in our mind. We can not force feed the TV audience statistics and gobbledegook or it will seek greener fields on the dial.

Costs of television production frightens the uninformed, yet any conservation department can start television operations along the Iowa plan with an initial annual budget of $15,000 or less, exclusive of studio construction and personnel costs.

Three major costs must be considered when setting up a new television program: Equipment, production, and personnel.

Our Iowa equipment can be replaced for $6,500 except for our studio. (Table 1)

Studio construction costs are variable, depending on whether suitable unused space is available. Starting from scratch, a frame construction studio should be built for $5,000 or less. Our 20-by-36-foot studio with 16-foot ceilings was built in the balcony of our permanent exhibit building for less than $500.

TABLE 1. TELEVISION EQUIPMENT COSTS

Motion picture equipment purchased prior to TV work:		
Title bench	$ 300.00	
Eastman Cine Special Camera and lenses	1,600.00	
Professional Jr. tripod	125.00	
Bell and Howell viewer	100.00	
Neumade rewinds	30.00	
Griswold splicer	30.00	
Film synchronizer	260.00	
Total	$2,445.00	$2,445.00
Equipment purchased especially for TV:		
RCA microphones:		
77-D	$ 144.50	
BK-1A	79.50	
Cord	6.00	
Plugs	3.10	
Brush earphones	7.20	
Western Electric monitor speaker 11″	35.65	
Kinevox bulk eraser	70.00	
RCA synchronous projector	600.00	
Bolex Pan Cinor lens	358.00	
Auricon Cine Pro camera	1,299.00	
Magnasync 16 millimeter magnetic recorder	1,224.75	
Fourteen light clamps	35.00	
Two converters (color tran)	239.00	
Carter converter (rotary)	191.25	
Two 6-volt batteries, storage	30.00	
Schoen squawker, magnetic and optical	160.00	
Reflector bulbs (24) Par — 38 about	30.00	
Stopwatch	12.00	
Sixteen millimeter magnetic tape	100.00	
Reels, can, shipping cartons	400.00	
Total	$5,024.95	$5,024.95
GRAND TOTAL		$7,469.95

There are some variables in production costs from area to area. Our production costs for 13 programs with 10 release prints of each, or 130 prints, was $3,693. (Table 2) These costs include laboratory services (recording, developing, work prints and release prints) cutting service and raw film. (Table 3)

The $5,000 budget for a series of 13 shows with 10 release prints of each show gives us ample safety margin. Our annual $10,000 budget is designed to take care of a spring and fall series and equipment replacement.

Personnel is the key to success in any endeavor where funds are available. Our present TV team consists of five employees of the department, none giving full time to TV production, plus our department guest expert. Our team includes producer, who utilizes about 10 per cent of his time in television; director, about 75 per cent;

TABLE 2. PRODUCTION COSTS OF THIRTEEN TELEVISION SHOWS

Laboratory service—re-recording, developing work prints, answer prints, release prints	$3,008.00
Cutting service	285.00
Original film	400.00
Total	$3.693.00

TABLE 3. PRODUCTION COST OF A TYPICAL PROGRAM

(Fox Trapping)

Original film, Dupont #901, 600 feet	$ 14.40
Developing original negative at .02 cents per foot	12.00
Work print	21.60
Studio film 200 feet	4.00
Developing	4.00
Work print	7.20
Re-recording magnetic track to optical	66.00
Cutting and matching film	25.00
Printing first film print—490 feet at .023 cents per foot	11.27
TOTAL COST ONE PRINT	$165.47
Cost of remaining 9 prints @ $11.27	101.43
Cost of ten prints:	$266.90

sound man, 5 per cent; projectionist, 5 per cent; and script girl, 10 per cent. In Iowa, no personnel was hired especially for television production. We are operating shorthanded and do need one additional man for full-time field photography and film editing.

Personnel costs will vary from state to state. A full-time, six-man crew should have two members in the departments' top pay bracket, two in the middle bracket and two in the lower bracket. At the top level are producer and director; in between, photographer and sound technician; and lower, projector operator and script girl.

Importance of the producer and director in such a setup can not be overstressed, and on their shoulders primarily depends the success of the television production. The producer receives policy directives and objectives from the department high command and is responsible for their execution. He is the TV stations' contact and is chairman of the story conference board. In Iowa, he is also master of ceremonies. Most conservation departments have people in their employ who can qualify for this position, providing they understand the limitations of motion picture photography and have full understanding of departmental programs and objectives.

The TV director is the top technician and is the key to successful film production. He operates the studio camera, directs the story flow, cues sound technician, timer and actors. He supervises film processing and editing, and is a member of the story conference board. The director is Czar in the studio during production. In the Iowa operation, the director also shoots and edits field film and trains and supervises the other technicians. This position is the critical one for most state departments to fill; top-notch men in this field are at a premium.

The projectionist operates the motion picture projector during studio productions, handles the clap-sticks, times studio takes and in Iowa is a member of the story production board. The sound man operates the sound equipment as his sole duty. The script girl takes stenographic notes to aid in editing and final cutting and keeps a running record of scenes, film footage, takes and narration during studio production.

Production of motion picture television film has become an important part of the information-education program of the Iowa Conservation Commission. Although the future of TV is not predictable, it is safe to assume that its influence will become greater and greater. *We will be in television for a long time to come.*

To review then; I believe that television is the world's most effective education medium; television stations throughout America will provide cost-free channels for first class conservation motion pictures; we are able to produce interesting outdoor film; our budgets can afford television film production; and lastly, that we shall be derelict in our duty to the American people if we fail to use this potent medium to show the world the relationship between man and his environment.

DISCUSSION

DISCUSSION LEADER JACKSON: Thank you very much, Jim.

You are looking at a kid whose sales resistance as far as TV is concerned is very, very high. We have been told by economists in this country for a number of years, however, that it is not husbands and fathers who hold the purse strings but rather wives and mothers and children. So we have a fine, big TV set in our home that operates full-tilt.

I know you people have some questions on this subject because this is something that has pretty well captivated America's imagination.

DR. GUSTAVE PREVOST [Quebec]: I would like to seek information for Montreal, the Province of Quebec especially. What would be the average length of time that is best for the average program? We had interviews on those stations in Montreal, and we don't like interviews too much. We would prefer a drama. We had live shows and films, and we do not like the films. We have the feeling in Canada, in competing with other programs, that we have to get as much live show as possible. Of course, it costs much more money. I don't think we should have much more than half films and half live. I would like to have your opinion on that.

MR. HARLAN: We use 13½ minute programs to give commercial time to both the opening and close of the show. We have film in a wrapped-up package that gives the studio 1½ minutes on a 15-minute program for their advertising. I don't use half-hour shows, although I think they would be very effective. If they were half-hour shows they would be 27 minutes long. Television eats up material faster than a growing family eats bread and butter, I can assure you.

We don't use drama in the sense of actors and actresses. We use the outdoor drama. For instance, in gun safety, we actually show our conservation officers teaching school kids how to handle guns effectively and safely. In fishing we show the use of a fly rod, for instance. We do that in the field, and actually it is a drama, but not written out.

It is very, very difficult to get wildlife actors to act as you want them to at the time they are filmed. We use film because we can do a newsreel-type of show, put it on film and get it to each of our 13 television stations with a very limited additional cost. We can show only one live show in the studio. We have done considerable of that, but we have 13 stations to reach, and we could not reach each of those stations in a live program, but we can very easily do it when it is on film.

MR. J. J. SHOMON [Virginia]: I would like to offer this comment for what it may be worth. Each year in the spring of the year there is a television workshop in Washington, D. C., put on by the United States Department of Agriculture. These workshops are really very fine. They last a whole week.

A partial answer to the gentleman who just appeared here—the feeling is among the professionals there at these workshops, that the big answer to TV with conservation departments is film.

I would like to ask Jim Harlan one question while I am on my feet, and that is, are you working with color, Jim, or are you working with black and white? If you are working with black and white, what arrangements are you making to get into color?

MR. HARLAN: We are working only with black-and-white at the present time, Joe. The reason for that is color film in studio work takes so much electricity and so much light. Eastman Kodak is developing color film that will use the same light that you now use in studio production for black and white. We will be ready to do color film. We have done many outdoor color motion pictures, and we will be able to use color film when color television is universal. We are in no hurry about it. We will wait until color film prices will be reasonable and down in the black-and-white range.

There is another point I might bring up here. I was dead set against black-and-white photography for sport club work. At these television shows the sports clubs are using them hand-over-fist, and they will show a color motion picture alongside of one of our black-and-white television shows, and they just eat them up.

MR. CLAUDE GRESHAM [New Orleans]: Do you permit stations to sell your program in order to get a better and more stable listening time? If not, do you have any trouble with getting that stable time? We are having some difficulty in getting our program to run at the same time long enough to build up an audience.

MR. HARLAN: Claude, we have not had so far. We had a little difficulty during our fall series last year; some of the stations ran them just before the football games and some just after. That varied a little. I didn't like it too well. But for the most part we are getting prime time after 6 p.m. in the evenings, and I think we are getting it because the quality of our shows is good enough so that they are tickled to have them at that time. Some of them are getting independent sponsors, but that is no concern of ours. We just provide the film and they provide us the time.

MISS EILEEN MAIR [New York]: In answer to the question of film versus live shows, at the University of Michigan they have a regular hour program on Sunday, and Shirley Allen, whom I believe most of you know, gave a course of eight half-hour shows. The general opinion is that live shows are much better over film always. He did use some film in his shows to explain his points, he had guest speakers every time. Professor Allen discovered that guests made a difference. He had a wonderful personality for television. Everybody loved him. I was connected with him because I tested some seventh grade children on their reactions to the course.

Always use live shows. If an error is made, the audience enjoys it. They know it's an error, and they have more interest. Also, with live shows, with your guests something will come up that you never expected to come up, and it makes the show much more interesting. Visual aids are very important in a live show. But, please, try to make it live.

DISCUSSION LEADER JACKSON: Jim, you may offer a rebuttal if you wish.

MR. HARLAN: I might just point out the difficulty with making wildlife shows live. We would have had a little trouble bringing 500,000 blue geese into our 36-foot studio; yet, we did put those 500,000 blue geese on the film screen for one of our shows. I think wildlife film must be used if we are to use the props that we have effectively. Thank you.

WHAT'S WRONG WITH STATE CONSERVATION MAGAZINES?

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INTRODUCTION

Somewhere near a million individuals regularly receive the 40 conservation magazines printed in the United States and Canada. Family members and friends who read the publications without paying or requesting to be added to the list swell the total readership per issue to LIFE-like proportions. But in spite of this large audience, and in spite of the fact that many an information and education division pours the lion's share of its budget and brains into its magazine, few if any states are satisfied with the results of their efforts in the publishing field. If they were, you would be spared these annual confessions of inadequacy.

There have been several excellent quantitative analyses of conservation magazines (Gordon, Mellquist and Gwinn, 1954; Kilgore, 1954; Mahaffey, 1951; Thompson, 1954), covering such details as types of articles and illustrations, circulation figures, costs, printing methods and readability.

This paper is a qualitative evaluation of the publications, based on conversations with editors and information-education division heads; on study of the analyses mentioned above; and on three years of casual reading of the publications preceding the intensive scrutiny of the past few months.

The discussion is divided into four sections. Each represents an obstacle to achieving the publications' purposes. Each appears so frequently as to merit separate attention by administrators of conservation departments, information-education division heads and magazine staffs.

The comments that follow are, of course, a reflection of my own training and experience—college journalism training, two years as an editor on a grubbily commercial trade magazine, and three and one-half years as editor of *Colorado Conservation*.

PERVERSION OF PURPOSE

Even a casual glance at a pile of state conservation publications reveals examples of a rather sickening perversion of the noble purpose that the publications claim for themselves.

As a starting point, we may loosely define this purpose as follows:

to provide to the public accurate and readable information through which they can evaluate resource management policies.

If that is an acceptable definition of purpose, then we may well ask what role personal glorification of governors, commissioners, department executives and the magazine staff plays in achieving it? The kind of article that trumpets the accomplishments of Our Leader as the savior of wildlife does nothing but arouse the suspicions of the reader as to the integrity of the magazine and the entire department, as it should. And in any case, the resource and its management are more important and longer-lived than the current crop of individuals working in the field, even if the latter are protected by Civil Service.

Other magazines seem to be designed to represent the agency itself as infallible, accomplishing miracles in spite of the ignorance of the general public. The idea probably comes from the current code of the huckster world that quality of product means less than persuasive advertising. That is a lot of nonsense; we are better off to make certain that our programs are indeed correct, and acknowledge our errors gracefully before they are crammed down our throats without lubrication. And if the general public is indeed ignorant, we might well stop to reflect that we are getting paid to educate them.

Still other publications seem to feel that the magazine's purpose is to instruct its readship in the fine points of game or fish hoggery. Possibly they have tired of trying unsuccessfully to interest the public in biology, and go to the opposite extreme in the hope that readers will feel kindly toward the department for helping fill their creels and game bags. At any rate, overemphasis on techniques of wildlife slaughter is as effective in destroying educational value as a full page picture of the director kissing a baby angler. Good writing and design, plus a judicious sprinkling of how-to articles, is a better approach to reader interest than abject surrender.

Some strong words are also in order on another obvious shortcoming in this general category—the studied avoidance by many magazines of anything controversial. Public lands and water development are two issues that dwarf some of the technical problems we write about frequently, yet not more than a handful of publications have dared tackle them. This does not mean that a partisan view must be taken. It does mean that discussion of even the most bitterly disputed issues can be printed without reprisal if space is given to both sides.

Piously embracing the general goals of conservation does not satisfy the obligation to inform on all vital issues, any more than opposing communism is any substitute for proposing ways to strengthen democracy.

In summary, it may be that the obvious distortion of the basic informative purpose is forced on the editor by higher authority. But most of it, we suspect, is voluntary—prompted by a desire to please rather than a clear resolve to serve.

HELTER-SKELTER DISTRIBUTION

Practically every inquiry into the function of the state conservation publication has concluded that "we don't reach enough people." That it is true, and realization of shortcomings is the first step to solution. But of far greater significance is the realization, now becoming widespread, that more careful attention to a selected audience might be the most effective use of the magazine.

There are really two alternative ways of reaching people. The first is to distribute the publication free to anyone who asks for it. There are several faults with this system of getting readers. In the first place, most states who try to publish a free quality magazine—with detailed, well-illustrated articles and attractive covers—find that the free circulation must be limited for financial reasons, and thousands of prospective subscribers are kept on a waiting list. Waiting, of any kind, is not conducive to warm feelings on the part of he who waits.

In the second place, a great deal of free distribution is likely to be wasted. There is room for plenty of latitude on this point, but it would seem to be reasonable that many who clip the coupon and send it in do so out of idle curiosity, or to get news of a specific season, or simply to get something for nothing. When one of these free riders receives the magazine, he just may be inclined to treat it as a purple cross hospitalization ad or some such thing. Even paid circulation magazines dare not assume that every subscriber is an ardent reader, and the assumption that wide distribution of a free magazine means wide readership is doubly dangerous.

People are inclined to accept your evaluation of yourself, and your product. If you don't charge for your magazine, they are likely to assume that you don't consider it worth a dollar or so. And your prestige suffers accordingly.

The basic shortcoming of the free, just-send-a-postcard type of promotion, however, is the effect that it is likely to have on the quality of the publication. Magazines cost money, and common sense tells us that a bigger, better magazine can be produced if more money is available to spend on a smaller, selected press run.

The free publication might tend to become a sort of glorified newsletter of a few pages, containing mostly news of seasons, bag limits, etc. That is a legitimate purpose, but one that could be met with a simple newsletter. And straight news simply does not give the reader enough background to intelligently evaluate and support resource management policies. Add this to the psychological unsoundness of any kind of largesse, and you come up with a pretty fair argument against scattering your product willy-nilly around the state.

The second—and to me the most desirable alternative—is to publish a magazine designed to compete in quality with commercial publications, direct circulation efforts at leaders in all economic, social and political groups who influence the success of sound resource management in your state, and charge them for it.

Obviously, the starting point for promotion should be the most directly interested parties—members and officers of organized sportsmen's clubs and landowner groups (Farmers' Union, Farm Bureau, the Grange, Stockmen's associations, etc.). Next in importance might be quasi-official organizations such as the members of Chambers of Commerce, followed by all the service clubs (Lions, Rotary, etc.) and professional groups (doctors, lawyers and the like.) Some of this missionary work for the magazine can be conducted by personal contact, through the invitations received by the department to address their meetings, but this should be supplemented by direct mail selling to members. Naturally, you'll need a mailing list of the membership; and to get it, you'll have to do a selling job to the organization secretary. You might even have to pay for it, but generally mailing lists are cheap. In some cases it will be refused, and personal contact work will just have to be redoubled.

Of course, you can use the magazine itself for further promotion, as well as every other means at your command. There are many influential and interested parties who cannot be contacted through organizations. But the higher percentage of community leaders you can attract, the better your chance of making support for sound resource management a reality.

It should be emphasized that a paid subscription magazine is going to compete directly with other publications for the circulation dollar. And any publication, for that matter, is going to compete with television, the outdoors, yelling children and all the other distractions of civilization. If you expect to make any impression, you will have to offer the potential subscriber a magazine of professional appearance, professionally written, and vigorously promoted.

UNTRAINED STAFFS

It may well be that the most widespread obstacle to promoting public understanding and support of sound resource use policies through magazines is found in the magazine offices themselves.

Before tackling the delicate subject of what a magazine editor should be, one point should be emphasized. It is this: given a certain publication budget, there is a tremendous gap between the best and worst magazines that can be produced for the amount invested. The value per dollar is dependent on just one item—the staff. A truly professional appearance, excellence in layout and readable copy, costs no more than the most amateurish kind of job. The difference may not leap out at the untrained eye, but it is there—and the readers, without naming it, will unerringly sense it.

In the higher realms of the publishing world, this basic fact of professional standards is recognized. In the conservation field, conservation administrators insist on certain training and education qualifications for technical jobs in biological fields, but the general attitude toward publishing seems to be than anyone who can write an intelligible technical report can write and edit a magazine. And that just isn't so.

There are some understandable reasons for this confusion over what to look for when selecting an editor. The basic dilemma is the contradiction between two demands made upon the staff members. Game and fish management has become complex and technical. The editor, ideally, should be equipped to understand and explain a great variety of scientific material. But on the other hand, the representative reader is not interested in research and management as such, but rather in the results; and only then if they are made interesting and meaningful.

The first requirement calls for a technically trained man, and the second for expertness in the fields of communication; and in my limited experience, those two qualifications are rarely found in combination. The type of man who seeks scientific training, and gets it, acquires along the way a set of values and indeed a habit of living that is contrary to the essentially gregarious nature of a public relations job. And yet executives of conservation agencies shrink, instinctively, from entrusting their public relations program to a man they feel is not one of "us"—not one of the dedicated conservation group.

The result is evident as you scan the publications produced by these two types.

Some magazines are edited by technicians with "a flair for writing" and with no understanding of communications as a field requiring aptitude and training of the highest order. These tend to concentrate myopically on techniques of research and management without telling the reader what they mean to him and to the state.

Others are edited by professional journalists with little understanding of, or feeling for, wildlife management. These tend to slight biology and management in favor of subjects easier to glamorize—record heads of governors and commissioners, for instance. And if I may add a personal complaint, newspapering—especially on a metropolitan daily—offers little or no opportunity to learn the nuances of magazine design, color use and other printing production details, that can make or break a magazine. There is also a profound difference in editorial approach between a publication that comes out every month and one that comes out every day. Anyone who's tried to make the jump from a daily to a magazine will tell you so; and judging from the few magazines that consist almost entirely of stale re-hashed news, there are some who just couldn't jump far enough.

The solution to the very plain fact that many of our magazines are nowhere near so attractive or so readable or so authoritative as they could be without putting another penny into them is largely up to the editors.

If their backgrounds have not prepared them to produce a professional magazine, courses are available from practically every state university and many private ones that will correct the shortcoming; provided, of course, that the raw materials are present.

If the magazine is pretty, but not soundly based on legitimate conservation objectives, then it would seem wise to make it a point to learn the fundamentals of conservation, and temper the desire for reader interest with the realization that that interest must be directed toward the goal of wise resource use.

As vacancies occur, the ideal applicant would be a combination of Henry Luce and the finest sort of academic training in biology. Lacking such an unlikely combination of genes, the solution should be to hire a journalist with a conscience and with experience in editing and in printing production.

He can get all the scientific help he needs; and if a technician is hired for the job, that leaves the department without a single expert in the publications field—and it is, I repeat, a field requiring aptitude and training of the highest order.

MISGUIDED ADMINISTRATORS

This first criticism of some of the finest men in the conservation field is based on little more than a hunch, but it is a pretty strong hunch. Most any administrator will go without nourishment or rest fretting over a big game problem. But I would guess there are mighty few who give more than passing thought to their magazine once an editor is selected and the magazine comes out with reassuring regularity.

This apparent attitude may stem from the technical man's natural inclination away from the whole public relations field, and in many cases it may reflect the old but lingering attitude that the whole business is a lot of frippery—that telling the public about your resources and so on is handing out a little dangerous knowledge. Whatever the motive, the boss is missing a bet. He should keep his editor excellently informed, and he should demand the same high level of performance from him that he does from his technical staff. If he doesn't know enough about communications to evaluate his publication, he should make it his business to learn. The time is long past when the director can safely turn his back to the confusing and distasteful business of informing the public.

A few administrators do indeed use their magazines as organs of personal publicity, although some of this may well be voluntary toadying on the part of the editor. In any case, such obvious tactics will wreck the magazine, and it may well wreck the administrator. Publishing his name and likeness at every opportunity may bring him credit of a sort, but if he makes himself the personification of game and fish in his state, he will also have to bear the blame for every failure of every one of his employees.

No doubt some editors are forced to sanctify in print government officials other than conservation department executives—commissioners, legislators and governors, etc. There is some pressure for this kind of thing in every state. Except in those few states where the conservation department is completely at the mercy of political influence, the editor ought to be able to dodge the issue through a combination of tact and fortitude.

There is still another danger to the effective conservation magazine from above. This may be simply described as meddling, wherein the administrator attempts to edit as well as administrate, deciding what articles will go in what issue, what subjects are taboo, etc.

Being blessed with a tolerant director myself, I would like to see every editor restricted only to the broad framework of department policy. Within that sphere, he should be allowed to choose how to approach the problems of conservation, and up to the point of political reprisal, he should be allowed to inform his readers honestly on the most controversial issues of the day.

CONCLUSION

Conservation magazines are published by all but a handful of states as a major part of the struggle to acquaint the American public with the need for sound resource management.

The goal is an important one, and if the magazines are to measure up to the burden entrusted to them, four common causes of failure must be carefully avoided.

The first of these is confusion of purpose on the part of those responsible for preparing the magazine. The magazine's prime function is to inform and, whenever this aim is subordinated to publicity for executives or the department itself, the cause of conservation is made to suffer. The magazine should be focused on the resource and its management, not the harvest of the resource or the individuals who do the managing.

The second common shortcoming is helter-skelter distribution—an uncoordinated attempt to reach as many people as possible without concentrating, initially at least, on reaching those people who are in a position to help or hinder conservation. Considering the very real financial fact that free distribution of a comprehensive and attractive magazine to anyone requesting it is extremely expensive and inevitably wasteful, the solution would seem to be to settle for fewer readers, carefully selected, and give them a quality publication at a price that will defray all or part of the costs. Widespread promotion of the magazine can still be encouraged, and the editor may be reasonably sure that his readers—who are willing to pay—are the sort who will absorb and spread the conservation gospel.

The third shortcoming is found in the fact that many magazines are edited either by technicians who know little of magazine journalism, or by journalists who know little about conservation. This is not to suggest that conservation publications are all badly done. On the contrary, an outstanding few will stand comparison with any of the country's periodicals, in appearance and accurate, readable writing. But it is fair to state that a special effort to learn more about editing and layout, or about conservation fundamentals, will result in far superior products at positively no additional cost, save sweat.

The final shortcoming is becoming less widespread each year—the misguided administrator. There is a cause for rejoicing in the fact that the untrained political appointee who sees the magazine as a means of defending his position against the next change of administration is being replaced by the career conservationist. And the latter, in spite of the scientist's misgivings, is coming to realize that jealously guarded knowledge never aroused public support for wise resource management.

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DISCUSSION

MR. JOHN BLANCHARD [Louisiana]: I would like to be the first to receive the purple heart. What is the objection, if any, to a give-away magazine provided you have funds?

MR. GREGG: I think you've answered your own question, John. If you've got enough money to publish a magazine, you can go ahead and blanket your state with a quality publication and probably accomplish a good end—and Louisiana does a good job. Of course, I'm the Scotch type, and I hate to see anything that good given away. I would wager, if you would charge for the magazine, you would probably get two-thirds as many subscribers as you have now. They would be the people you need. You would probably get more respect from them, from the press, and very likely from your own department.

CONSERVATION EDUCATION IN THE NORTHWEST

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INTRODUCTION

It is the purpose of this paper to outline some of the conservation education programs that have taken place in the Northwest in recent years, particularly in the States of Oregon and Washington. It would be impossible to list all the programs in the time allotted, but it should be possible to stress the trend and give the highlights of a few of these projects. It may also be desirable at this point to indicate my concept of conservation education. I believe that anything man learns from man is part of his education. If he learns anything from his fellow man in the wise use of the natural resources, then this is conservation education. This concept permits me to wade into conservation projects more or less uninhibited.

HISTORY OF THE NATURAL RESOURCES

The history of our natural resources in the Northwest is different from that of some other regions only in the fact that resource depletion started at a later date and therefore the natural resources have held out later in the development of the continent. Yet with full knowledge of what has happened to the natural resources in many other parts of the United States and other countries, we have profited very little. The commercial fishing industry believed that there were endless runs of salmon and permitted nets and fish wheels to take unrestricted numbers of fish. The now valuable sturgeon was considered a pest by early commercial fishermen and, when found in the salmon nets, was killed and dumped overboard. Big game had almost disappeared by the end of the nineteenth century. Only scattered herds of Olympic elk and black-tailed deer roamed the inaccessible rain-soaked forests of the coastal range. Mule deer, white-tailed deer, antelope, bighorn sheep, and Rocky Mountain elk were few in numbers, victims of the hide hunters and hungry pioneers; and buffalo bones marked the passage of this great American animal. The destruction of the wildlife resource is only a sample of the unwise use of the natural resources of the rich Northwest. The ravages of forest fires were brought to the attention of the public by the Tillamook Burns in northwestern Oregon. In the years 1933, 1939, and 1945, 355,000 acres of trees were destroyed by fires which killed over 13 billion board feet of timber (U. S. Forest Service, 1944). Most of the area was burned over by all three fires.

In the early '30's there were a few efforts along conservation lines, but in general they were misunderstood or misguided. There weren't any organized cooperative efforts to promote the wise use of the natural resources of the Northwest; so the unorganized efforts miscarried. An example of this was the effort by representatives of public land-management agencies to reduce overpopulations of big game. Due to the protection provided by the buck law, deer and elk were exceeding the carrying capacity of some big-game winter ranges. The public land managers asked for doe deer seasons and cow elk seasons. The Game Commissions asked for factual data to support the claims of overuse instead of ocular estimates. The public agitated against either-sex hunts and demanded that supplemental winter feedings be made instead of harvesting the surplus animals. Many deer and elk starved to death even though they were fed hay. The overused browse on some of these winter ranges is still producing far less than its potential yield.

A definite lack of cooperation existed in the conservation field and few attempts were made to educate the public in the wise use of the natural resources. This lack of conservation education and cooperative endeavor in the past makes the present picture look good and the future picture look even brighter, for by contrast conservationists are getting things done in the Northwest. They are working together, pooling their assets, and forgetting the lack of cooperation that once existed.

THE "KEEP GREEN" MOVEMENT

Following the bad forest fire year of 1939, conservationists in the Northwest realized that an action program was essential if the people were to understand the need for wise use of the natural resources of soil, water, timber, wildlife, and forage, all vital to the progressive development of the area. Almost simultaneously, industry and government began to stress conservation education. The first progressive step was the formation of the "Keep Washington Green" movement in 1940 by a group of private individuals, agencies, and corporations. This publicity campaign, with Stewart Holbrook as director, was extremely effective in arousing fire consciousness among the citizens of Washington and a large number of tourists (Goodyear, 1940). It is difficult to say how many forest fires this program has prevented. The campaign spread rapidly and now plays an important part in the fire-prevention work of the forest fire protection organizations. It also is playing a big part in getting conservation education material into the classroom. (Brier, 1949).

THE TREE FARM MOVEMENT

At about the same time the Keep Green program was getting started, another worthy conservation program was initiated. Weyerhaeuser Timber Corporation planned to set aside a large area of cut-over land for studies of tree growth and reproduction. The idea at first was to get public cooperation for more adequate forest fire protection of this area. The editor of the Montesano, Washington newspaper, The Montesano *Vidette,* had a theory that to call the area a tree farm would make the public respect it more, and that the public understood the term "tree farm" because it implied growing timber as a crop. The Clemons Tree Farm was established in Grays Harbor County on the Olympic Peninsula in Washington in 1941 with 120,000 acres. It now consists of 154,860 acres owned by Weyerhaeuser Timber Company and 327,000 acres over-all under the general management plan. This movement spread, and now there are 256 tree farms covering 4.4 million acres of forest land in the Douglas-fir region of the Northwest. Throughout the United States there are now over 4,500 tree farms covering 29 million acres, and the movement is still growing. It has definitely been a conservation education tool and has made many private landowners aware of the need for better fire protection and more applied forest management (Montesano *Vidette,* 1951).

CONSERVATION WORKSHOPS

In 1945 the Central Washington College of Education organized the Conservation and Outdoor Education Workshop. The Northwest, although late in getting started in the conservation workshop field, profited a lot from the work of other areas and came up with a very fine workshop. In 1952 this workshop was cosponsored by the Washington State Department of Public Instruction. Consultants from federal and state agencies, the State Grange, garden clubs, private industries, the Washington State Sportsmen's Council, and other organizations take active part in the workshop. Last year (1954) 66 teachers and resource people attended the training session from August 15 to August 21 at Rustic Inn on the Wenatchee National Forest in central Washington. Because greater facilities were not available, 20 potential trainees were turned away. Representatives of 4-H, Boy Scouts, Girl Scouts, women's organizations, and sportsmen's groups attended the workshop course, although most of the trainees were school teachers.

The trainees were taken out to timber sales and shown how to mark and scale timber. They examined bug-killed trees; they planted fish; and they visited beaver dams, irrigation canals, strip mines, and placer mines. They observed a sawmill in action and learned the relationship of the resources to the schools, churches, and homes of the community. They also observed the important relationship between plants and animals and their environment. They learned that a forest is made of living things competing for soil, water, and sunlight. The emphasis is placed on doing the work in the outdoors.

The cooperation in these workshops between industry, state and federal agencies, and private organizations such as the State Grange and the Washington State Sportsmen's Council is excellent. The state and federal agencies sent trained men who explained their work and demonstrated its application. Forestry industries made active logging areas available, and the teachers used the power saws to fell in a few minutes giant Douglas-fir trees that took 400 years to grow to maturity. The Washington State Sportsmen's Council sent champion fly casters and riflemen who demonstrated their skill and taught the trainees the use of a fly rod and the safe handling of a gun.

There are now six conservation education workshops in the Northwest, but it is generally felt that the one at Rustic Inn sets the pattern for the others.

THE TILLAMOOK BURN TREE PLANTING PROJECT

Under the direction of the Portland Public Schools and in cooperation with the State Forestry Department and conservation groups, trees are being planted on the Tillamook Burn by school students. The conservation organizations furnish money for buses and some members of the organizations for supervision. Bus loads of students plant trees furnished by the Oregon Forestry department on depleted areas of the Tillamook Burn. The students enjoy this work and later watch the plantations as they develop. Other communities throughout the Northwest have community forests planted by their school children, but the planting job on the Tillamook Burn is so large, 250,000 acres, and as the leadership and organization are furnished by the students themselves, it deserves special mention.

OREGON WATER RESOURCE COMMITTEE

In Oregon the State Fish and Game Commissions became aware that the basic resources of soil and water were essential to good wildlife management and needed protection from erosion, siltation, and pollution. In checking some of the rivers it was found that if all the water rights were used as allocated there wouldn't be enough water left in the river beds to support fish life. Through the efforts of the Oregon conservation groups, the 1953 Oregon Assembly passed a bill setting up a water resource committee with an allotment of $50,000 to study the water resources of Oregon. This committee's report has been submitted to the 1955 legislative body with recommendations for two bills. One provides for a state water resource board that would resolve all conflicts concerning the use of water. The second bill sets up a ground-water code that would control the future use of ground water.

This water resource committee is important because it was sponsored by conservation groups and some 1,500 persons attended the 15 public hearings held throughout the state. In all, some 250 people appeared before the committee to present briefs concerning the use of water. (Lane, 1955). The people of the Northwest are keenly aware of the importance of the resources, and recent political battles in this region have centered on conservation issues.

A CONSERVATION EDUCATION COMMITTEE

One of the most unique committees in the Northwest is the one working on conservation education for the Oregon Division of the Izaak Walton League. This committee is composed of representatives of industry and government. It includes members from the power interests, timber interests, and businessmen of the community. Government agencies include the schools, forestry departments, and other resource agencies. This committee, working closely together, came up with a publication, "Inexpensive Written Materials on Conservation for Oregon Schools," (Conservation Education Committee, IWLA, 1953). This publication received an award of merit in 1954 from the National Association of Conservation Education and Publicity. The best part of this endeavor is that it has paved the way for better relationships between industry, public agencies, and conservation groups. This may mean a lot in the future development of the Northwest.

The latest publication of this committee is hot off the press. It is titled "Conservation Programs and Projects for Classrooms and

Clubs." (Conservation Education Committee, IWLA, 1955). This includes ideas for conservation programs as well as a list of possible conservation projects. We believe it will be of interest to all teachers and sportsmen and may have uses by other groups.

JUNIOR CONSERVATION CAMPS

Camps for teenagers are filling an important conservation education need. There are many of these in the Northwest. The Washington State Sportsmen's Council sponsors a camp for junior sportsmen at the Moran State Park on Orcas Island. Approximately 100 high school boys, aged 14 to 17, attend this camp each year. These boys learn the wise use of our natural resources and are trained in leadership responsibilities to their home communities. Through this leadership, some 40 junior sportsmen's clubs have been formed and there are estimated to be 10,000 organized junior sportsmen in the State of Washington.

Other junior camps are sponsored by the American Society of Range Management and the Oregon Museum of Science and Industry. There are, of course, other summer camps for young people: one at Camp Silverton on the Mt. Baker National Forest in Washington trains grade school children in conservation. A special session is devoted to handicapped children. There are also 4-H camps, girl and boy scout camps, and many others all important in conservation education.

COOPERATIVE CONSERVATION PROJECTS

Community projects to develop some phase of conservation are not new, and no doubt stem from the pioneer days when a barn-raising was an occasion for a celebration. Community conservation projects are maintaining this tradition in the Northwest. In recent years three lakes have been built by community endeavor. The biggest, Delintment Lake, is 52½ acres in size and holds 450 acre-feet of water. There are 2,000 yards of material in the dam. The lake is a fisherman's paradise and trout grow from 9 inches to 16 inches during a summer. On July 4, 1953 practically the entire town of Burns, Oregon turned out to build the dam that holds this lake. Hines Lumber Company furnished the equipment; merchants furnished food and supplies, and presto! the job was done. A neighboring town, Prineville, decided it should have a lake, too, and pitched in and built the 25-acre Walton Lake named after Izaak Walton.

Outside of fish stocking by the Oregon Game Commission, no government funds were used in these projects.

BRING BACK BITTERBRUSH

The bitter brush shrub (*Purshia tridentata*) is an important browse plant for deer in the eastern sections of Oregon and Washington. Due to overuse by both big game and livestock, it is fast disappearing from big-game ranges. In order to bring back this shrub, the Multnomah Anglers and Hunters Club of Portland, Oregon collects and packages bitterbrush seed and distributes this seed each year for planting by hunters going to areas where bitterbrush normally grows. This project, originated in 1951, has spread throughout the west. Washington has a similar project and so does California. It is not believed that planting these bitterbrush seeds will ever rehabilitate the stand of the shrub so that it will substantially aid as big-game food. However, as a conservation education tool it is priceless and has done a lot to interest sportsmen in big-game management.

ANTI-LITTERBUG CAMPAIGN

The anti-litterbug campaign that brought about Keep America Beautiful, Inc., a national organization that is doing something about litter, started with the Portland, Oregon Chapter of the Izaak Walton League. Credit for this campaign cannot be claimed by any one group, for garden clubs, women's clubs, and the Sierra Club have been working to do something about litter for a long time. Yet it took a group of conservationists with the drive to push the project to get it national recognition. Stickers, posters, and litterbags were used to interest people to dispose of their litter, not broadcast it. Now that the project is in the hands of the capable Keep America Beautiful organization, that mountain of litter should soon become a molehill.

CONCLUSION

The excellent cooperation that now exists in the Northwest between representatives of government agencies, private industry, and sportsmen's associations can set the pattern for other areas. There is a keen public appreciation of the economic importance of our natural resources. For example, excellent progress has been made in the field of wildlife management. Beaver, big game, and the fishery resources are making remarkable recoveries in recent years. Many cooperative projects to preserve or restore these resources are now in progress. Last September the Oregon Game Commission solicited the aid of sportsmen and their motor boats to poison trash fish in the 3,000-acre Diamond Lake. The response was splendid, and this large project was carried out very efficiently. Other such projects receive assistance from sportsmen and conservation groups. Community action has sponsored numerous conservation projects. There are many resource management jobs still undone, but action speaks louder than words, and cooperative endeavors in the Northwest have just begun.

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DISCUSSION

DISCUSSION LEADER JACKSON: Thank you, Bill.

We are so often accused of talking to ourselves in this business that the question has become trite. Yet, on the other hand, it seldom is applied without justification. I wonder just how effective these campaigns on litterbug control and bitterbrush reseeding, and all that, really are. Do you have any opinions on it, Bill? Are we kidding ourselves with it, or are we actually getting through to the public?

MR. HUBER: I think, Bud, we are getting information across to the public. These projects have taken hold very well. The bitterbrush, as I said, does not mean too much in itself, but it has got our sportsmen interested in what deer eat. Now I think they can tell the difference between sagebrush and bitterbrush which I am sure a lot of them could not do before.

As long as I have a little time, I would like to tell you about another project. We have another project coming up which is not in the paper. It is known as the ''Red Hat Day.'' We launched this program before I left, and I think it is going to be one of the biggest public relations jobs that we put out. The idea is to get greater public appreciation for the natural resources of the state and induce recreation users to do five things: Practice better sportsmanship; display more respect for property; hunt safely; obey the game laws; and prevent fires.

We have 250,000 hunters in the State of Oregon, and I believe we had 37 accidents last year with 13 fatalities That is too much. We offer this Red Hat Day program to get people so interested in safety that they won't go out and shoot each other. The idea of the program is to have the Governor set up a day which he will call Red Hat Day. We in Oregon usually open our deer season around the 1st of October. We will probably ask for the fourth Friday in September as Red Hat Day. Then we will put on campaigns, programs and have all the civic organizations and interested people take part in Red Hat program.

We hope to get some little lapel buttons, or something like that, maybe a pin, of a red hat that we can give to ladies and people who would not like to wear Red Hats. But the rest of us will put our Red Hats on and take over to try to carry on a program of these five things I have pointed out. I believe this will be a very worth-while project, and I hope it spreads nationally.

THE SOUTH IS WORKING ON ITS PROBLEMS

ROSE E. FLEMING
Mississippi Game and Fish Commission, Jackson

I represent a section whose wealth and poverty—beauty and ugliness—have woven a tangled net that has bound her people for a hundred years. We have many problems, but we have recognized that they are mutual ones which we can solve by mutual planning.

We have many voices in our land today. We have many prophets—calling us back to the grass roots—to the soil, to the forests, to the water, and to the wild creatures. That was our beginning. And that is our ultimate destiny.

Perhaps no other section in the United States is more dependent upon her renewable natural resources that we of the South are. Much of our expansion in the past two decades has been built upon their greater use.

It is fine that we know it—that more and more of our people are determined to build a richer and more productive South out of the substances God gave us—the forests, and the oil, the black loam and the fast-growing fish.

In this Renaissance we in conservation-education have a vast responsibility. We must challenge the apathetic—strike a spark against the flinty pessimist—reduce the optimist to practicality. And yes, in some cases, but with diminishing frequency, we must woo the technician into seeing that *conservation* is not a secret rite for only the high priests to conjure with, but a living and a vital philosophy stirring our humblest citizen to a new and a better way of life. . . .

We conservation-educationists are by and large emotional people—by training and by temperament. We have been taught—and we know by trial and error—that our material isn't effective unless it "comes alive." So we keep digging behind the techniques and statistics for the blood and the bone—for the pulse that makes it throb.

We know the average man doesn't become inflamed about erosion problems in Tibet, but the poverty of a family he knows on some desolate strip of land means something to him.

This is a point we information and education people keep hammering on. And the South, generally speaking, has done a good job of *personalizing* its publicity for reader interest that captivates while it instructs.

Joe Shomon's magazine, *Virginia Wildlife,* which was rated tops in the national field last year, is one good example of *creative* education

—always personal, never dull—yet seldom lightweight. There are important concepts skillfully woven into most of Shomon's material—yet it has that *emotional* overtone which we can't afford to underrate.

But *we can go overboard on emotion.* The work of some pseudo-conservationists is still almost entirely emotional, beginning and ending with the hackneyed tale of woe—the defunct passenger pigeon, the vanishing quail. Here, my friends, is an ideal place for that long—and minor—chord. Here is the place for that vision of a white-columned mansion, that vision of virgin pine—only, however, for those who can afford the luxury of regret—and who of us can?

Here instead, let us emphasize, is actually the place for *construction*—for laying the bricks side by side till we've got a foundation too strong for wind and water to crumble.

And here is where the South, slow-starting though she may have been, has a strong edge.

She has the benefit of the experiences of other sections.

She can analyze the fine educational program of a state like California, and borrow her ideas, and avoid the pitfalls California has survived.

She can learn—not by lonely trial and error—but by observation, by conference, and by neighborly advice—that the nation as a whole, collectively and individually, has drawn some important conclusions.

1. That education is a vital tool in the protection and development of our natural resources.
2. That a conservation program must have unity.
3. That a conservation education program is worth little unless it is based on a broad generous over-all concept.

There's no room in this philosophy for interdepartmental jealousy. There's no room for technicians engrossed in one little orbit and one alone. There *is* room for men of vision, who always consider their specialized field in relation to others and to the ultimate goal.

Thank God, we have many men like these. And the legions are growing.

Their voices are heard like this:

In the Southern Public Administration Council—composed of representatives of the TVA and eight Southern universities—who after ten years of research, in the fall of 1954 issued "Natural Resources and an Informed Public"—crying for a state resource policy that would integrate resource programs within the states and within the nation—crying for *information* upon which the people could decide public issues of vital concern—so "especially significant in the field of natural resources."

And these voices drift down from Michigan where in January of this year Resources for the Future financed a conference to discuss how to free agencies for the most effective administration of resources —how to stir private landowners and the general public to constructive action.

This conference was sponsored by the College of Mining and Technology, Michigan State College and the University of Michigan; and the Michigan Division of Conservation recognized these as being some of the problems which cooperative planning hopes to overcome:

"(1) Public resource managers may not have the funds or legal authority to really practice conservation with the resources under their custody.

"(2) Private owners may not yet be educated either to the need of applying conservation to the resources in their custody or educated to the point where they can apply the necessary conservation practices.

"(3) Finally, the public has not accepted its share of the responsibility of practicing conservation on private land. Private land and water owners cannot be expected to invest time and money in conservation practices from which the public and the distant future will profit. . . ."

And among our own number, their voices were heard last May in California when the members of the American Association of Conservation Information drew this conclusion:

"Educators prefer to deal with all phases of conservation—soil, water, forest, fish, and wildlife—as related subjects. They are unwilling to absorb any one element without the other. This makes it very difficult for any individual conservation agency to sell its program to the schools." . . . .

These are but three examples of our recognition that our problems are mutual ones—which must be solved by mutual planning.

These are but three examples of the growing trend toward integration, toward education, and toward *cooperative planning.*

I certainly do not mean to exclude the general public in this discussion of "education" nor to imply that the varied media of press, radio, and exhibit do not educate. The many movements now underway which point toward integration are in many areas; however, our progress in this direction in the South may perhaps best be illustrated by our work with teachers and in the public schools.

We have put this idea of cooperative planning in practice in Tennessee where the program of the State Department of Education and the Department of Conservation has won national recognition, bringing Conservationist James L. Bailey one of the American Association of Conservation Information national awards in 1953-54.

We have put it in practice in Alabama where the State Board of Education in 1950 published the outstanding "Using Resources of the Community to Build a School Program"—a bulletin which might profitably be studied by those of us interested in promoting school programs and furnishing assistance to teachers and schools.

We have put it in practice in North Carolina where conservation specialists have participated in a number of highly successful Resource-Use Education Conferences—and where specifically the work of Lunette Barber of the Conservation Commission has been particularly effective, because, with her background of teaching experience, she has worked out practical units and projects which teachers use.

We have put it in practice in Kentucky where the recent experiment at Murray College is worth studying for its down-to-earth practicality.

Here in September, 1952, the entire staff of Murray Training School attended a camp at Kentucky Wildlife Refuge near Cadiz, where specialists in natural-resource management assisted in the study of wildlife, soil, geology, forestry, etc. During the school year of 1952-53, these supervising teachers and their trainees made an effort to work this material into their regular teaching.

During the summer of 1953, a 17-day workshop in the "Techniques of Teaching Conservation" was held at Murray with the entire staff of the Training School as faculty. Representatives of federal, state, and county agencies have cooperated with the whole program, and it is being studied by educators throughout Kentucky.

It is one of the few examples I know about of consistent "follow-up." A full report of the program might help us to answer that question so often asked, "How do you *teach conservation?*"

In the Southeastern states, in general, there is a growing trend toward Conservation Education Committees—generally composed of representatives from the State Conservation Agencies, the Department of Education, and the Institutions of Higher Learning.

In many Southeastern states we are tackling this job of teacher training by encouraging college-conducted summer workshops, and members of our organization frequently serve as consultants.

Cooperatively, with limited funds, we are activating programs we could not hope to keep rolling alone.

And *cooperatively,* our programs are better—because they are unified. They are broad in concept. They present not one but many facets of the complex conservation picture. And, in most cases, we are making a vigorous effort to "make our program come alive" in local community problems which the student—the reader—the teacher—can interpret and understand.

We recently held a field trip for Mississippi teachers which we felt was effective, because the Soil Conservation Service, the U. S. Forest Service, the State Forestry Commission, and the Game and Fish Commission all participated.

The field trip allowed us to present local problems with which the teacher was concerned, and to present them with a broad inter-agency viewpoint.

As an example, we showed the hardwood girdling processes now going on all through the Southeastern states, sometimes at the expense of wildlife. We discussed the inter-agency conferences which are developing policies practicable to both timber and wildlife interests. We showed the mast traps employed in the field to collect data which will support our recommendations to the Forest Service—data on the size and age of trees which are the best mast producers.

Thinning processes, commonly employed in the forests of our Southeastern states, were observed and discussed in relation to forest management, wildlife, soil, and water.

Controlled burning interested the teachers immensely. And here we had a fine opportunity to discuss a problem with which all the agencies are concerned.

Our Southeastern states, in general, have a grim problem of forest fire, and our state forestry departments have concentrated much of their publicity on fire prevention. Actually they have done such a super selling job in my own state that many of the teachers were incredulous when the wildlife specialist pointed out that controlled burning is a tool of game management, wisely employed at certain seasons, to burn out undergrowth so new shoots can come up for wild turkeys and other game. The U. S. Forester defined, from his angle, additional situations in which controlled burning is effectively used—to destroy undesirable shrub oaks and underbrush—to prune, or to check diseases still alive in a carpet of pine needles.

This particular field trip may be cited as an example of cooperative planning in Mississippi. We offered it under the sponsorship of the National Association of Biology Teachers and in answer to a request their state group made last year.

You are probably familiar with the history of the Conservation project of the National Association of Biology Teachers, which came into being soon after the publication in 1951 of "Conservation in the American Schools," by the American Association of School Administrators.

Financed by a grant-in-aid from the American Nature Association, it concentrated first on the publication of a handbook, "How to Teach Conservation and Resource-Use in Science and Biology." The movement has been concerned with training both the student teacher and the teacher in service.

Many Southeastern states have cooperated in various fashions with this organization. There were a good many of our number present at their 1954 Boston National Conference which edited the handbook. The Soil Conservation Service has been quite active in the movement, has promoted university-conducted conservation workshops for teachers and has offered a number of scholarships on the state level.

In the Southeastern states these conservation workshops have a variety of sponsors.

The University of Tennessee started such a workshop in 1953, and the Tennessee Garden Clubs are providing scholarships for teachers who attend. The 1955 Tennessee Workshop is sponsored by the Department of Conservation and the University of Tennessee. Half the time this summer will be spent at Big Ridge State Park which is a wildlife refuge.

Dr. Malvina Trussel, who has directed outstanding Conservation Workshops at Florida State University, may be cited as one of the biology teachers who has done a great deal to advance the cause of conservation.

In Mississippi, working through the National Association of Biology Teachers, we have succeeded in getting a wildlife management course offered this spring for the first time at the University of Mississippi, And we have succeeded in promoting a conservation course at the Gulf Coast Laboratory, which is directed by the Seafood Commission and the Mississippi Academy of Science.

The major problem in our state seems to be our teacher attendance. This holds true for the teachers in training also. Mississippi Southern College, for instance, which offered a conservation course in answer to our request, had to discontinue it because of low enrollment.

This appears to be a national problem which was discussed at the Boston Conference of Biology Teachers. As a specific example, they cited Rhode Island where a well-planned Conservative Workshop, well-financed with adequate funds for scholarships from the Wildlife Leagues, Garden Clubs, and other organizations, *well-staffed,* and with credit offered by both the State Teachers College and the University, still failed to attract an adequate number of students.

We are certainly a long way from success in this matter of teacher training—and the road ahead will not be easy.

But consider what a responsibility the conservationist has in this field. We are advised by the U. S. Office of Education that less than 60 per cent of the nation's high school biology teachers have the equivalent of a college major in that field.

Think what specialized help can mean to teachers anxious to spot and help solve local resource problems. Think how challenged we are to present our material in forms which will inspire teachers to use it.

From my own experience of the past three years—which includes not only contacts with many Mississippi teachers, but participation in the 1954 Boston Conference and in the First Southeastern Regional Conference to Improve Biology Teaching, held in the fall of 1954 at the University of Florida, and financed by the National Science Foundation—I believe I can safely state that many teachers are unaware of—and certainly not taking full advantage of—the valuable help available through the various conservation agencies.

We conservationists have a "selling" job to do with many of our teachers—but we must take into consideration their overloaded schedules and wide range of teaching demands. We must work out ways in which teachers can attend field trips and workshops without hardship to the individuals or to the schools. Our willingness to present all phases of conservation — forest, soil, water, and wildlife — will prevent their labeling us "soap hucksters." We must give a broad overall conservation picture. Above all, we must *"make it come alive."*

All around us the voices are being heard—the voices of the men of vision crying for cooperative planning.

In November 1954 the Southeastern Game Commissioners meeting in New Orleans invited soil and forest representatives to participate in panel discussions. That was a significant innovation in this growing trend. *Integration* is the trend.

Until recently the Southeastern Game Commissioners Meeting has been confined to technical sessions only. But now they are including public relations panels. *Education* is the trend.

On a national level, the U. S. Fish and Wildlife Service says "improved public relations" ranks high on the list of pressing needs. Is this yet another indication that management recognizes the powerful force of a good information and education program?

Personally, I feel that the most encouraging sign of all is that so many groups in the nation—and particularly in my own Southeast—are at work on constructive *action plans* which cannot fail to bear fruit.

In the words of one of history's all-time conservation "greats," Aldo Leopold, "Fear never kindled a flame in the human heart."

We're not talking "scare-talk" now in the South. We're promoting conservation workshops. We're cooperating on educational bulletins. We're tackling problems of inter-agency relationships. We're meeting in groups to iron out our difficulties. We're through with those luxuries—regret and self-delusion. We are planning now for the richer world of tomorrow.

DISCUSSION

FRANK CARLSON [Connecticut]: Mrs. Fleming, particularly in the Northeast where there are highly populated urban areas, and the children do not have the opportunity to get out and see the various processes of nature, how, then, do you develop this concept of conservation?

MR. FLEMING: We have thought seriously about that. For instance, at the workshop—and our two-day field trip was a real workshop because everybody participated—there was no one there who did not speak his piece before it was over. One suggestion made by teachers was that we have additional field trips for children or have them sponsored by the various schools.

There is a very interesting point here. We recently conducted the Izaak Walton League contest for the outstanding young conservationist of the state, and the boy who won has been a town boy all his life. He is a very active Boy Scout worker. Of course, he lives in a small community. We don't have the metropolitan areas in Mississippi that you have. But he has been particularly effective in the public relations field. He sold the sportsmen's clubs on the idea, and organized a sportsmen's club, as a matter of fact, in his county. He has gotten the consent of private landowners and personally delivered and helped to set out 5,000 trees. So it is not a hopeless task at all to inspire the cities. I think they are frequently most anxious to get into the out-of-doors.

Where I live in Jackson, Miss., we have 140,000 population, and one of our school superintendents, who has been particularly interested in nature study, from the first grade through the sixth grade, has developed a nature trail on the school campus and adjoining territory. He told me with considerable pride the other day that while he did not have a select group of students, but just a broad American group there with high, low and medium IQ's, still his achievement tests were way above the norm. They were three to six months ahead in every subject area, English, geography and arithmetic. They were ahead of the district norm. The teacher was asked if it had any bearing on his science training of the students. He feels that nature study carried over into other subject areas so that students got more meaning from their reading, and I think that's an interesting point.

SELLING CONSERVATION BY CONFERENCE

D. B. TURNER
Director of Conservation, British Columbia Lands Service, Victoria

Professor Waddington, Professor of Animal Genetics at the University of Edinburgh, in a different context yet in plain parallel, explains how difficult can be the work of those in the field of conservation information and education.

Professor Waddington says in his book[1]: "It is probably stupid to expect any new way of life, whatever its advantages over the old, to be thankfully accepted even by the people to whom it will most obviously do good."

"Trying to cure the ills of society," he writes, "is rather like dosing a cow; you want one man to hold its mouth open while another blows the pill to the back of its throat down a cardboard tube; and it has the same danger, that the cow may blow first."

The way of the conservation educationist similarly is beset with danger. If he is to succeed in persuading society to adopt a better way of life, based on wise and prudent trusteeship of natural resources, he must be ready to "blow first" whenever a sound conservation plan is proposed or whenever a conservation emergency arises. At all times he must be sensitive to the situation in respect of administration, development, and use of the products of the environment. Let me give an example or two of what he must bear in mind.

Individuals and society may differ in their attitudes towards the use of resources. Thus their ideas on what is economic development may be quite at variance. Because the "what is good for me" philosophy, often characteristic of the individual, may be directly opposed to the "what is good for us" philosophy of society, conflict may arise from the clash of interests and, of course, such conflicts will be sharp or mild according to the degree of opposition in the two views.

When the situation just described exists, a host of factors may enter to complicate the circumstance. At one extreme are found the individual rights of life, liberty, and happiness, and at the other the responsibility of the nation to provide for posterity. At stake in the struggle are the health and wealth of our soil and water and other natural resources.

This plain view of the opposing forces in what has been described as "the perpetual warring within democracies" has been presented

[1]C. H. Waddington, *The Scientific Attitude*, Penguin Books, Ltd. 2nd rev. edition, 1948.

ably by an Australian agricultural economist, J. G. Crawford.[2] After examining the problem thoroughly, he concludes that there is common ground for individual and social action, and that in the matter of natural resources, the elements of agreement and partnership are research, education and conservation action.

The conservation educationist places his trust in these elements of research, education, and action. Further, he is convinced that the common ground for individual and social action can be described as an integrated policy of resource use. In other words he firmly believes that the present and the future are made secure by adhering to the principles of inter-dependence and integration of resources because on these principles the individual and society can meet and agree. It might be said parenthetically that the British Columbia Natural Resources Conference, to be discussed later as an example of success in conservation through conference, was founded upon this sound philosophy.

The point has been made that the interests of the individual and those of society can be reconciled on the question of resource development and use. It follows, I believe, that it is the job of the man in conservation education to incorporate this fact into the thinking of the general public. It is his unending task, therefore, to reach the many with the teachings of conservation and to persuade the citizens at large to adopt the conservation philosophy and outlook as a substantial, component part of their lives.

Now that this primary function of the conservation education officer has been defined, the problem arises, "*How* is the public as a whole to be reached?" This introduces my second example of the many facets of human behavior that the conservation educationist must always keep in mind. The example is one of educational methodology and relates to the aim to be taken by the educationist before firing the bullets or bulletins of information.

For our purposes today, the scope of educational methodology can be narrowed to one question. Shall the program of information and education be directed to the individual in terms of self-interest or in terms of idealism? Dr. Harrison Lewis, a life-long worker in the wildlife conservation field, has examined[3] this problem which faces the conservation educationist and his discussion of the horns of this particular dilemna is well worth summarization and consideration.

Dr. Lewis asks whether our appeal to the public shall emphasize

[2]Crawford, J. G. *The Economics of Conservation,* Bureau of Agricultural Economics, Dept. of Commerce & Agriculture, Canberra, Australia, 1952.
[3]Lewis, Harrison F., President, Canadian Conservation Association, *A Message from the President,* Quarterly Newsletter No. 3, December 1, 1954.

principles or a higher materialistic standard of living for the individual. He states that the principles which underlie conservation, the idealistic and moral considerations that should mould and guide the relations of man to his fellow species and to soil and water, have been sought and outlined; that neither the validity of the principles of conservation nor the desirability of resource relations in harmony with these principles is in question; and that we may look on these principles as the most satisfactory reason for conservation and harmonious resource relations as the praiseworthy and inevitable outcome of their application.

Dr. Lewis contends that, in the long run, it is equally unquestionable that the practice of sound conservation of renewable resources will operate to the material benefit of the individual; that the modest restraints that conservation requires in due course will recompense the individual by a measurable augmentation of the material things which meet his needs and pander to his pleasures; and that, from this aspect, conservation is often regarded merely as intelligent self-interest.

Eight years ago, when I returned to British Columbia from Cornell University, this question that Dr. Lewis discusses was in my mind too, but less clearly. I wanted to initiate an annual Resources Conference in B.C. and of course I wanted to establish it on such a firm basis that it would be almost self-perpetuating, just as one breath leads automatically to the next. To bring about this desirable condition of self-generation it was obvious that decision had to be reached concerning the possible two appeals to the public—whether to advance the thought that the conservation of renewable resources was right because it was in accord with validated and recognized principles or whether to win support by presenting the argument that conservation is the true way to the acquisition and possession of more of the things that people want? The former appeal, as Dr. Lewis says, would lead to the surest foundation, the latter would sway the greater number of people.

It was fortunate that the founders of the B.C. Natural Resources Conference again felt, as in the case between individual and social interests and attitudes, that there was a common ground for conservation action, and that advancement and acceptance of the principles of conservation was compatible with the encouragement of intelligent self-interest. What was needed, it became evident in our early planning, was an independent, objective, non-profit, and province-wide conservation education organization, dedicated to improving the general welfare as a whole. In particular this aim was to be realized by providing the facts about our natural resources for all the people, to help them plan the disposition and use of their resource heritage in terms of their needs and desires, and for the future as well as the present.

The seed of the British Columbia Natural Resources Conference, believed to be unique among conservation organizations of this continent, was planted late in 1946, in the best of all culture media, the minds of men. The men chosen to establish the Conference were selected carefully. All were leaders active in resource fields, men who were keenly aware of the lavish blessings with which nature had endowed British Columbia, humbly grateful for such gifts, and determined to help administer, develop and use the many natural resources to the full but in a prudent manner and with an eye to perpetual maintenance and even improvement wherever possible. More than that, the founders of the Conference and their successors on the directorate, set out to persuade the general public to this way of thinking. Today the Conference maintains this as its primary aim —education of the public through dissemination of knowledge about their natural resources. We believe that we are making good progress towards reaching our goal, that of reaching the many with the message of conservation. We feel that we are moving at an encouraging pace in selling conservation by conference.

A review of the British Columbia Natural Resources Conference, its organization, aims and objectives, record of achievement, limitations, and current status, can now be given. This will be done in outline rather than objective form. In that way the adaptability of the organization to other provinces and states can be measured without reference to the conservation issues and controversies inherent in any other conservation conference.

ELEMENTS IN THE ANNUAL B.C. NATURAL RESOURCES CONFERENCE ORGANIZATION

1. Each B.C. primary resource is represented by a member of the Executive. The resource fields have been arbitrarily determined, and alphabetically arranged if classified as Industry. The resource categories are soil, water, agriculture, fisheries, forestry, mining, power and energy, recreation, wildlife, and people. Soil and water head the list because they are the fundamental resources. The category "people," actually the most important one, is placed last to represent the summation of all the natural resources.

There are thus ten Executive Members, each responsible for a specific resource. The President serves as Chairman. The Secretary and Treasurer are appointed by the Executive.

2. Each Executive Member is a leader in his field, *e.g.* agriculture, fisheries, forestry, mining. His candidature for the Executive is advanced from within his own resource group. It is understood that the honor shall rotate generally among the three sections of each resource, representing the three spheres of University, Industry, and Government.
3. In any one year, a relative balance is noted in the executive, about a third of which represents resources activities and resource viewpoints respectively of University, Industry, and Government. This they do, of course, in addition to representing a specific resource. This harmonious arrangement has become firmly established, I should say, more by custom than by constitution.
4. An Executive Member normally serves for two or three years. Thus there is a turnover of members of about one-third each year, which provides for a desirable continuity in conducting the affairs of the Conference, pursuing its aims, and carrying out the permanent policy.
5. Depending on such variable factors as incurable enthusiasm, devotion to conservation, which resource has not yet had the top honor, and the individual power of resistance, any Executive Member may find himself "pushed into the presidency." Actually, in our happy history to date, no one has either sought or shirked this senior responsibility.
6. The Conference has a constitution but it is successfully hidden in the Proceedings or Transactions of the Second Conference. Vague reference has been made to it several times and, to my knowledge, it has been read once since it was written. This, we are told, is in the best tradition of British law.
7. The Annual Business Meeting takes about 20 minutes out of the life of the delegates. Election of officers ordinarily requires about three minutes; but one year, because a dark-horse candidate for president entered the lists at the last moment, balloting was necessary and nearly double the usual time was required to complete the presidential item. Passing reference could be made to the fact that the dark horse, and winner of that particular election, was Dr. Ian McTaggart Cowan, summarizer for this 20th North American Wildlife Conference. It was his bad fortune to have just delivered a brilliant Conference summariza-

tion and it was obvious that he would be an outstanding President.

THE ANNUAL MEETING OF THE CONFERENCE

1. One three-day meeting of the Conference is held each year, toward the end of February. This time was selected because it made possible the widest representation of delegates from the three spheres of University, Industry, and Government. The end of February occurs about midway in the University Spring term, the annual reports of Industry and Government are written and filed, and the summer exodus of survey and research men from the metropolitan centers of Vancouver and Victoria is still distant.
2. Each resource is entitled to a minimum of one hour on the conference program, with each Executive Member assuming the responsibility for his own resource. If for the presentation of a particular panel more time is required for adequate presentation, Executive Members cooperate generously to make such arrangement possible.
3. Special panels on general subjects, such as climate and pollution, which are pertinent to integrated resource development and use, are presented each year.
4. Thorough integration of resources is an important aim of the B.C. Natural Resources Conference and to that end all presentations are directed to the unifying Conference theme. This can be noted in the sample program of our 8th Conference, in your hands at this moment.
5. While the social side of the Conference is not overlooked, the prevailing tone throughout the three days is a serious one. The 250 to 300 delegates seem bent on benefiting from the annual three-day short-course dealing with the natural resources of B.C.
6. The Conference is open to anyone interested in the natural resources of B.C. Special invitations to attend the Conference are not issued except to the patron, who is a cabinet minister in the Government of B.C.
7. One guest speaker is invited to the Conference each year. Dr. Ira N. Gabrielson, President of this Conference, came to us four years ago and delivered a powerful and stimulating message. This year's speaker was Sir Douglas Copland, Australian High Commissioner to Canada, who spoke on the resources of his country, and their place in the economy of the Pacific.

FINANCING THE CONFERENCE

1. Delegate fee for the last few years has been $5.00. This does little more than pay the printing cost of the copy of the annual Proceedings which is mailed to the delegate without further charge.
2. To print the 1,500 copies of the annual transactions, financial help is contributed by donors from industry and by the Government of the Province.
3. As there are no salaries to pay, the operating expenses of the executive are small.
4. To relieve the not-unknown state of depletion and even deficit of Conference funds, special projects may be initiated. For example, a beautiful picture map of British Columbia was printed and offered at a modest price to the public, chiefly through two newspapers. The sale was remarkable, some 40,000 having been disposed of to date. The educational value and artistic appearance of this picture map of B.C. have helped materially to further the appreciation of the general citizen for his province and sharpen his concern for its resource welfare. In addition, of course, the sale has helped to keep the Conference solvent.

A second special project directed to advance our chief aim of getting knowledge about our resources to all the people of B.C. will keep us busy for the next year. An atlas of maps, to cover all resources including people, currently is in course of preparation. Again it will be possible to further our general aim of reaching the many with basic data about our resource heritage. At present, the Executive is seeking ways and money to finance this full-color atlas of maps but such is the good name that has been built up for the Conference, little if any finance difficulties are anticipated. The sale seems assured, for the maps will be prepared under the direction of a technical map committee, will have the full benefit of advice, on a free consulting basis, from the Executive Members representing the individual resource fields, and the maps will be so designed that it is hoped they will be found indispensable in school, office, and home.

ACCOMPLISHMENTS OF THE B.C. NATURAL RESOURCES CONFERENCE

1. A *permanent* and *independent* conservation organization has been established in British Columbia, and in the eight years of its existence has built up a respected position in the minds of all sections of society.

2. The organization has persistently and in large degree successfully pursued its objectives. Comment on two of these objectives will illustrate.

   Objective: *"To contribute to public understanding of our primary resources."*

   Free and wide distribution of the annual Transactions to all senior and junior High Schools, Universities, public libraries, provincial newspapers, Members of Parliament, community leaders, and senior government officials throughout the Province has been our chief contribution in discharging this objective.

   The annual Transactions of the B.C. Resources Conference are regarded as the most authoritative and most up-to-date references available about the resources of B.C. They are in daily use in educational institutions, in government circles, in business, and in the home.

   Objective: *"To emphasize the principle that all our resources are interdependent and hence their development and use must be coordinated."*

   There is a place on the program each year for each resource. Interrelationship between resources is stressed on the platform and during the intermingling of the delegates between sessions. In the past few years this principle of interdependence has been extended into human relations. Cooperation has been woven more into the every day business relations among the delegates, who by and large attend the Conference each year and who, in many instances, have come to know each other on an informal basis.

   It is important to note that the delegates are *leaders* in resource fields and what they say and do is advanced by their employees. Thus the delegates are the pebbles that cause the ever-widening ripple on the waters of conservation, so to speak.

3. Each year about 250 delegates attend the Conference. They represent the working parts of all of the primary resources, including administration, survey, research, development, management, and use. Furthermore, the viewpoints of all the interests, those of University, Industry, and Government, similarly are represented.

4. Public relations aspects of the Conference continue to expand as the years roll by. Press, radio and now television coverage of Conference papers and discussions has moved steadily towards the "front page." It is of interest to note in this connection that

Conference program items and features have been given national radio hook-up in the last three years.

In the summer of 1954 the Executive accepted the invitation of the Pacific National Exhibition, rated as the second largest in Canada, to present twice-daily short talks to the public on the natural resources of B.C. with emphasis on conservation.

Other avenues for information and education dissemination continue to open up before us.

5. The Conference presently is undertaking the preparation and publication of an atlas of maps for B.C. to be ready for sale in 1956. This effort, which will probably cost about $50,000 as estimated at this time, has been discussed previously.

   The executive is convinced that the strongest and farthest-felt conservation education effect is obtained through the medium of maps which are specifically and specially prepared for general use.

6. The Conference Executive feels that a great challenge must be met in the immediate future, the pressing need for preparation and publication of an authoritative but *popular-style interpretation* of the valuable and authentic natural resource information presently contained in the annual Transactions. The excellent, but often quite technical, papers and panel presentations are not suitable for teen-age consumption.

   Until this interpretation is made, and a popular type book established as the key reference work to the story of the conservation of natural resources in B.C., the Executive knows that its prime purpose of reaching the many, such as the school children, instead of the few, such as the school teachers, with conservation information and education, will only have been realized in part.

Not all of the story of the remarkable impression that can be created by an annual Conference such as the B.C. one has been told. Enough has been said, however, about the elements of the B.C. Natural Resources Conference to suggest, perhaps, that its organization and operation would succeed equally well in every province and state on this continent. That B.C., with a population of about one and a quarter million can support a high quality medium or clearing-house for conservation information and education should be encouraging news to other North American political units with equal or greater population.

The B.C. Natural Resources Conference could be taken as a model or guide in organizing provincial and state conservation education bodies elsewhere, much as this admirable North American Wildlife Conference once served in some ways to create the pattern of the B.C. Conference. If the B.C. venture with its multiple-resources emphasis is emulated it is hoped that formation of such organizations and their operational policies will be based as far as applicable on the following statements and recommendations, to which the success of the B.C. Conference has been attributed:

1. The Conference is maintained as an *independent, objective organization,* and is respected as such. "Axe-grinding" is confined strictly to recommendations for advancement of the general welfare through conservation of resources.
2. The major aim of the Conference is *educational,* the objective being to bring the facts about the natural resources of the province to the attention of all its citizens.
3. The Conference is not an action body. Resolutions are not permitted, though they were in the first few years of the Conference. *Recommendations* as to conservation policy, however, are strongly encouraged.
4. High quality performance is expected of Executive Members in the exacting duties that are theirs, and high quality papers and panel presentations are demanded from those participating in the annual meeting.
5. *All* the primary resources are represented in the Conference, on the Executive and on the program, on a basis of equal importance.
6. Conference speakers are urged to prepare their papers for the *general* reader and for delegates representing other resources, not for their colleagues or other experts in their own special resource field.
7. The principle of interdependence of resources, and the corollary principle of interdependence of human relations, are kept before members and delegates at all times.
8. *All* segments of society are recognized through the voices of University, Government, and Industry, both in the Executive and on the Conference floor.
9. The B.C. Natural Resources Conference Executive body feels that a continuing force, in the person of a permanent Secretary, is essential to inspire and sustain the long-range objectives of the Conference. He should be trained in the field of conservation information and education.

COMMENT

A final word needs saying. The B.C. Natural Resources Conference is far from perfect as a conservation education agency. Many of the statements I have made in this paper are only half true. They describe standards of perfection not always achieved. We aspire to high ideals but at times yield to the ordinary frailties of human nature. Despite weaknesses, however, we believe that progress is being made in extending conservation information to the majority of our citizens. What we have said and printed enjoys a wider and more enthusiastic reception as the years go by.

DISCUSSION

MISS IRENA BAIRD [Ottawa]: As I have been listening to these papers I have been thinking that this is a tremendously important conference. During these last two days, two very disquieting questions have come to my mind. Now, you take our session this morning. Public relations covers what we are all trying to do. It is of major importance.

We are the breech between the field man and the lab man, to put it broadly. I would like to toss this question out at you. Don't you think that this sort of thing is important enough to consider at a General Session? Don't you think that the Conference at large would be benefited if each of us knew more about what we are trying to do?

My second question is, I wonder if we are preaching somewhat to the converted all the time. I think there is a danger perhaps that we might be. I would like to see another General Session another year consisting of a panel to which we invite a managing editor, an industrialist, a head of a sportsmen's group, and a teacher, perhaps, which would bring some of these people into our discussions and we could really trade viewpoints; not polite cliches, but viewpoints. I am just tossing this question out to the panel and the gathering as a whole.

DISCUSSION LEADER JACKSON: Thank you very much, Miss Baird. I think both your questions are pretty much academic, but we are all prejudiced, you see, and that makes a little difference. I quite agree with you that these things are important enough to justify our giving them greater attention than we have. I know the members of the panel agree; I doubt that there is anyone who would seriously disagree. They are points of view which we will keep in mind in recommending for next year's program.

Are there others of you who have something to say?

MR. FRANK CARLSON [Connecticut]: In regard to this business of discussing the various problems which one must face in dealing with resources, we at Yale have been having numerous seminars. We, too, have been discussing it from the conservationists' point of view. But we have been eliminating the industrialists in many cases or the person who can only look at it from the purely economic point of view.

In connection with this, I would like to throw out a couple of things on conservation. You might have two phases of conservation. In the first you have the practical aspect of conservation; that is, the actual work that is being done in the various fields, soil, water, flowers, streams, and so forth, and above this you have a conservation philosophy which can look at these resources as all belonging to a unit. Now, when you are working with people and say "this idea of a watershed," it should be put over to them so that they can see the various units of the watershed being interrelated. Many times the emphasis is too much on the point that part of the watershed should be treated separately.

In other words, are we just working with a series of inanimate objects rather than the single living organ?

DISCUSSION LEADER JACKSON: It's a good point of view and a good question. I might ask Dave a question myself. You know, historically, at the North American Conference, we shy away from placing ourselves on record in resolutions, and that sort of thing, and I wonder what sort of attitude they take toward that particular thing in British Columbia. Dave, would you mind explaining?

DR. TURNER: We started off with the idea that all resolutions would be thankfully accepted. We carried on that business for the first years of the organization—two or three years. However, instead of finding ourselves becoming an action group, we found we were rapidly becoming a pressure group. We realized quite quickly that we had ten resources represented in our audience, so that if the mining group, for instance, wished to offer a resolution and get the backing of this Conference group as a whole, they thought this would be an excellent spot. I'm just using mining as an illustration. They didn't actually do it, but some of the groups did.

They proposed resolutions and tried to get an endorsement from the audience as a whole. But, fortunately, our audience was composed of just about the best scientific brains in the academic, administrative and management fields. They said immediately, "We won't deal with that. Are you expecting us to discuss and decide that this is a good resolution in mining when we are foresters, agriculturalists, biologists, and so on?"

Now we have no resolutions at the Conference, but we do recommend that committees and panels, if they are so minded and are agreed, bring in strong recommendations, for instance, regarding the establishment of pollution control agencies; that is fine. Recommendations, yes; but no resolutions.

MRS. FLEMING: I think it is pertinent right here to mention, in connection with what Miss Baird had to say and also Dr. Turner, that the South is certainly aware that you cannot consider a part without considering its relation to the whole. At the last meeting of the Southeastern Game Commission in New Orleans, they invited in soil, forestry and water representatives for the first time, and also they had a public relations panel for the first time.

MR. CLAUDE GRESHAM: I would like to add one point to this. We feel, also, the thing that Rose said there. I know of two instances where all of the resources in a particular area have been called in. I'm not particularly familiar with one of them. Maybe it was California, and maybe there are some people here who have more information than I do.

Out there they have an annual breakfast. I believe it is in San Francisco, and to it are invited all of the leaders in the various resource fields, as well as administrative and political leaders in the state.

In Louisiana about a year ago we had a meeting of all the resources connected with our marine area. It was for one purpose only; it got them together, and we put down in black and white for the first time, as far as we knew, the exact relationship of how one industry affected the others. The oyster men told their tale of just how oil industries were affecting them, and in many cases, just by bringing these facts out into the open, the solution of many of the difficulties immediately became apparent. I think that is the coming thing.

DISCUSSION LEADER JACKSON: Good, Claude. Thank you.

Dr. Foerster, if you would like to comment, we would like to hear from you.

DR. R. E. FOERSTER [British Columbia]: I would like to take this opportunity of pointing out that while the program today seems to be devoted largely to educating the public in conservation, I think our B.C. Natural Resources Conferences have indicated very strongly that one of the first things we have to do is educate the members who are engaged in exploiting resources before we worry about the public.

I think it was very evident at the first Conference we had that foresters had very little information on the effect of their forest methods on fish, for example. The power interests had no realization at all of the effect of their operations on fish.

The interrelationships between the various resources were not quite understood and not even appreciated, and certainly not given any consideration in the programs being devised. I think that one of the very important things that our Conference has done is acquaint other natural resource sources with the importance of considering the other natural resources.

I forget which Conference it was now in which was discussed the multiple use of our natural resources. I think the information that was given to the utilizers of our natural resources was of much greater importance than impressing the public. I think it is very obvious that most foresters, fishermen, and so forth, are really less acquainted with conservation generally than many of the public generally.

Therefore, I think we have to start first with the people who are exploiting our natural resources and acquaint them with wise conservation measures before we look too much to the public generally. To accomplish this end is a major aim of the B. C. Natural Resources Conference.

TEACHING FIREARMS SAFETY IN PUBLIC SCHOOLS

JOHN E. DODGE

Fish and Game Department, Concord, New Hampshire

A conservation information opportunity exists when you can sell an idea to the group best equipped to put it into practice. New Hampshire's plan for teaching firearms safety within the framework of the public school program has won wide recognition as the most logical step yet undertaken to reduce accidents due to gunfire among teen-agers on a year-round basis. As in driver education, which national statistics have proven highly effective, this training of juveniles is calculated to have immediate as well as cumulative effects upon adult behavior patterns. Responsibility rests where it should, on local school authorities, while primary reliance is placed upon maintaining standards of training, rather than upon any test to screen out incompetents from the license buying public.

A practical program for firearms education has been available to our high schools for just one year. Yet, already more than 46 of our 108 accredited public high schools and academies are participating, with a total of more than 1,600 students receiving firearms training. In New Hampshire, Driver Education was initiated in 1935. After 13 years, it was reaching 974 students scattered among 23 schools.[1]

By the end of its first year, firearms teaching had outstripped—yes, more than doubled—the progress achieved by its closest counterpart over 13 years. What brought about such prompt acceptance of a new program by local school administrators?

[1]In 1954—its twentieth year—driver education in New Hampshire processed 2,393 candidates representing 66 of our schools. As there were 23,981 students registered in all our accredited schools that year, the maximum potential was about 6,000, for the average sophomore enrollment—the school year in which most students become fifteen and eligible for driver-training—is 25.3 per cent of the total.

First, a *permissive* statute was enacted by our legislature in 1953. It authorized any school district which so wished to teach safe handling of firearms and to appropriate money for this purpose. No clause was attached to make such a course either mandatory or a prerequisite to the sale of hunting licenses to teen-agers. Currently, at least five states have followed suit or are in the process of so doing.[2] Others which have enacted requirements without providing appropriate training opportunities are encountering problems of pyramiding pressures upon instructors.

Next, the New Hampshire Fish and Game Department and State Department of Education, in conjunction with the State Police, set about promptly to provide the schools with a workable course and practical help in implementing the program, but nobody undertook to do their teaching for them. It was made clear from the outset that each school should provide its own instructors from its own staff. Fortunately, concrete evidence was available to demonstrate that such direct-skill training would be effective in reducing accidents—from the five-year record achieved by the National Rifle Association's young trainees in New York State and from results through driver programs in their own schools which had reduced New Hampshire automobile casualties among trainees by at least 50 per cent.[3]

But before our public schools could tackle the job, there were gaps

Operator's Survey, 1951	Driver-Educated	Non-Driver-Educated
Number involved in 1 accident	23	104
Number involved in 2 accidents	3	7
Number convicted of Motor Vehicle Violations	5	43

This study was based on a comparison of records of 1,270 driver-educated teen-agers with a control sample of 1,270 non-driver-educated youngsters of the same age group selected at random from the files.

Comparison of Accident Incidence by Age Groups

		1954	1953	1952
Total License Sales		245,000	236,000	234,000
Driver Age Group	25-29	2,137	2,115	2,081
	20-24	2,061	1,445	2,069
	30-34	1,793	1,332	1,900
	16-19	1,632	1,765	1,600
	35-39	1,428	1,311	1,368

Years marked by a constant increase in the total number of licensed drivers in New Hampshire, witnessed the 16 to 19-year-olds—just past driver training age—holding the relative number of accidents in which they were involved to fourth place by age groups—with actual number of accidents on the decrease.

[2]Louisiana already has a similar statute. The National Rifle Association's legislative service advises us of pending action in 1955 legislative sessions in other states as follows: Connecticut, House Bill #1406; Maine, Senate Bill #91; Washington, Senate Bill #265; Arizona, Senate Bill #104.

[3]From unpublished data of the New Hampshire Motor Vehicle Department.

to be bridged. They must believe that the need applies to entire school populations, not just potential hunters. School men accept responsibility for teaching boys and girls to live safely under modern conditions. Statistics for our state prove that a majority of gunfire accidents occur outside the hunting field, and that juveniles are involved in more than their share. Hence the program could best be launched as Firearms *Safety* Education.[4]

Make no mistake, however. Our sights are not lowered to any mere measure for prevention or prohibition; the real goal is positive skill training which will open important new windows on outdoor education for lifetime enjoyment. Inventory of activities current in a number of our schools provided convincing evidence that firearms education can do just that.[5]

Here were ideals the educator could support; it remained for us to supply the means. If the program was to be teacher-centered, our schools had no source of trained instructors, no course, no text. A

	Fatal	Non-Fatal	Total	[1]Juvenile-Shooter or Victim	Juvenile—both Shooter & Victim
1951	4	16	20	9	5
1952	3	13	16	5	2
1953	5	22	27	12	9
1954	3	26	29	15	6

[1]Juveniles are counted as those 19 years of age or less.

survey of firearms teaching across the nation indicated that the National Rifle Association's "Hunter Safety Course" provided the best available materials, but that was only a 14-page outline of concepts and information.

Hence we assembled a work team of specialists from law enforcement, education, and the sporting arms and ammunition industry to build on this framework an effective teaching instrument. Security must rest upon rigid standardization of instruction. That could best be founded on the experience of men with firsthand knowledge of New Hampshire's actual problems in the field.

The first step was a clinic for all of our 35 New Hampshire Conservation Officers and members of the State Police. This took place at the State University in January, 1954. The officers served as a test group and contributed their practical knowledge while they trained themselves to act as consultants to the schools in their districts.

[4]New Hampshire Hunting Accident Statistics.

[5]Dover High School, for example, has offered a rifle club as an activity with indoor, outdoor, and competition shooting now for six years. Initiated as a result of a juvenile fatality, it is conducted by a staff member and two skilled National Rifle Association instructors and is oversubscribed since only 66 boys and girls can be accomodated each year.

A representative of the National Rifle Association flew in from Washington to conduct the clinic. The Governor of New Hampshire, the Fish and Game Director, the Superintendent of State Police—all took the course for credit. In those three days we hammered out the rudiments of a practical course to provide sound training for high school instructors. Our working staff consisted of law enforcement men, arms company representatives, and R.O.T.C. personnel from the University who handled the live firing and range procedures.

Beyond its immediate purposes, this consultant's clinic served as detonator for a series of chain reactions which are still exploding across the state and nation. The arms companies and the National Rifle Association brought in top-flight advertising men, because they recognized that we were staging a national "first" in firearms education. Our students included top executives from the state and national level. We arranged for our own staff to make TV footage on film. We brought in name writers from the region and feature writers from our own papers. We took pictures.

None of that effort was designed primarily to secure the national coverage received. Its sole goal was to induce local and state authorities to believe that what they had achieved was important. But from it I could read you an impressive series of statistics—inches of newsprint which run into three figures and maybe four—feature articles which struck pay dirt in national magazines and run into the scores. We don't care about that—much. The important part is that national recognition for an achievement as yet unborn worked wonders with our local schools. Genuine firearms safety teaching at the grassroots is already a reality.

School administrators throughout the state climbed on the bandwagon. When we announced a clinic for high school instructors assigned for the task, the response was overwhelming. Eighty-four per cent of our school unions participated. Superintendents and principals arrived with their instructors. Neighboring states sent observers. The press was there. Tape recorders took down transcripts of all contributions, as they had done for the officers' clinic. That was in March, 1954.[6]

On March 30, the second day of that clinic, Boston papers blazed into three-column spreads. A Newton schoolboy, 14 years old, had been shot dead at his desk by a classmate experimenting with a newly acquired pistol. Our reporters had a field day. New Hampshire was

[6]At the present writing 39 of our 48 superintendents of schools (or 82 per cent) are utilizing the program in one or more of their high schools.

providing the official cure to a problem which by no means restricted itself to hunting states. Urban schools needed firearms education!

From these two clinics were we assured of a state-wide group of high school instructors and consultants, all with a common background to insure standardization of teaching imperative to securtiy in the long run. Yet there was a by-product perhaps equally as important. The joint insight and reactions of educators and law enforcement men working as a group on this common problem were now on record. A sound firearms course was there for the taking.

To my associate, Jack George, state director of physical education, belongs complete credit for transforming those many hours of tape recording into this 87-page Instructor's Manual. It is built by an educator for educators. Breaking down the subject matter into teachable units, it covers everything from equipment and visual aids to problems of school policy. Firearms Safety Education is recommended to our schools as an extra-curricula activity, not in competition with the three "R's."[7]

Because Jack is working for his doctorate in Education on this topic, no amount of double-checking is deemed excessive. The original consultants have been over the manual with a fine-tooth comb. It is now in the process of being tested on youngsters in our schools.

Seven "pilot" schools selected for this purpose have subjected their activity groups to pre-testing and post-testing by Jack himself according to the pattern described in Unit I of the Manual. Preliminary results show that 77 per cent of the students make tangible gains in the special skills tested as a result of taking the course.

Meanwhile, the Fish and Game Department has continued to do its share in keeping popular interest throughout the state at white heat. We have produced two sound-color films for the purpose. The first, "Death is a Careless Hunter" is dramatic shock treatment. It reenacts a deer hunting fatality which actually took place in our woods a few years ago, and the subsequent law enforcement routine and emotional impact on the man who did the shooting.

The second, "Tomorrow We Hunt," is the first-person narrative of how a teen-age boy who yearned to handle his own gun and to hunt used the New Hampshire Plan to persuade his Dad and his school

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[7]In unsigned responses to a subjective evaluation sheet circulated to participating school men during February, 1955, 73 per cent of the first 49 forms returned were checked affirmatively in the two highest brackets of approval on each of four key questions—(a) need for this school program (b) student approval (c) parental approval (d) value of the Manual. It was notable that student approval only went ahead of parent approval by two points. This same questionnaire inquired in which grades school men felt the program should be taught. Peak approval was for grades 8, 9, and 10.

authorities to make firearms training available. This is simply a promotional vehicle, no attempt is made to "teach the course."

General Merritt Edson, Executive Director of the National Rifle Association, calls the New Hampshire Plan the best yet developed for making firearms education an integral part of public school education. Our goals and his are identical—to enable our youngsters safely and officially to enjoy a character-building sport that is part of the American tradition. We want them to learn to shoot habitually well under controlled range conditions because we believe that will make them safer anywhere else they handle guns. And we are not blind to the fact that such training will stand them in good stead in the event of a national defense emergency.[8]

That is why this March we have scheduled another clinic, in response to the demands of our local school people. One day will be devoted to training high school instructors who missed the sessions a year ago, but the final two days will enable many of our present school instructors to further equip themselves for teaching youngsters live firing on the range. This course is to be directed by a highly qualified National Rifle Association instructor. Graduates will be qualified as National Rifle Association Small Bore Instructors.

Admittedly the New Hampshire Plan will not process so many young hunters in initial years as appear to be qualified under a mandatory statute which requires each new license holder to pass a test. Each of our schools will develop its program at the pace and to the extent for which it is ready. But in the long run, we believe that results will be far more positive and permanent.

During 1954, New Hampshire enjoyed a fatality-free deer hunting season, with 90,000 hunters roaming our woods. That, however, is incidental. If any credit is due the firearms safety education program, it derives from added publicity. The real work lies ahead.

CONCLUSIONS

It has been attempted to document how success in the promotion and development of New Hampshire's Plan for teaching Firearms Safety in her public schools was dependent upon applying the following principles pertinent to capitalizing upon any conservation information opportunity.

[8]In January, 1955, General Edson attended a national conference in Washington, D. C., of *Shooting and Firearms Education* sponsored by the American Association for Health, Physical Education and Recreation (the branch of the National Education Association with largest membership). New Hampshire's Instructor's Manual and program were presented by Mr. Jack George. The General states that current official recognition for the value of firearms instruction by the nation's educators derives from state pilot programs such as we are describing.

1) A conservation information opportunity exists when you can sell an idea.
2) To achieve this, the idea must be packaged in a form acceptable to the primary group in a position to put it into practice.
3) The timing must be such as to capitalize upon a recognized need.
4) Evidence must be presented that the means endorsed for meeting this need are realistic, and will get the desired results.
5) Requisite tools and consultant services must be readied and placed at the disposal of the activating agency before it is asked to go into action.
6) Advance and current publicity should be such as to insure enthusiastic public support for the activating agency, both at the state and the community level.

DISCUSSION

MR. KENNETH MAYALL [Toronto]: I would like to ask a rather back-handed question. I have been attending these meetings of the North American Conference since 1938. It seems to me that the Chairman, when he said with reference to one question academic and the other abstract, that that was not the case. I think there are only two kinds of really useful information, only two uses, really, that we get out of these Conferences.

One of them is the technical information that we get from technical papers which, incidentally, we get also from the regional papers of the North and Northeastern Wildlife Conferences; and the other is the matter of educating the public, and also the children. I think these are infinitely more important than the General Sessions which cover, in very abstract form, material which is covered many times over by people from Aldo Leopold up and down the scale.

My back-hand question is this: Is there anyone on the panel who would object to the idea of suggesting to the Resolutions Committee that more time be spent at future North American Wildlife Conferences in technical sessions; that is, technical sessions for education, and less time on the General Sessions?

DISCUSSION LEADER JACKSON: I would say that it is well within the province of the panel to offer suggestions to the Wildlife Institute staff and the Wildlife Society concerning the content of these programs. It is not a matter of a resolution, I would say. If your question is aimed towards enlisting the services of this panel in recommending to the Wildlife Society and the Institute suggestions for their program content next year, if it meets with the general approval of the members of the panel, I think it is well in order to ask Dan Saults, as the Chairman of this session, to take whatever steps are necessary.

This Conference accepts no resolutions as such.

MR. FARLEY TUBBS [Michigan]: I would like to make this comment, to John, particularly, because I think that the program that has been developed, or is in the process of developing, in New Hampshire has contributed materially to slowing down the demands for legislation in the State of Michigan. I want to thank you for that because we are another state who does not feel you can legislate some of these things. In other words, it does not lend itself to that type of program.

We do have existing in our school laws the right to use funds from the school monies for such training, so we have that setup that was made possible by the legislature.

There are a couple of other comments I want to make. Four or five years ago—and if I am wrong, Dan can correct me—we had the first hunting safety clinic at Haven Hill outside Pontiac, Michigan. At that time we brought in six or seven states and the Province of Ontario, and tried to figure out means of getting information that we needed if we were going to put on good safety programs. We need a backlog of information such as no one state could collect by its own efforts. That thing snowballed from there and I think there are 32 states and provinces that are reporting on the report form. The report form is not perfect. There is much that is said and gained by the report, but there is a heck of a lot to be done to make the material more meaningful.

We, for instance, lack comparisons; the number of shotguns and rifles in the field. We make a statement of accidents by rifies and shotguns, but we don't know the relative proportion to the number in use. That is one of the problems, and I think we could dig out 15 or 20 instances where, if we had a little more information of a certain type, the figures we have now would be better. But this thing we are gathering is making a good basis for firearms instruction, and I think that sooner or later when more people get to hear about this thing, we are going to come up with a lot of good ideas that have not been thought of up to date, and we will be able to incorporate them in the program.

Another item I would like to mention—and I want to back John to the hilt here, because he has done a wonderful service for Michigan—is that we are interested in hunting and the use of firearms more than from the safety angle. That seems cruel, especially if there is a mother in here who has lost a son through a hunting accident. But hunting accidents, by and large, on a national scope, compared with other things that cause accidents, are not alarming. We are, I am afraid, in our exuberance to put forth a safety training program, calling to the attention of many of the people, and the press, that we are dealing with a vicious thing here, and certainly it crops up in the minds of about five-sixths of the people in this United States of ours that the best way to get rid of this hunting accident problem is to have more firearms legislation.

I don't believe there is anyone in this room who wants that. Let's take the saner approach. Take firearms and put it in its proper place in the resource field and teach kids to use it. There will be accidents in spite of what we do the same as there have been in driver-training. Maybe we can save some. But I think the big point is to take this thing, put it where it belongs, relegate it to the right position and give the training from there on.

I might say that probably in the field of proper conservation, this thing has no more right there than, we will say, water safety or safety pertaining the use of any other resource. In other words, the school people have this responsibility in teaching kids to live a wholesome lifetime. They have the responsibility, and recognize it, and are willing to do something about it.

I think what we should shoot for, as well as firearms safety, is safety across the board in all of these things that pertain to sports particularly, and let industry handle the other.

MR. DODGE: Bud, I have an editorial comment to make on Farley's speech. I don't know how you people feel, but I don't recall any panel where somebody came along and finished the balance of the speaker's talk which time did not allow. You hit exactly the point I would have, in saying let's not concentrate on the accidents as our entire focus.

We in New Hampshire, for instance, were lucky enough not to have a single fatality during our deer-hunting season last fall. We did have other accidents, but let's remember that you and I are in business here to have more people enjoy more hunting in our states and harvest more game. Even the technicians will buy that. Harvesting more game is going to make more game. Let's get the idea in our heads that we are working with, in the same manner, those fellows in the firearms and ammunition companies who are our best supporters in this thing. We both have the same objective which is more gunning successfully done by properly trained people.

We owe each other an awful lot, and we are doing if for the same reasons.

DISCUSSION LEADER JACKSON: Thank you, John.

My responsibility up here has been to keep us on time, and we have hit it almost exactly on the nose. I am going to throw responsibility over my shoulder and keep you here for another 120 seconds while I ask two gentlemen who are in the group here, and who have long been intimately associated with the Rifle Association, Carl T. Frederick and Frank Daniel of New York and Washington, respectively, if they have any comments to offer at this point.

MR. CARL T. FREDERICK: I didn't know you were going to treat me to this, Bud. This whole subject of firearms and safety and the legislation that attends that problem in more or less vicious form has interested me for a great many years.

I come from New York. In New York they started teaching firearms safety, especially in connection with hunting licenses, outside the schools. We have nothing that penetrated any depth at all in the schools. Whether that will be possible—and I agree that that approach is a most admirable one, highly desirable if it can be done—we have not seen our way yet to do it.

Firearms safety, to my mind, has always resembled auto safety. I have always rather resented the expression "accident" in connection with their use, because, in my own opinion, probably 95 per cent of the misfortunes that occur are caused by sheer ignorance, negligence or stupidity, not to say malice, in some cases, but they are hardly true accidents. The gains that can be made through reasonable instruction are almost unbelievable for the depth and strength and extent.

Certainly, I congratulate New Hampshire on doing a magnificent job. Their approach, as I said, is different from that which we have yet been able to devise. There are differences between the two states that I think may account for that. New Hampshire has, by no means, 15 million to deal with nor the vast school population gathered together, in large part, in great cities. I think it would be an almost impossible thing at the present time to introduce such a thing in the schools of New York City or in Buffalo, say; in many other parts of the states it might be.

A voluntary approach, I hope, may succeed in getting us somewhere.

DISCUSSION LEADER JACKSON: Thank you very much, Carl. I understand there may be some comments forthcoming from some of the Sporting Arms & Ammunition Manufacturers' Institute people who are here.

MR. FRANK DANIELS (National Rifle Association): I would like to make myself heard. I would just like to say one thing quite apropos to what Farley Tubbs said in finishing up your very capable presentation. I have had the good fortune several times in the past year to attend conferences made up primarily of educators and people in the field of recreation, and while there has been very little done so far that can be spoken of in more than generalities, it is certainly obvious that professional educators, particularly those in the fields of physical education and recreation, are beginning to ask themselves some very pertinent questions.

They are becoming, I think, acutely aware of the tremendous number of people who hunt and fish, and they are beginning to say to themselves, "Why do we spend so much time and effort in the schools teaching these students to play basketball and badminton and volley ball in physical education courses, when all the rest of their lives, after they get out of school, the same youngsters are going to hunt and fish?"

It is one of the most refreshing new horizons that I have seen for a long time. It is going to place on all of us in all phases of the conservation field a tremendous responsibility because I think we are going to have to lead the way if we want to see courses in the schools on hunting and fishing, and I mean in the broadest sense of the taking of game, the preservation of game, the proper preparation of game, survival in the woods, and all that sort of thing, which is what the educators are thinking about now. We have to be the trail blazers, we have to show them the way, and it is going to be a tough job.

MR. DODGE: Now I will cut back in, if I may, to present two men from the Sporting Arms and Ammunition Institute Manufacturers, Mr. Dick Webster and Harry Hampton. Do you fellows want to get up and make yourselves known, and at least one of you say a word?

MR. HARRY HAMPTON: I think what I can add to this discussion this morning would be best said by attempting to say to you fellows that the sporting arms industry is ready, able and willing on a moment's notice to be of all possible service to you in broadening the efforts of education which you are trying to do. There can be no doubt that the over 16 million hunters who purchased licenses last year are going to increase in number, and while fishing to us is perhaps not quite so important, the 20 million fishermen or thereabouts, added to the 15 million hunters, represent well over one-quarter of the population of the United States and Canada.

We have an obligation and a responsibility to them, we feel, in the industry, and we will do everything in our power to meet that obligation, by providing tools which you, as workmen in the conservation and educational field, can make best use of.

THE CHALLENGE WE TAKE

Appraisal of the 20th North American Wildlife Conference

IAN MCTAGGART COWAN

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In introducing this appraisal of the Twentieth North American Wildlife Conference I cannot neglect a sincere tribute to the Wildlife Management Institute that annually makes this conference possible and to Pink Gutermuth, Gordon Fredine, and all who have given so tirelessly of their thought and time to making it a success. One thing about the Institute and the conference seems to me particularly noteworthy when measured against our theme of challenge.

This is that the conference is truly international and stands as an example of the only framework within which we can meet the needs of the wildlife and fishery resources of the American Continent. That of developing common objectives, and sharing common problems while acquiring an understanding of the complications imposed by three differing political systems inherent in the maintenance of our national autonomies.

The problems that confront us in maintaining or developing our resource potential are so many and so varied that the tendency towards increased specialization in the training, activity and viewpoint of each one of us is almost irresistible.

However, this trend leads inexorably to the end that each specialist, burrowing ever deeper, and mole-like throwing up detritus on his back trail, becomes isolated in thought from his fellows, who are often parallelling him, out of sight. Our dilemma may become that of the mole in the mating season, with relatively well-formed immediate objectives but no sure idea of which way to go to achieve them.

In bringing us to the surface once a year, this series of conferences has a most fertile influence. It is almost unique among the resources and has influence outside our own continent.

One caution I would make to future planners, authors and editors of our conferences: In the spring, as the prairie chickens converge on their booming grounds and the wildlife men follow suit, let us not lose sight of the fundamental point that, while noise may be the most obvious feature of booming ground activity, fertility is the most important one.

In pursuit of my assigned task I have, in a few cases, entertained the feeling that what I was reading had so little of the vital germ of fact in it, that its chance of fertile union with an idea was remote.

The many speakers at this conference have shown us much of the variety and magnitude of our problems and have measured yardage gained or passes fumbled. It is my purpose to review what seems to me to be some of the high spots of our deliberations and to convey to you the ideas they arouse in me. For purpose of this appraisal I will comment separately on the following aspects of the challenge we take—1. Education; 2. Research; 3. Management; 4. The political or social position of our resource. If there were important facts or ideas in papers that have not reached me, I can only say that I regret their absence but the fault is not mine.

Before embarking on my separate appraisal of the several aspects of our deliberations, I feel I must first comment on the most able presentation by Vogt. There must be few among us who would deny the truth of the picture he paints. We are all too well acquainted with the consequences of overstocked habitats and with the inevitable results of geometric progression where new mouths, be they animal or human, are concerned. It is right and very necessary for us to concern ourselves with this all-pervading problem of increasing human populations, for as Vogt has pointed out, and Ackerman, Swift and Wagar have detailed, the problem of human increase transcends all others in shaping values, and it is upon relative values in terms of human needs that the fate of our wildlife resource rests.

When faced with the full realization of this incontrollable tide of human reproduction, the heavy hand of pessimism is apt to grip us, and some may ask, What's the use, what can we hope to achieve?

Gentlemen, there is no more sterile human emotion than pessimism, the problem presented to us by Dr. Vogt is potentially much more subject to solution than is the inevitability of eventual death that each of us faces from infancy, and yet we do not let this interfere with our striving towards the ideals we have set. So while the larger solution is being sought, let each of us contribute our best to the immediate problem in hand, with an eye on the compass, we continue to put our backs to the oars.

Education. We cannot overestimate the importance of education in the development of sound conservation to meet ever-changing human needs. Under education I refer not only to the training of new men for research, management and protection staffs of our wildlife and fisheries agencies, but equally to the entire field of public enlightenment. For under our form of political organization, regulations cannot far outstrip public understanding and cooperation.

Thus the university teacher and the few who on podium, or through press, radio and television, shape the attitudes of the many, are on the same team and hold positions of key importance.

The growing recognition of this is revealed in the program of this conference wherein 10 of 57 papers have dealt with education in one aspect or another.

I regret that no reference has been made to the training of the future professionals in the biological resources. I feel most strongly and with increasing conviction that the concentration upon the technical aspects, as seen in so many colleges on this continent, is fundamentally unsound. The growing need is for men who are, first, soundly trained biologists educated to the point where they can achieve a perspective of human aspirations and an understanding of the political organisms they have developed. Upon this foundation must one develop the thought processes of the scientist and the detailed knowledge appropriate to proficiency in the chosen field. It is easy to train technicians; it is very difficult to produce socially conscious, well-educated men and women who are also competent in the technical field, and the criticism levelled at so much of our present training in our own profession is equally applicable to the several other applied sciences. May I also commend public speaking as a worth-while art in our calling. The inarticulate are not followed.

Turning to the larger field of public education Cottam has pointed out forcefully that we spend too much of our time preaching to the saved. Evangelism, to be most effective, is best directed towards the damned. I can but echo Cottam's words when he says: "Our greatest need in the wildlife conservation field is to devise more effective ways of selling conservation to the public. . . ." We must recognize that wildlife management involves far more than meeting biological needs of wildlife. It requires the shaping of ideas, the subtle direction of human activities which affect wildlife and human use of the wildlife resource. Good public relations are fully as important as sound programs and brilliant research.

In detailing this theme, we have had several worthy papers. Stiltes has reminded us that radio is still a useful vehicle being neglected by many conservation agencies and Harlan has outlined his experience in the use of television. He very rightly states that wildlife and fisheries conservation is a "natural" for this medium. With some concept of the new medium and with a keen imagination, the opportunities for programs of the greatest human interest are unlimited, and by skillful preparation each can do more than interest; it can educate, it can spread the gospel of wildlife conservation.

The very able critique of conservation magazines by Gregg would bear study by the majority of state conservation agencies. He makes a good case for the trained editor with experience in periodical work as the key man in this important sphere. One added caution I would make, this editor should also be biologically informed. If he is not, you give rise to an editorially excellent magazine loaded with inaccuracies and inconsequentials. Sales techniques and the basic tenets of science, upon which our resource must rest, are poles apart and some sort of equatorial tryst must be arranged, within the framework of the ideal conservation editor.

I must refer here to the well-known fact that the printed word will only educate the already sympathetic and that visual materials, as by television and motion pictures, or the demonstration or personal approach, are the only ones likely to make inroads on the very large group of apathetic or unsympathetic in the populace. These then should be our tools of evangelism, while the written word keeps the interested informed.

Gregg further infers that any educational effort to be immediately effective is best directed towards the leaders in the human community, as whether we delude ourselves or not, they are the people who influence the decision and actions of their fellows.

Other aspects of conservation education have been dealt with by Huber, who deals largely with the workshop or project approach with very interesting and valuable examples. Turner has outlined the accomplishments of the British Columbia Resources Conference. Speaking as a long-time associate with this effort, I can speak in the highest terms of its real achievements in furthering an improved attitude on the part of each major resource, for the needs, aspirations and contributions of the others. The extent of this achievement can be indicated when recently a newly appointed commissioner of the provincial Hydro-Electric Commission told me that one of his first tasks was to convince the wildlife people that engineers had souls. This may come as a new idea to some of you too!

Research. There have been very evident trends in research in the wildlife field, and I think they are well indicated in the papers submitted to our conference. A decade ago our research men were largely pre-occupied with techniques of range appraisal and with the developing techniques for gauging the size of animal populations. General research then was almost all directed toward getting reasonably accurate answers to immediate problems and while much of it still is, there is a grow realization among the research directors that such hit-and-miss programs will have limited applicability and are unlikely to provide the source material for major advances.

We still need greatly refined methods of census so that we may gain better understanding of trends in change, increment, and loss. We have had two contributions in this sphere. Moore described a new refinement in census of upland game birds. I'm only sorry he did not send his paper for review. Banfield, Flook, Kelsall and Loughrey outline their extensive experience in the use of aircraft to census big game over very large areas, and Fisher uses the same technique on the harp seal. I'm sure they would be the first to admit that they are not yet satisfied with their results, and I am confident that their continuing studies will equip us with still further refinements of the aerial survey techniques.

The twin concepts of population, and of limiting factor controls have been, in my mind, the greatest contributions of research to management of wildlife and fisheries stocks of the last quarter-century. Inevitably much of our present and future research is and will be devoted to drawing in the details of our currently rather rough picture of population phenomena.

During this conference, one of the most important papers presented has been Elder's report on fluoroscopic measurement of waterfowl hunting pressure. This is a valuable paper not alone in that it provides us with a new tool for measuring hunting pressure, but it gives us a cross-check on the meaning of waterfowl band recovery that will necessitate revision of a number of current concepts. Elder's long-continued devotion to this field of research and his skillful and imaginative analysis of data is in the best scientific tradition. His results have the added advantage of being immediately applicable both in research and management.

The study of mortality factors is of the greatest importance and these have been represented in papers by a series of researchers on a variety of game species. More and more, mortality factors are pointing to nutrition as one of the vital integrating forces and we are given a further instance of this by Thompson for lemmings. This type of study expanded into a team attack at a level involving the fundamentals of animal physiology, and continued over a period of years, appeals to me as offering excellent prospects of solving the cycle question.

Leopold in his summary of the 19th Conference remarked upon the dearth of studies in this field. Might I urge the biologists not to let the mathematicians scare them off. This is a biological problem, the animals are not dying of calculus or statistics, and there are such things as cycles in animal populations though we have sometimes gone overboard in ascribing all fluctuations to this category.

Of the most tremendous importance in this respect is the type of physiological study reported by Christian and Davis on the shock syndrome, as seen in the adrenal glands of mammals, as a sensitive response to population size. In this same way, basic studies of wild animal physiology of the type reported by Stullken and Kirkpatrick for the sea otter, are of the most urgent need if we are to reach an understanding of population behavior.

We have had a particularly impressive group of fisheries papers that have served to reinforce the realization that in many cases the standard of achievement in fisheries research and management surpasses that of terrestrial wildlife management. There are several reasons why this is so that need little comment from me, but one seems to be of fundamental importance.

The plethora of terrestrial species and environments has led to a multiplicity of *ad hoc* studies each concentrating on the species as such, or the environment as such, rather than seeking to use the species or environment to discover basic principles of animal reaction to physical environment, associates, or members of the species in different density. This lack of focus inevitably arises from too early concentration on techniques and details within a narrow field, and may I reiterate that a wildlife research worker to pull his weight in research today must be first and foremost a good biologist, with the breadth of background in the other sciences that this entails.

Management. Those of us dedicated to the perpetuation of our biological resources recognize a continuing development of dams on the western rivers as inevitable. Our greatest problem then becomes the amelioration of the obstructive effect upon our salmon and trout populations moving upstream to spawn. A notable contribution to this is the paper by Hourston and his associates. This is a most satisfying study not only in the breadth of biological awareness revealed but by the incisive reasoning and realistic treatment of economic and engineering factors inseparable from the study. No airy dreamers these men—may their gene flow reach flood level.

The other fisheries papers presented reveal a realistic and imaginative approach to problems both economic and scientific. While it is a minor point in Kerswill's study, his report of the devastating effect of forest spraying with D.D.T. should be documented in full and given wide publicity.

It is most interesting and encouraging to see an increasing number of wildlife men challenge accepted ideas and management techniques. Thus Miller in Alberta has revolutionized the trout management programs of our western mountain area.

And at our present session we have seen Lauckhart present that elusive virgin the hen pheasant stripped of all her mysteries. Some will say, "for shame, Burton, no lady should be treated thus." Others will look forward to the potential thus exposed.

We have heard Hickey ask the question, "Is there a scientific basis for waterfowl management?" and come to the conclusion that our program is more of an art than a science. I regard this as no condemnation. Wildlife conservation will always be an art, but we must strive to make it ever more soundly based upon the achievements of science.

The team approach in research and management is a fruitful trend and may I point to the number of joint authorships on our convention roster. Noteworthy was the contribution of Neils, Adams and Blair to the ever-vexing problem of big game damage on forest land.

We must meet game damage control with all our ingenuity, not alone as a management problem but as a vitally important one in maintaining or improving the position or our resource in the public sympathy. A useful stop gap was described by Paynter.

Social. The greatest challenge we face is clearly mirrored for our view in the series of excellent papers reviewing our progress through the last two decades; for the trends reaching us out of the past are surely continuous into the future.

We can see ahead a great trampling horde of humanity clamoring for outdoor recreational opportunities and we have reason to be appalled at the task that confronts us. Not alone to provide these opportunities but to provide them in an environment still bearing some resemblance to unspoiled nature we hold as our ideal. We compete for the minds of men and our task, as Wagar so clearly and convincingly has told us, is to disperse the great cloud of ignorance, biological, ecological, and recreational, that leads the business men in government to carry their two-color complex and Chamber of Commerce ideals into the field of biological resources management where they become a veritable Trojan horse.

To gain support for our cause we have emphasized the economic values it represents and have soft-pedalled the great intangible forces of recreating the human soul, because we have not known how to talk about them in words of mutual understanding. We have stood embarrassed and tongue-tied in the presence of the dollar.

We must secure many more areas whose primary purpose is to maintain our resource in its unspoiled environment; we must be prepared to accept the idea that some land we now regard as ours will, under increasing population, be put to the more immediate purpose of producing the food and energy upon which our nations thrive.

However, let us never forget that after the basic essentials of food, clothing and a roof have been met, the rest of the expenditures by any man are a matter of his personal choice. This choice is directed by advertising and salesmanship into the various trappings that we call our way of life, the wildlife dollar is the same as the automobile dollar or the whiskey dollar, and we must compete for it with all our imagination.

Even in the most heavily populated areas of Europe there is evidence in abundance of the all-pervading kinship of man with the land and its other inhabitats, and this we must capitalize on, build a greater bond of understanding among men for the ideals we hold and their value in our civilization. So as I began with education, I end with it.

It seems to me that the long term challenge we take can only be seen in its broad generalities. The details will be filled in by forces in human affairs that we cannot yet gauge but must attempt to guide.

Therefore it is of paramount importance that at every level we recognize fully the basic principles upon which we must build our programs.

We must be resilient, quick to adjust to the over-all best interests of the development of the resources of the country, and to make the most of all opportunities to maintain our resource as a secondary as well as a primary product of land use. We must be skilled as scientists, completely dedicated to the ideal we support, and more and more of us must become skilled in the art of directing human attitude and shaping values; for it is in this larger arena of human affairs that we will triumph or lose our shirt.

As Wagar has so aptly said, we must remind those who think lands worthless unless swarming with people, of the effort and money expended as an interest in heaven by people who have never been there.

ACKNOWLEDGMENT OF APPRECIATION

C. R. GUTERMUTH
Vice-President, Wildlife Management Institute, Washington, D. C.

Friends, we are about to close another successful Conference. While listening to that excellent critique by Dr. Cowan, I pondered the complexities of the undertaking, and of the effort that goes into the appraisal of the entire program of an international meeting of this magnitude. It is an arduous task, and when I search for words of praise for a job well done, it makes me realize that a mere expression of appreciation is inadequate.

I am responsible for starting these program summarizations at the 1946 Conference in New York, when the beloved Dr. Rudolph Bennitt pioneered the way. Now, it looks as though I have been caught in my own trap. They got me to agree to summarize the program of the Northeastern Conference in Atlantic City next week. Ian, I only hope that it will be possible to do half as well as you—thanks very, very much. You certainly did a grand job.

In behalf of the Conference sponsor, the Wildlife Management Institute, I wish to thank all of the organizations, agencies, and individuals, that contributed to the success of this year's Conference. It really has been a pleasure to return to Canada, and to be able to review and discuss mutual problems and programs. Incidentally, the last of these international Conferences to be held here was in Toronto in 1942.

We want to thank the members of The Wildlife Society, and C. Gordon Fredine, in particular. Of course, we are grateful to all those on the Program Committee, which as you know, includes virtually all of the national organizations, but, in addition, Gordon did a splendid job and the program of the technical sessions reflected his efforts.

On the part of all these conservationists, thanks to the press. Although I really haven't been able to look at a newspaper for several days, I am sure that the Conference has had good coverage. Kindest regards to the newspapers and wire services, and to the magazines for those items that will appear later.

In your behalf, and that of the Institute, thanks to the Sheraton-Mt. Royal Hotel and to the Montreal Convention and Tourists Bureau; the Bureau was most efficient in handling the room reservations, and in helping with the Conference registration.

It is believed that you will agree that we had a good banquet last night. This room was filled to capacity, and the musical and variety show produced by Jack Morton Productions was up to the standard that we have tried to maintain. Thanks to the Black Watch Regiment for furnishing the pipers, so that we could adhere to and enjoy that unique Canadian custom of "piping in" the celebrities at the head table.

The Institute staff always is glad to see each of these large annual conferences come to an end. This is the tenth one that I have engineered, and it really is tiring. There are a couple of other "behind the scenes" folks who also merit public recognition at this time, because they certainly deserve credit. I watched those two work tirelessly again this year, early and late, and know of their patience and endurance the year round, if you please. Permit me to ask Mrs. Gabrielson and Mrs. Gutermuth to stand.

Now then, the registration was about what we expected. We never are able to get more than about 75 per cent of those in attendance to enroll. Likewise, we knew that many people from the States would not be able to overcome the obstacles of both distance and out-of-country travel. We are delighted, therefore, that you Canadians helped to establish a good attendance record. We had a capacity crowd of 506 at the banquet, and at the last check, we had more than 700 registered. That's not bad for our kind of a meeting in Montreal, *in March*. I can imagine what the turnout would be in summer. Even so, the attendance record is not the measuring stick of the success of these international conferences. We must weigh the program in its many aspects, the related meetings, and the personal contacts that are made. Then again, if these Conferences accomplished nothing more than to give us such splendid appraisals of the current trends, like Dr. Cowan's summary, these yearly get-togethers would justify themselves.

I do not know where we will meet next year. I am sure, however, that we will go South. Into the deep South, I hope! Thanks to all of you, and a safe trip home. We will be looking for you again next March. Happy Landings!

REGISTERED ATTENDANCE AT THE CONFERENCE

ALABAMA

Ralph H. Allen, Jr., George A. Averitt, I. B. Byrd, Verne E. Davison, J. G. Givens, Arnold O. Haugen, Frank Haynes, W. L. Holland, J. E. Keeler, Claude D. Kelley, Carl H. Lay, Paul Martin, J. H. Parsons, Jas. R. Parker, G. W. Ponder, E. C. Suttle.

ALASKA

A. W. Boddy, J. L. Buckley, Mrs. J. L. Buckley.

ARIZONA

Fred Faver, John Hall, Wm. R. Hanson, Charles Roger Hungerford, Max Layton, Lyle K. Sowls.

ARKANSAS

S. C. Dellinger, T. H. Holder, T. A. McAmis, F. H. McCormack, Earl Wells.

CALIFORNIA

David R. Brower, Fred P. Cronemiller, George D. Difani, Richard E. Genelly, Ben Glading, Luther G. Hester.

COLORADO

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