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INTERNATIONAL BIOLOGICAL PROGRAM

HEARINGS
BEFORE THE
SUBCOMMITTEE ON
SCIENCE, RESEARCH, AND DEVELOPMENT
OF THE
COMMITTEE ON
SCIENCE AND ASTRONAUTICS
U.S. HOUSE OF REPRESENTATIVES
NINETIETH CONGRESS
FIRST SESSION
ON
H. Con. Res. 273

MAY 9; JUNE 6; JULY 12; AUGUST 3 AND 9, 1967

[No. 6]

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HOUSE CONCURRENT RESOLUTION 273—CONCURRENT RESOLUTION EXPRESSING THE SUPPORT OF THE CONGRESS, AND URGING THE SUPPORT OF PERSONS AND ORGANIZATIONS, BOTH PUBLIC AND PRIVATE, FOR THE INTERNATIONAL BIOLOGICAL PROGRAM

TUESDAY, MAY 9, 1967

**HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND ASTRONAUTICS,
SUBCOMMITTEE ON SCIENCE, RESEARCH, AND DEVELOPMENT,
Washington, D.C.**

The Subcommittee on Science, Research, and Development met at 10:27 a.m., in room 2325, Rayburn House Office Building, the Hon. Emilio Q. Daddario (chairman of the subcommittee) presiding.

Mr. DADDARIO. This meeting will come to order.

My apologies to both Dr. Revelle and Dr. Bennett, who will be the witnesses here this morning. I had one of those early morning meetings which kept me beyond the projected period of time.

The meeting this morning is on Concurrent Resolution 273 as proposed by the chairman of the full House Committee on Science and Astronautics, Mr. George Miller, of California. It concerns the support which the Congress should find and declare for the International Biological Program, and it is a very important resolution which we would hope would get through both the House and the Senate in an expeditious time.

(The resolution is as follows:)

[H. Con. Res. 273, 90th Cong., first sess.]

Resolved by the House of Representatives (the Senate concurring), That the Congress hereby finds and declares that the international biological program, which was established under the auspices of the International Council of Scientific Unions and the International Union of Biological Sciences and which is sponsored in the United States by the National Academy of Sciences and the National Academy of Engineering, will provide a unique and effective means of meeting the urgent need for increased study and research related to biological productivity and human welfare in a changing world environment.

The Congress commends and endorses the international biological program and expresses its support of the United States National Committee and the Interagency Coordinating Committee, which together have the immediate responsibility for planning, coordinating, and carrying out such program in the United States.

The Congress calls upon all Federal departments and agencies and all persons and organizations, both public and private, to support and cooperate fully with the program and the activities and goals of such Committees.

Mr. DADDARIO. We are pleased, as we always are, to have Dr. Roger Revelle, who works with our permanent Science Panel, is one of our country's distinguished scientists, and is here this morning under another title as Chairman of the U.S. National Committee for the International Biological Program.

And we are, as I have said, Dr. Revelle, happy to have you here, and as always, are anxious to listen to you. If you would proceed.

STATEMENT OF DR. ROGER REVELLE, DIRECTOR, HARVARD CENTER FOR POPULATION STUDIES; ACCOMPANIED BY DR. T. C. BYERLY, ADMINISTRATOR, COOPERATIVE STATE RESEARCH SERVICE, U.S. DEPARTMENT OF AGRICULTURE; DR. HARVE J. CARLSON, DIRECTOR, DIVISION OF BIOLOGICAL AND MEDICAL SCIENCES, NATIONAL SCIENCE FOUNDATION; AND DR. FREDERICK E. SMITH, SCHOOL OF NATURAL RESOURCES, UNIVERSITY OF MICHIGAN

Dr. REVELLE. Thank you very much, Mr. Chairman, members of the committee.

I have a prepared statement on House Concurrent Resolution 273, which I would like to submit for the record. And if I may, sir; I will talk from that statement.

Mr. DADDARIO. You may proceed in any way you would like, Dr. Revelle.

(The prepared statement submitted by Dr. Revelle is as follows:)

PREPARED STATEMENT BY PROFESSOR ROGER REVELLE, CHAIRMAN, U.S. NATIONAL COMMITTEE FOR THE INTERNATIONAL BIOLOGICAL PROGRAM, NATIONAL ACADEMY OF SCIENCES

In our times of unprecedented change, biologists are well aware of the rapidly growing ability of their fellow human beings to alter the face of the earth through technology. But they are equally aware that these alterations can bring about far-spreading and often destructive changes in the web of life that is stretched so thinly over the surface of our planet. Our technology has outpaced our understanding, our cleverness has grown faster than our wisdom.

Technology produces more than physical change. With his newfound powers man has also radically altered part of his own biology; on a worldwide basis, his average length of life has nearly doubled during the last few decades. In many regions of the earth this rise in life expectancy has not increased human happiness; rather it has multiplied human misery. To achieve a decent life for the living generation, further change in our biology may be needed: we may have to reduce fertility as reckoned on a worldwide basis to levels below any previously attained. To meet the needs of human life and human dignity, there must also be a vast increase in the productivity, and this will be doubly true for the generation that will be born before the end of this century.

Because of our limited understanding of the relationships among living things, we are limited in our ability to predict the effects of technical change or to help the technologists conserve the values and utilize the abundance of the world of life. Our goal should be not to conquer the natural world but to live in harmony with it. To attain this goal we must learn how to control both the external environment and ourselves. Especially we need to learn how to avoid irreversible change. If we do not, we shall deny to future generations the opportunity to choose the kind of world in which they want to live.

Greater understanding will make it possible for man to respond to opportunity as well as to react to need. To gain such understanding is the underlying purpose of the International Biological Program.

This program has three related objectives: human welfare, scientific advance, and international scientific cooperation. These three objectives cannot be separated. Biologists can contribute uniquely to human welfare only by advancing scientific understanding, and the basic premise of the International Biological Program is that the growth of understanding will be accelerated by international cooperation among the world's biologists.

To be effective the program should focus on problems whose solution most depends on international cooperation in biological research. Accordingly we are concerned with conserving and expanding the world's biological resources to better serve human needs, with the quality of the natural environment as a realm of human happiness, with nutrition as a basis for human health, and with the biological aspects of the problem of balancing human fertility and natural resources. Our scientific objectives are also limited. Our basic interest is the development of scientific ecology in its broadest sense. We believe we should give special emphasis to the genetics and dynamics of populations, to the factors that control biological productivity, to the ways in which plants, animals, and especially men, adapt to their environment, and to the changing distribution of living things in the sea and in the air and on the land. If scientific opportunities are to be created for the future, scientists of different countries will need to cooperate in preserving natural areas as well as in research.

Because most of the land surface and many of the most interesting problems lie within the territories of less developed countries, international cooperation in the scientific development of these countries is not only desirable in itself but is also essential to attain the scientific objectives of the IBP. These objectives will be achieved only if there is a free exchange of scientists for research and training among different countries, and a broad and rapid flow of scientific information.

New methods must be developed and old ones greatly extended. In the past, ecologists have studied particular limited communities; now, work on a few large-scale systems is needed to test and extend our understanding. New techniques for worldwide biological surveys (perhaps by satellites as well as in other ways) are needed to improve our description of the biosphere. Greater comparability of methods of measurement and adequate arrangements for data handling are needed for a better appraisal of its productivity.

Field research in appropriate environments by scientists of different biological disciplines will be an essential aspect of the program. This will be most effective if different kinds of research can be undertaken in the same areas; thus a few carefully selected and well-described international research sites would prove highly beneficial. Only a relatively small number of biologists are skilled in the use of modern methods of field research or in studying the interactions among organisms in large ecological systems. The training of young scientists, particularly those from less developed countries, must be a basic component of the entire program.

The IBP had its origin in the International Council of Scientific Unions. Its theme is "the biological basis of productivity and human welfare." Biologists nationally and internationally for over two years have been planning the research phase of this program. The research is to start in July of this year and the program is to continue for 5 years. The National Committee is supported by nine subcommittees whose titles and purposes are briefly stated in the enclosure to this statement.

From the two-year planning effort now drawing to a close the U.S. program is just starting to emerge. As a first step research now on-going in Government and under private sponsorship that should be allied with the IBP is being identified. More importantly new projects and programs are being proposed, and if pursued will provide the support to the IBP that is needed. In February of this year a report entitled "U.S. Participation in the International Biological Program" was distributed to approximately 50,000 biologists of this country. Resulting from both this report and from planning discussions previously held, biologists have submitted over 60 research proposals to granting agencies recommending in each case that the project be included in the IBP. The estimated costs per proposal range from \$5,000/year to \$450,000/year for a duration of 1 to 7 years. The majority of these proposals, if funded by granting agencies, will be included in the program. These proposals total approximately \$7.5 million.

Four significant IBP programs are now being actively organized. Each will be made up of related projects and will require coordinated efforts. These are:

Atmospheric Dispersal of Biologically Significant Material

A program requiring extensive U.S. effort and involving international participation in Europe, Australia, Japan and elsewhere to advance knowledge on the atmospheric dispersal of spores, pollen, microbes, insects and particles and gases that biologically affect organisms. This program has been endorsed internationally and nationally. It is justified since large segments of the population are pathogenically sensitive to pollen, fungus spores, algae and other airborne particles; since plant pathogens atmospherically dispersed annually cause crop losses in the U.S. worth several billions of dollars; since insects, scattered widely by wind currents, serve as vectors of viruses and other pathogens that contribute to human misery and loss of life; and since gene flow does occur by this means and new subspecies of pests and hybrid plants are thus brought about. Scientists of many disciplines nationally and internationally are interested in this program and have contributed to its present coordinated development. Benefits of new knowledge to be acquired will impinge on all pollution programs and will provide better bases for studies by plant and animal pathologists toward understanding aerial spread of diseases. Broadly the program is estimated at \$2 million for the first year and will increase at approximately 25% per year leveling off at approximately \$5 to \$6 million per year as new investigators are trained in the scientific disciplines that will be involved. A description of this program is attached for the record.

Investigation of Endemic and Invading Biotas of the Hawaiian Archipelago

This program has kindled the constructive thought of scientists world wide. The Hawaiian group of islands, from a biological point of view, has been geographically separated from other world influences for centuries. That biological isolation in the last 100 years has been dynamically and massively penetrated. In a few more years the opportunities for study of the accelerating changes now occurring will diminish and many opportunities will be permanently lost. In March of this year a group of biologists associated with the IBP met with scientist of both the Bernice P. Bishop Museum and the University of Hawaii to assess the value of a program in this area, to select the plant and animal groups most urgently in need of study, and to interest the State and military officials in the urgency of undertaking this research now. The report of this meeting has been given to the Special Committee for the IBP of the International Council of Scientific Unions and has been strongly endorsed by that group. This report is also attached for the record. The estimate for this program is \$2 million over a five-year period.

Drainage Basins and Landscapes

This program seeks as its objective better understanding of the interactions in complete ecosystems so that evaluations may be made of man's dual role as a manipulator of, and a functional component in these systems.

Man is using his dominance of the earth to produce the most far reaching, sudden and drastic upset of natural conditions the world has ever seen. During his time on earth, man has changed nearly all the world's biotic communities in one way or another. He has altered beyond recognition much of the land surface in temperate regions and in the drier part of the tropics. His modification of his environment is continuing at an accelerating rate.

Man has shown that he can guide the evolution of communities of organisms in ways which satisfy his esthetic sense and at the same time provide him with a stable source of natural resources, or alternatively he can destroy that which is irreplaceable.

Reconstruction of biological resources, the development on a world-wide scale of biotic communities which will have a maximum stability and usefulness to mankind is a task that has been barely begun. It will require for its successful completion all the knowledge of living organisms we can acquire.

The role of biologists is clear. We must supply the basic information needed by the architects and planners of these new and more productive, man-directed communities of organisms. Ecologists, conservationists, foresters, agriculturists, and other specialists will require a great quantity of facts, now unknown, both about the individual species that can form these communities and their actual or potential relationships to each other.

The task is to explore the diversity of life. We must learn about the population structure of individual species, the ways in which species interact with each other in communities, and how both species and communities evolve. Such knowledge is essential to make the world a better home for mankind.

Within this program are planned intensive studies of large areas such as landscapes and drainage basins. It is estimated that the cost over 5 years for the development and operation of one such area for research purposes is about \$7.5 million. The program has a goal of six sites in different biomes for a total of \$45 million. Manpower availability may restrict operations to only 2 sites at a time for a total of \$15 million over a 5-year period.

Like the previous two programs this program is endorsed nationally and internationally. A description of the analysis portion only of this program is attached for the record. Dr. Smith of the University of Michigan, the program director, is present and will answer any questions in his area. He has a prepared statement which he will leave for the record.

Program on Peoples of the North Circumpolar Area

This program is based on the broad need for increased understanding of man's adaptive processes.

The evolutionary success of the human species is based on genetic, morphologic, physiologic, and behavioral variation. The processes and mechanisms of human adaptability are of special concern. The term "adaptability" is here used in several senses, including the adaptability of populations and individuals and genetic and phenotypic adaptability.

Human adaptability has multiple bases, with consequences for human survival. However, neither the distribution nor the sources of man's variability have been rigorously measured. The International Biological Program presents a unique opportunity for determining the relative importance of these aspects of variability, and of establishing underlying mechanisms on the basis of closely comparable studies of different groups—groups having contrasting genetic, social, nutritional, and climatic backgrounds.

The range of human populations available for this type of investigation should provide the IBP with possibilities for thoroughly assessing sources of variability. At one extreme are few surviving examples of hunting-gathering and incipient agricultural populations, which represent what were the norms of adaptation until relatively recent times. At the other extreme are diverse industrialized populations.

This study involving scientists of the U.S., Canada, Finland, Norway, Sweden, Denmark and of other countries has not yet been as clearly delineated as the previously described programs. Its goals in general are clearly understood, and like the other programs this also is nationally and internationally endorsed. It proposes to study on a multidisciplinary basis the following characteristics and effects of the peoples of the north polar area:

- genealogy and demography;
- dermatoglyphics;
- anthropometry, growth, and development;
- dentition;
- blood groups and other genetic markers;
- serum epidemiology;
- physiology;
- nutrition;
- ecology (general and nutritional); and
- behavior and psychology.

The U.S. and Canadian efforts initially will concentrate on three Eskimo populations:

- Alaskan Eskimo populations centered at Wainwright.

- A central or Eastern Eskimo group centered about Igloodik, Pangnirtung (Cumberland Sound) or perhaps the Copper Eskimos about Cambridge Bay.

- Greenland Eskimos including the Polar Eskimo group and extending south along the Greenland coast.

The programs and projects so far described total between \$30 and \$60 million for a five-year program. This by no means represents the total program. Within the next 12 months I think that the new work proposed will total between \$50 and \$75 million. This is a modest amount for achievement of the goals established.

Other specific programs within the U.S./IBP are taking shape. At this time it is too early to describe them or to estimate their costs. They will be related to productivity on the land, in the freshwaters and in the sea; to the biological processes that influence productivity; to ways of improving the use and management of our biological resources; to greater understanding of environmental

physiology of plants and animals; and to improved taxonomic understanding so vital in modern biological research.

In November 1965 Federal interest in the International Biological Program was formally expressed in a letter from Dr. Donald Hornig, Director of the Office of Science and Technology, to Dr. Leland Haworth, Director of the National Science Foundation. The Foundation was asked to coordinate activity in this area. In response, an Interagency Coordinating Committee was formed with Dr. Harve Carlson of the Foundation as chairman, and with membership from the Atomic Energy Commission, the Departments of Agriculture, Commerce, Defense, Health, Education and Welfare, Interior and State, the National Aeronautics and Space Administration, the Smithsonian Institution and from their major subordinate offices, agencies, institutes, and departments.

Through this Interagency Coordinating Committee the Government agencies have supported and participated over the last two years in the U.S. planning for this program.

At the last meeting of the National Committee for the IBP funding of the research phase of this program was extensively discussed. Discussion was primarily concerned with the desirability of an IBP line item in the President's Budget and brought out the following points:

1. In the opinion of a number of members of the U.S. National Committee the lack of a line item inhibits the generation of IBP research proposals. Competent principal investigators view the IBP as a broad program with an unestablished fiscal base. Therefore, they are inclined to believe that individual IBP proposals, large or small, compete against other proposals supporting programs that are fiscally established, and that only those IBP proposals allied to other recognized program goals compete equally during granting agencies' reviews. Principal investigators and their institutions are also concerned over the possibility that IBP proposals could reduce the level of effort allotted to established programs.

2. Some granting agencies have tentatively earmarked funds for IBP research, and limited program support is thus available. That support is not formally and programmatically recognized at this time. Supportable, competitively acceptable IBP proposals are needed by granting agencies before they may establish recognized programs.

3. If multiple IBP budget line items one in each Government Agency's budget having responsibilities for IBP program development and execution were established the sense of agency participation initiated with the efforts of the ICC would be increased and evidence of a fiscally established total program would be available and on view within all granting agencies.

COMPONENTS OF THE U.S. PORTION OF THE INTERNATIONAL BIOLOGICAL PROGRAM

Productivity of Terrestrial Communities (PT) in collaboration with Productivity of Freshwater Communities (PF)

This subcommittee will concentrate on ecosystem function, including: (1) productivity and its utilization, with emphasis on primary and secondary energy flow within the framework of the concept that man is both manipulator of and a functional component of his environment; (2) circulation of nutrients, with emphasis on vital processes of decomposition and the nutritive base of production; (3) pollution and other disturbances that affect production in and composition of ecosystems; (4) search for means of integrating data on components within a systems-analysis or other statistical-model framework.

Production Processes (PP)

This subcommittee is encouraging studies on recognized specific problems, including: (1) physiology of plants under water stress; (2) frost resistance and the growth limitations imposed by upper or lower temperature thresholds; (3) restricted rooting volume due to low base status, acidity, and aluminum toxicity of the soil, especially in tropical areas, and physiological studies of root growth and root rhizosphere interaction; (4) problems of nitrogen fixation, including (a) assessment of regional bases (types of soils and climates) of the magnitude of nitrogen fixation; (b) factors influencing rate and magnitude of fixation; (c) potential fixation in inadequately studied areas such as the Tropics, the Arctic, and the hydrosphere; (d) distribution of organisms and symbiotic systems that bring about fixation; (e) nitrogen budget and cycle as related to primary production; (f) systematics of nitrogen-fixing organisms; (g) the bio-

chemical mechanisms of fixation and physiological processes associated with the reaction. The subcommittee believes that a study of these specific problems will ultimately lead to a more general treatment of broader problems.

Conservation of Ecosystems (CE)

This subcommittee is concerned with freshwater and marine conservation as well as terrestrial, and will help to provide the scientific basis for the preservation of natural and research areas. The program includes: (1) establishment of criteria by which ecosystems can be described and classified; (2) registry of areas now available for research; (3) selection of types of areas still needed to complete a system of research reserves representative of American ecosystems; (4) identification of types of ecosystems and species threatened by destruction; (5) ecological surveys of American sites for concentrated multidisciplinary studies; (6) provision of scientific data in support of the preservation of natural research areas.

Productivity of Freshwater Communities (PF)

In collaboration with Productivity of Terrestrial Communities (PT), efforts will be concentrated on a selected stream and a selected lake and their drainage basins. Areas of research will include: (1) detailed analysis of aquatic and terrestrial communities; (2) soil and geological descriptions; (3) transfer of material from air, soil, and bedrock to water and the reverse; (4) nutrient cycling; (5) role of organic substances, as in eutrophication; (6) population dynamics at all trophic levels; (7) role of bacteria in trophic dynamics and decomposition; (8) energetics of the ecosystems; (9) migration of organisms; (10) computer analysis and synthesis of biotic and other variables. Other general objectives include studies of eutrophication, primary and secondary production, fish production, the chemical environment, production in man-made lakes, and production under extreme environmental conditions.

Productivity of Marine Communities (PM)

Because studies of the open ocean are already included in other international programs and because man's influence is most marked on estuaries and inshore waters, this program will emphasize the latter. The approach will include: (1) ecosystem analysis as a central problem involving hydrology, solar radiation, nutrients, dissolved and particulate organic matter, phytoplankton pigments, rate of photosynthesis, and the abundance and biogeography of benthos, phytoplankton, zooplankton, and fishes; (2) human food resources; (3) natural and artificial modifications of the environment; (4) distribution and abundance of organisms; (5) development of better understanding of basic ecological mechanisms.

Human Adaptability (HA)

The goals of this subcommittee are to measure the distribution and identify the sources of variability of man's adaptive capacity and to elucidate the processes of adaption. In fulfilling these goals, the subcommittee will undertake studies on a variety of human populations ranging from hunting and gathering groups to industrialized societies which exhibit significant contrasts in genetic background, habitat, and culture. Special attention will be given to the biological adaptation of isolated and migrant groups. The emphasis will be on population dynamics, human genetics, adaptation to stress, and morphology, growth, and aging. In studying adaptations, the subcommittee is especially concerned with (a) fecundity and fertility, (b) adaptation to undernutrition, and (c) adaptation to disease.

Use and Management of Biological Resources (UM)

This subcommittee will emphasize: (1) development of plant gene pools of important groups—especially tropical groups, including rubber, pulses, cocoa, and coffee; (2) use of blood lines as means of identifying and locating potentially useful genetic material for different animal species; (3) biological control; (4) nutrition, with emphasis on protein supply, fatty acids, nutrient-intake levels, establishment of norms, food resources correlated with human needs, means of food preservation, and the incidence of human disease and parasitism as related to nutrition; (5) relation of total biological resources to human nutrition for various countries; (6) growth and ecology of cereals; (7) little exploited sources of protein, both animal and plant; (8) development of microbiological processes for food production.

Systematics and Biogeography (SB)

This subcommittee has adopted the following projects and encourages suggestions for additional ones: (1) participation in the ecological surveys of other sections of IBP, especially PT, PF, PM, and HA; (2) salvage operations in natural areas where species and communities are threatened with extinction; (3) comprehensive descriptions of marine ecosystems, particularly where changes may be imminent, as on both sides of the Isthmus of Panama; (4) studies of endemicty, extinction, and inventory of local biota.

Environmental Physiology (EP)

This subcommittee has construed its terms of reference broadly as (1) physiology of population dynamics with emphasis on (a) understanding of the dynamics of both cosmopolitan and limited populations, (b) factors determining population equilibria and geographic differentiation, including genotypic and environmentally induced variation in key species of the tropics, estuaries, oceans, deserts, and freshwater; (2) physiological mechanisms of adaptation at individual levels and as bases for community structure, including (a) chemical defense methods and (b) response and survival in extreme environments; (3) description of the biosphere and changes occurring therein because of the impact of the expanding human population and industrialization.

(Additional material submitted by Dr. Revelle will be found in Appendix A.)

Dr. REVELLE. We have here in addition to Dr. Bennett, Deputy Director of the Office of Science and Technology, also Dr. Theodore Byerly, who is Vice Chairman of the National Committee for the International Biological Program and Administrator of the Cooperative State Research Service in the Department of Agriculture; Dr. Harve J. Carlson, Director of the Division of Biological and Medical Sciences of the National Science Foundation and Chairman of the Interagency Coordinating Committee for the IBP; and Dr. Frederick Smith, professor at the University of Michigan in the School of Natural Resources, who is director of one of our major programs.

Mr. DADDARIO. The committee welcomes Dr. Byerly, Dr. Carlson, and Dr. Smith. And if you would like to have them all come forward, they can sit around the table, it may be helpful, and they may, if they would like, participate in any way they can or would like to.

Dr. REVELLE. Mr. Chairman, the International Biological Program has been slowly developing now for 4 or 5 years. It was conceived internationally by the International Union of Biological Sciences, and accepted as a major effort by the world's scientists through the International Council of Scientific Unions, which has set up a committee of scientists representing different parts of the earth and different kinds of biology, and called it the Special Committee for the International Biological Program.

This is a very similar organization to the one established for the International Geophysical Year, and similarly, within the United States there is a national committee appointed by Frederick Seitz, the president of the National Academy of Sciences, which is called the U.S. National Committee for the International Biological Program. This committee has selected nearly a hundred scientists to be members of its special subcommittees dealing with different aspects of the program, and they in turn have consulted with and worked with some 150 or so other biologists so that at the present time we have a rapidly spreading network of concern and discussion and ideas among American biologists, thinking about what can be done in the International Biological Program.

You notice I do not call it an International Biological Year, unlike the International Geophysical Year. There are several reasons for this, the principal one being that we are slower than the geophysicists. We expect to spend at least 5 years on this international research program rather than 18 months as the geophysicists did, simply because the problems we are concerned with are so much more complicated, so much more difficult, and so much less understood.

The geophysicists were generally engaged in a straightforward business of making various kinds of measurements. In our case we have to find out what kind of measurements to make. We have to develop similarly, within the United States there is a national committee appointed by President Seitz, the President of the National Academy of Sciences, which is called the U.S. National Committee for the International Biological Program. This committee has selected some hundred scientists, or nearly a hundred scientists to be members of its special subcommittees dealing with different aspects of the program, and they in turn have consulted with and work with some 150 or so other biologists so that at the present time we have a rapidly spreading network of concern and discussion and ideas among American biologists, thinking about what can be done in this International Biological Program.

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The second reason is the geophysicists were generally engaged in a straightforward business of making various kinds of measurements. In this case we have to find out what kind of measurements to make in many cases. We have to develop new methods and new techniques, and we have to train people because the kinds of people, the kinds of scientists who are needed for this program just do not exist in sufficient numbers.

What kind of a program are we talking about? We are talking about those kinds of biology that need to be studied internationally; and we put another limitation on it also: we are concerned primarily with biological problems that can contribute in the near future to human welfare and, in the long run, perhaps to human survival. So we are concerned about such things as food supplies, about the balance between different kinds of organisms in nature, and about the whole structure of the natural environment, what biologists call ecology.

This is essentially a program of field studies; not what can be done in the laboratory with pieces of organisms, with cell or organs, or even by experimentation with whole organisms, but rather the relationships among animals and plants as they exist in the field.

One basic concern motivated the scientists who initiated the International Biological Program. This was that man, more or less in spite of himself, is becoming a geological and biological agent, who can, because of his powerful technology, not only change the world but destroy large parts of it, sometimes without even realizing he is doing

so. This is particularly true for living creatures, because they are so easily affected by human activities, and because of the very tenuous, precarious nature of the web of life that exists on the surface of our planet.

In the IGY we were dealing with the whole mass of the earth, several billion trillion tons of matter. In the case of the International Biological Program we are dealing with a thin web of living creatures on the surface of the earth, the total weight of which is probably less than a hundred billion tons, about the same as the weight of buildings on Manhattan Island. One of our other concerns is that human beings have altered part of their own biology by lowering their death rates. This is a radical alteration, and it requires, most biologists believe, two actions: One, a further modification of human biology by lowering birth rates; and two, a very great increase in the production of food and other biological materials to take care of the greatly increased population that we are bound to have before the lowering of birth rates can take effect.

We are also concerned with the problem of the quality of the environment, how men and women and children can live in the natural world—not to conquer the natural world but to live in harmony with it; how can people be happiest in nature?

So our practical objectives are: to conserve and expand the world's biological resources to serve human needs; to maintain and improve the quality of the natural environment as a realm of human happiness; to improve human nutrition as a basis for human health; and to gain greater understanding of the questions of balancing human fertility and natural resources.

The kinds of science we are going to be especially concerned with are the genetics and dynamics of populations, the factors that control biological productivity, the ways in which plants, animals, and especially men, adapt to their environment, and the changing distribution of living things in the sea and the air and on the land. This in general is a particular kind of biological science, ecology; that is the relationship of organisms to each other and to their environment. And as I said, we want to emphasize those projects and programs which can benefit from international cooperation and a free exchange of scientists, research results, and students among different countries.

We are also interested in using some of the new technologies that have been developed such as, for example, the use of satellites as a means of surveying the earth and mapping the distribution of different kinds of plants, different kinds of plant diseases, and so forth.

MR. ROUSH. May I ask a question, Mr. Chairman?

MR. DADDARIO. Yes, of course, Mr. Roush.

MR. ROUSH. Doesn't the use of satellites almost necessarily have to be preceded by the use of just ordinary aircraft? I believe they are using sensory devices to survey the plant life of various areas of the country. I know that my own State university, Purdue University, is involved in a contract with McDonnell Aircraft Co. to do just this thing.

DR. REVELLE. Certainly, in the development of the technology, this is quite correct. The technique for using this multispectrum sensing has to be developed by a combination of ground observations and carefully controlled air observations.

Mr. ROUSH. Is this being done throughout the world?

Dr. REVELLE. No, it is mostly being done in this country at the present time because of our interest in developing satellite technology. But obviously, if that technology can be developed, it can be used on a worldwide basis.

Mr. ROUSH. Thank you, Mr. Chairman.

Mr. DADDARIO. Since Mr. Roush has opened up the opportunity for discussion here, I would just like to refer to the importance which you have pointed out, of the development of ecology and the part it plays in this.

This committee has been aware of the importance of this field of science and that more ought to be done about it than there is.

You will recall that last year we completed a set of hearings which resulted in a report on environmental pollution which we entitled "A Challenge to Science and Technology." I think it is of interest at this point that I just touch on a sentence or two from that report and I quote:

Ecology generates a viewpoint or an attitude which, simply stated, involves wise use of our environment for the benefit of man. It does not imply a balance of nature or avoidance of change in the landscape. Rather, ecology encourages the manipulation of nature, but with knowledge of the interacting forces and immutable laws, not haphazardly or indiscriminately.

We look with great favor, Dr. Revelle, on what you have been trying to do, not just in the support of this resolution, but in your work which is recognized internationally. By recognizing the side effects of a booming technology which can have disastrous effects on man, we recognize that there is a danger that man and the animals cannot adjust themselves rapidly enough to these tremendous changes. We have got to recognize the importance of what you say and do something about it.

By your support of this resolution, and by the work that you will do during this 5 years of activity—which I understand only to be impetus to other things of the future—you are helping to bring attention to the need of actually bringing together the people and the development of the field so that this challenge can be met.

Dr. REVELLE. That is exactly the problem. We are very much aware of these changes—many of them undesirable or unpleasant—that men are more or less inadvertently bringing about in the environment. But the problem is we do not know enough about ecology in most cases to know what to do about them or even to be able to establish the facts.

Ecology has been a science which inevitably tended to lag behind the laboratory biological sciences because it was necessary to get basic information at the molecular level and the cellular level and the organ level before it was possible to understand the whole organisms and their relationships to each other.

I think perhaps the best way to describe what we are proposing is to talk about a few of our major programs. These emphasize the necessity and the desirability of different kinds of coordinated work as the international biological program envisages it; and one example that I am particularly excited about is our program to study the animals and plants of Hawaii.

Islands have fascinated biologists for a hundred years. The reason is that on islands there are to begin with only a small number of

species that get there by various kinds of accidents, and there is no relationship at all between the species that get to the island and the species that can flourish after they get there.

The problem of getting there is a very special kind of problem of having seeds that are waterproof or that can be carried by birds, or being an animal that can hang on to a floating tree trunk or something like that.

And in the case of the Hawaiian Islands, just to illustrate how this has worked, there are about a thousand species of fruit flies in the world; half of them are in the Hawaiian Islands. And the fruit flies on Hawaii, which perhaps were just a few individuals to begin with, have evolved in literally hundreds of different directions.

One of the fruit flies on Hawaii is as big as a horse fly and fills the ecological niche of a horse fly. Another is a tiny insect that lives in the egg cases of spiders. What happened is simply that a few fruit flies got there and their descendants evolved to fill a great many ecological niches.

Another example is the tar weed which in California, where it comes from, is a weed you would hardly notice, an inconspicuous plant that lives in the fields. In Hawaii it has evolved in many directions. One species is a tree; another is a plant that looks like a yucca; another species is marvelously covered with flowers; another is a creeper that spreads over the ground. It has filled literally hundreds of different ecological niches.

These characteristics of islands were recognized by Darwin long ago when he wrote about the different species of birds on the Galapagos Islands.

But the tragic and disturbing thing is that these species that evolved on islands have few defenses against other competing organisms, so that the plants and animals that have been introduced by man in Hawaii just run wild over the species that lived there before man came. And the question we will try to answer is: How do the introduced species interact with these very peculiar and very vulnerable native species? This is the kind of thing that can be studied in different islands throughout the world; it is a marvelous example of how biologists throughout the world can work together to get a new understanding of the interactions among species.

Another program that is quite different in character is our plan to investigate the adaptability of human beings to their environment by studying the isolated groups of human beings who live around the North Pole; the Chukchis in Siberia; the Eskimos in Alaska, Canada, and Greenland; and the Lapps and their relatives in Scandinavia. One of the most interesting of these groups are the Polar Eskimos who live on the northwest coast of Greenland, about 250 miles north of Ultima Thule. They were discovered in 1818. At that time there were 250 of them, and they thought they were the only people on earth. They believed that somehow this was it, 250 human beings were all the human beings there were, living in an awful desert of ice and snow. There are still 250 of them now, 150 years later. They have an extremely high infant and child mortality. The kids mostly are killed by dogs; perhaps four out of five of the children die before they reach the age of puberty. So in this society, unlike most human

societies, every woman has every child she possibly can, and there is a strong evolutionary pressure, a selective pressure on the population.

The men make a living mostly by hunting polar bears and seals. The trouble is the polar bears hunt the men as much as they hunt the polar bears, and the hunters who aren't awfully good at the job get killed and eaten by the bears. So the men who survive have four or five wives. And this is a constant pressure toward improving the breed, which—

Mr. DADDARIO. It improves the polar bears too, then.

Dr. REVELLE. They improve the polar bears too, that's right. [Laughter.]

Undoubtedly it does. This competition between the men and the polar bears probably from an evolutionary point of view is good for both of them.

But the other kind of genetic pressure among the polar Eskimos, of course, is in breeding. There are no fear of them that any bad trait would tend to be strengthened by in breeding, if it was not wiped out by this very, very severe pressure on survival.

What we find in general, among the different human groups who live around the Arctic, is that somebody has studied the teeth of one group of Eskimos, while somebody else has studied the blood types of another group, or the kinship relationships of a third. But nobody has ever taken a single group of these people and tried to find out all there was to know about them in terms of their genetics, in terms of how they have adapted to their environment, in terms of their food and their ideas and the interactions between their ideas and their physiology. And that is what we hope to do in co-operation with the Russians, the Canadians, and the Scandinavians.

Mr. DADDARIO. Dr. Revelle, what happens to these people when they are brought into a different kind of a civilization? Are they able to adopt?

Dr. REVELLE. The Eskimos do pretty well. They are very tough and very bright.

Mr. DADDARIO. I am talking about this isolated 250.

Dr. REVELLE. I don't think they have ever been taken away from where they live. But the Alaskan Eskimos have, and they turned out to be wonderfully skillful in working with machinery. They can take apart or put together a tractor or a bulldozer from its parts. They can keep these machines going long after the white man has given up. They are awfully good with their hands and mechanical things. So they are able to compete pretty well with the white man, at least in the polar regions and the subpolar regions.

Another one of our major projects is of quite a different kind. This is a study of airborne biological materials, all the way from insects, to spores of fungi, and wheat rust, to pollens that give people hay fever and other allergies. Here it is quite clear that one needs to have a worldwide cooperative program. With such things as wheat rust that starts in one place, the spores or the genetic material get carried up into the air and transported long distances. When they fall down again they infect wheat somewhere else.

Similarly with insects; they get carried by the air currents very great distances and in many cases the diseases they carry, human

diseases in which the insects are the vectors, are carried with them.

We are working closely with the meteorologists to develop a co-operative program between biologists and meteorologists to study these problems of dispersal and transportation and the viability of different kinds of airborne organisms.

This particularly appeals to me because it is something like oceanography. As you may know, the oceanographers are interested in a group of organisms called plankton that float in the ocean and are moved around by the ocean currents. This is a project to study atmospheric plankton, which both the meteorologists and the biologists up to now have pretty much neglected.

Still another major program is the one that Dr. Smith is the director of, and this bears directly on the problems you were talking about, Mr. Chairman, a few minutes ago: The problems of pollution of our lakes, our rivers, and our streams, and how they are affected by what happens on the land.

Dr. Smith is interested in using the most modern techniques of systems analysis and computerism to study the interactions between the plants and animals on the land and the plants and animals in the water, how they affect the environment and how the environment affects them, particularly what ecologists call the flow of energy through the system, which is quite different than it is in a cultivated field, for example.

These are four quite diverse programs, the Hawaiian project to study the interaction between the peculiar island species and the new species, the airborne biological materials program, the study of the polar peoples, and the study of drainage basins and ecosystems. And we are trying to develop other similar programs: One that has just come up deals with the times of the year at which different biological events occur in different places, what is called phenology.

Now, we might talk a little about organization, what we hope to do, and how much it is going to cost.

As I stated, there is a national committee under the aegis of the National Academy of Sciences and the National Academy of Engineering. It has a counterpart in the Federal Government: The Inter-agency Coordinating Committee for the International Biological Program. Dr. Carlson here is the chairman of that committee, representing the National Science Foundation, and it has representatives from AEC, Agriculture, HEW, Defense, Interior, Smithsonian Institution, NASA, and other Federal agencies.

One of our problems is financial support. This goes beyond the resolution, but if I may, I would like to take advantage of this forum to say something about it: At the present time all projects under the IBP go to the regular granting agencies like the National Science Foundation or the National Institutes of Health, simply as projects. We do not say whether they are good or bad. We simply say that we think they are relevant to the IBP, they will benefit by international cooperation, and they fit into our program. But we do not try to say whether they are good or bad projects. It is the prerogative and the duty of these granting agencies themselves to evaluate them from the quality point of view.

But the problem is that these granting agencies do not have any pot of money which is specifically assigned to the support of projects for the International Biological Program. They simply have to be fitted in with all the research proposals from all different kinds of biology. And this has two disadvantages. One is that the molecular biologists, the biochemists, and the laboratory physiologists are liable to say, quite understandably, that any support given to the International Biological Program is likely to take support away from them. The other disadvantage is that it is very hard to develop a coherent and meaningful program unless we have an assured source of funds.

So I would like, myself, to see as soon as possible in the budgets of the NSF and the NIH and perhaps also in Agriculture, Interior, and AEC, a line item which is specifically directed toward the International Biological Program.

Mr. DADDARIO. What have those agencies said to you about that possibility?

Dr. REVELLE. Well, up to the present time they have been sort of ambivalent about it. I think Dr. Carlson in the privacy of his office might say it would be a good idea, but God knows what he will say in a public hearing.

Mr. DADDARIO. He might say just the opposite.

[Laughter.]

Dr. REVELLE. That is right.

Well, I think that many people—

Mr. DADDARIO. I won't ask him that question.

Dr. REVELLE. Many people in the agencies would think it is a good idea and others would think that perhaps the present system is the best.

But my own opinion is that we should aim for a line item in the budget.

Mr. DADDARIO. Do you have any feeling about how it would work out if you are successful in your line item ambition which you stress in your paper? We always hear the argument that new things take away from on-going ones. I wonder if that is always true. I do not at the moment feel that that was an overwhelming or compelling barrier here.

Dr. REVELLE. No, I don't think it is. In fact, I think that all our experience to date shows that when we have a really important and justifiable new program or new focus like ecology, which, as I said, has had to lag behind the other sciences and now is ripe for pushing—

Mr. DADDARIO. What would happen—

Dr. REVELLE. This simply stimulates everything else. In fact, it will help the other kinds of biology rather than detract from them.

Mr. DADDARIO. I am looking at it from the standpoint of the best practical way to approach this, since you raise the question of the lack of line item inhibiting the IBP research proposals.

If they are line items, if the agency heads of all of these Government organizations are anxious to support them, if you do succeed in your line item desires, and if the Congress is in a cutting mood, because these are new proposals these proposals would stick out like a sore thumb and could be eliminated and you would not have any

money at all, and the agencies would be under the injunction not to support you in any case even if they wanted to.

Dr. REVELLE. That is exactly the risk you run, and that is why the agencies have had this ambivalent feeling about it for just the reason you mentioned.

Mr. DADDARIO. I think there is a danger in this, Dr. Revelle. There is your anxiety to show everybody—and it would be very nice if you could, if you knew it was going to be successful—that you do have support since you can point to each budget request and show how it is being supported by each of these agencies.

However, the danger in doing this is something that ought to be considered among yourselves.

You might be better off just making certain that the agency heads are anxious to support it. They will be getting a little extra, I would expect, in the budget than they got last year, so that they can support the program.

Dr. REVELLE. This I think is a very powerful argument.

The kind of money we are talking about is not negligible. At the present time the programs that I have described come to around somewhere between \$30 and \$50 million.

Mr. DADDARIO. Over what period of time?

Dr. REVELLE. Over the next 5 years.

Mr. DADDARIO. How does it grow? Where do you start and how long does it take you to get to the high point?

Dr. REVELLE. So far we need, essentially, funds for planning purposes and for coordinating purposes.

Mr. DADDARIO. Which come to what?

Dr. REVELLE. These have been of the order of \$400,000 for the last 2 years.

Now we are having to think of an order-of-magnitude increase.

Mr. DADDARIO. Where did you get those funds?

Dr. REVELLE. From the various agencies represented on the Inter-agency Coordinating Committee.

Mr. DADDARIO. I see. This went along with their representation?

Dr. REVELLE. That is right.

Mr. DADDARIO. And then where do you go from there? How does it pick up?

Dr. REVELLE. Well, the next step under the present budgetary system will be individual proposals for the management of these programs that will be sent to the ICC, and they will decide which agency should consider them. And then there will next be direct support of individual research workers or groups of research workers in the standard pattern that has been developed by the NSF and NIH.

Well, they may actually assign proposals for consideration, say, by NIH or by NSF.

Mr. DADDARIO. They try to balance it out somehow.

Dr. REVELLE. That is right. That is my understanding.

Mr. DADDARIO. How does this compare as against those programs, in cost of other countries? What are we talking about altogether? Is there any estimate of that?

Dr. REVELLE. I would think probably that the total cost will be three times the U.S. cost, whatever the U.S. cost is, judging by the

IGY. The way these international programs have worked in my experience is that the United States has to take the lead, kind of give an image to the whole thing, and the other countries then for various reasons come up to just about the same level and may go ahead of us, then we go ahead of them, and then they go ahead of us a little. But in the long run it comes out, at least judging by other programs, to about three times whatever the United States spends.

Mr. DADDARIO. Then you do then give a kick in this way to developing ambition in other areas throughout the world to support programs which they otherwise would not?

Dr. REVELLE. That is right; very much so.

Mr. DADDARIO. Building up some international involvement which cuts across all kinds of political and social lines.

Dr. REVELLE. That is right. That is it exactly.

And the appealing thing about this is it's being the kind of science that needs to be done and can be done in the poor countries, countries like India and Pakistan and those in South America and Africa.

Mr. DADDARIO. Would you explain that a little bit?

Dr. REVELLE. Well, because it does not involve expensive laboratory equipment, and it does not involve very highly trained specialized people. Anybody can play this game because it is essentially a game of going out and using your eyes, and making measurements in the field which are fairly simple and straightforward measurements. Moreover, it is the kind of thing these countries need because they need to understand their own resources, what there is in their environment that they can use for their development.

One very exciting aspect of these possibilities in the underdeveloped countries is that in Africa the native herbivorous animals, the zebras, the various kinds of antelope and deer may be a better source of meat and protein in the African environment than domestic cattle. Their productivity may actually be greater.

Mr. DADDARIO. It wouldn't have to be very good, from what I have seen.

Dr. REVELLE. That is right. Cattle have a hard time there, while these wild animals live in enormous herds and apparently are very well adapted to their environment.

Mr. DADDARIO. I wish anybody else would pitch in at any time if you have anything to say.

Dr. REVELLE. I think we ought to turn to Dr. Bennett now.

STATEMENT OF DR. IVAN L. BENNETT, JR., DEPUTY DIRECTOR, OFFICE OF SCIENCE AND TECHNOLOGY

Dr. BENNETT. I would like to cite a specific example of why this is the type of research that is so important in the underdeveloped countries, and I can cite from my own experience.

In 1956 in the Sagar District, in Mysore State in south India, there appeared what seemed to be a new disease. It made its appearance in a very dense forest in the form of a disease that killed monkeys so that it was first evident because monkeys were found hanging dead from tree limbs. This was in an area known as the Kyasanur Forest.

There was no great difficulty, through the use of modern techniques, in identifying the fact that this was a new virus that had never before appeared in India, and it appeared to be related to a group of viruses known as the Russian spring-summer complex.

At that time, the fear was that this represented yellow fever, which is a human viral disease that attacks monkeys, and India happens to be in a part of the world that, for reasons that are totally unknown, has escaped yellow fever until now, although the *Aedes* mosquitoes that transmit it have been there and there appears to have been ample opportunity for years for yellow fever to come in. At that time, in an etiological investigation, because there was some concern how this disease would spread, it became evident that we did not know what the vector of the disease was—that is, how it was transmitted. We did not know there were animals that formed a reservoir, and there was simply no information about the basic ecology in this particular area.

As an example, we were able to find—when I say “we,” I am speaking, really, of the Rockefeller Laboratory at Poona. They were able to find it killed monkeys in 8 days or less, but there was absolutely no information about how far these monkeys traveled, whether they traveled 100 miles or whether they ranged over 500 miles. There was also very little information about the parasites on these monkeys.

They did locate the virus in some *Ixodes* ticks, which, according to all the books, are ticks that attack rodents. On the other hand, they were unable to find the virus in rodents. And finally the problem was solved when field investigations by the Rockefeller people demonstrated that these ticks were also found on monkeys though there was absolutely no prior information to this effect. So that over a period of about 3 years, not on the basis of laboratory virology, but on the basis of field studies of the type that I have described that had to do with ectoparasites on animals and the whole ecologic web, it was finally possible to work out the transmission of the disease.

In this country it would have been ridiculously simple because we have this type of information about our lower animals and the ectoparasites, but the absence of this information in a country like India, where it could very easily have been obtained had there been an interest in it, led to spread of this disease over an area of several hundred square miles before the transmission could be understood and steps were taken to eliminate the ticks which were the actual carriers of the disease.

I might also add, in conclusion, that no one knows yet how the virus arrived there. It happens to be in one of the flyways that come down from Siberia, and it is thought it may have been brought by a bird. But in that instance the stumbling block to understanding the spread of this disease was simply a lack of knowledge of the fauna and the flora in the area. It just does not exist in these countries.

Mr. DADDARIO. You reemphasize the point Dr. Revelle made earlier that this is very necessary for developing countries which have the capability of developing good programs at small cost and of great effectiveness for controlling problems?

Dr. BENNETT. I would emphasize also that they are able to do it at relatively little cost, and that participation in this program will give it a type of respectability scientifically that is very necessary in these countries.

Mr. DADDARIO. Does your presence here, Dr. Bennett, prove that the administration is in support of this proposal?

Dr. BENNETT. Yes, indeed.

Dr. REVELLE. Do you have a statement, Dr. Bennett?

Dr. BENNETT. I do have a statement that I will be glad to read into the record that I think portrays our views.

Mr. DADDARIO. You can read it. Before you do that, are there any points which have come up during Dr. Revelle's remarks which you would like to emphasize?

Dr. BYERLY. If I may, Mr. Chairman, I would like to emphasize a couple of points that Dr. Revelle has made.

First of all, this is basically an organizational structure of a non-governmental nature through the International Council of Scientific Unions. But as is always also the case, I believe, Dr. Revelle, there is close working relationship with the U.N. agencies, with UNESCO, with FAO, with the World Meteorological Organization to which you alluded, with the World Health Organization, and this facilitates this working together on development in some of the underdeveloped countries which otherwise would be more difficult.

As a nongovernmental organization, we refer, too, to Dr. Revelle's statement with respect to worldwide participation. There are very substantial programs, for example, in Japan, in Australia, in South Africa, in the Soviet Union, and Poland, in Czechoslovakia and a number of other countries, and the real difficulty is finding means for developing the programs and projects in the undeveloped countries; all of the possible cooperation is one of our particular problems.

Dr. REVELLE. Our particular concern is with the South American countries, because we feel that we should have a more intimate relationship with them, and their needs for scientific development are very great.

Mr. DADDARIO. Do they have comparable organizational setups so that they can or are developing for these purposes support both private and governmental?

Dr. BYERLY. This is true, Mr. Chairman. I think we have a list that could be supplied for the record of those countries which have national committees. In Latin America, for example, thus far Brazil, the Argentine and Mexico have such committees, and Chile.

Mr. DADDARIO. Without objection, that may be supplied for the record. I think it would be very helpful.

NATIONAL NEWS

PARTICIPATING COUNTRIES

The following thirty-six countries had, by June 1966, established national committees. The name and address of the Chairman is given in each case.

Australia:

Sir Otto Frankel, CSIRO, P. O. Box 109, Canberra City, A.C.T.

Austria:

Dr. Wilhelm Kühnelt, Zoologisches Institut, Dr. K. Luegerring II, Vienna 1.

Belgium:

M. Marcel Florkin, Rue Naimette, 6, Liege.

Bulgaria:

Professor N. Stojanov, Institute of Botany, Bulgarian Academy of Sciences, 7 November Street 1, Sofia.

- Canada :
 Dr. F. C. MacIntosh, Department of Physiology, McGill University, Montreal, P.Q.
- China (Republic of) :
 Dr. Hsein-wen Li, Institute of Botany, Academia Sinica, Nankang, Taipei, Taiwan.
- Czechoslovakia :
 Academician Ivan Malek, Czechoslovak Academy of Sciences, Vltavska 17, Prague 5.
- East Africa :
 Dr. D. P. S. Wasawo, Head Office, Uniafric House, Koinange Street, P. O. Box 7288, Nairobi.
- Finland :
 Professor Hans Luther, Helsingin Yliopiston, Kasvitieteen Laitos, Unioninkatu 44, Helsinki.
- France :
 Professor Th. Monod, Museum National d'Histoire Naturelle, 57, rue Cuvier, Paris 5e.
- Federal Republic of Germany :
 Professor H. Ellenberg, Untere Karspüle 2, D 3400 Göttingen.
- Democratic Republic of Germany :
 Professor Dr. H. Stubbe, Institut für Kulturpflanzenforschung, Gatersleben.
- Greece :
 Professor G. Athanasiades Novas, Akademia Athinon, Athens.
- Hungary :
 Professor Dr. J. Balogh, Pushkin u. 3, Budapest VIII.
- India :
 Dr. N. K. Panikkar (Acting Chmn), National Institute of Sciences, Bahadur Shah Zafar Marg, New Delhi—1.
- Indonesia :
 Dr. Otto Soemarwoto, Director, National Biological Institute, Bogor.
- Israel :
 Professor J. Magnes, Hebrew University, Hadassah Medical School, Jerusalem.
- Italy :
 Professor C. Barigozzi, Instituto di Genetica, Università di Milano, Milan.
- Japan :
 Dr. Hiroshi Tamiya, The Tokugawa Institute for Biological Research, 41 Mejiro-Machi 4th St., Tokyo.
- Republic of Korea :
 Professor Yung Sun Kang, Department of Zoology, College of Liberal Arts and Science, Seoul National University, Seoul.
- Mexico :
 Dr. Enrique Beltran, Director del Instituto Mexicana de Recursos Naturales Renovables, Recursos Naturales Renovables, A.C., Dr. Verez no. 724, Mexico, D.F.
- Netherlands :
 Professor Dr. J. Lanjouw, Instituut voor Systematische Plantkunde, Lange Nieuwstraat 106, Utrecht.
- New Zealand :
 Professor G. A. Knox, Department of Zoology, University of Canterbury, Canterbury.
- Nigeria :
 Professor Eni Njoku, University of Lagos, Lagos.
- Norway :
 Professor Rolf Vik, Zoologisk museum, Universitetet i Oslo, Blindern, Oslo 3.
- Philippines :
 Dr. Deogracias V. Villadolid, National Research Council, University of the Philippines, Diliman, Quezon City.
- Poland :
 Professor K. Petruszewicz, Ecological Institute, Polish Academy of Science, Nowy Swiat 72, Warsaw 1.
- Rhodesia :
 Professor E. Bursell, Department of Biological Sciences, University College of Rhodesia, Salisbury.

South Africa:

Professor C. A. du Toit, Department of Zoology, University of Stellenbosch, Stellenbosch.

Spain:

Professor Alfredo Carrato, Instituto Cajal, Velasquez 144, Madrid 6.

Sweden:

Professor C.-G. Heden, Bakteriologiska Institutionen, Karolinska Institutet, Stockholm 60.

United Kingdom:

Professor A. R. Clapham, Department of Botany, The University, Sheffield.

United States of America:

Professor Roger Revelle, Harvard School of Public Health, 655, Huntington Avenue, Boston, Massachusetts.

Union of Soviet Socialist Republics:

Professor B. E. Bychowsky, Zoological Institute, University Embankment, Leningrad 164.

Republic of Vietnam:

Mr. Pham-Hoang-Ho, Biological Society of Vietnam, Faculty of Sciences, B.P. A/2-Saigon.

Yugoslavia:

Dr. Branko Miletic, Inst. "Rudjer Boskovic", Zagreb.

The following twenty-two countries, in which national committees have not been established, are in touch with IBP through national correspondents:

Argentina, Brazil, Ceylon, Chile, Colombia, Cuba, Ethiopia, Hungary, Iran, Democratic People's Republic of Korea, Morocco, Pakistan, Peru, Portugal, Romania, Switzerland, Tunisia, U.A.R., Uruguay, Vatican City State, Venezuela, Democratic Republic of Vietnam.

NATIONAL PROGRAMMES

The following programme documents have been received by the Central Office to date:

Australia: "Provisional Australian Programme" (dated and circulated February 1966).

Austria: "Preliminary Framework of the Austrian Program of the IBP" (dated and circulated February 1966).

Bulgaria: "Proposed Researches of the Bulgarian National Committee" (dated April 1966, circulated at II General Assembly).

Canada: "Provisional Canadian Contribution to the International Biological Programme" (circulated March 1966).

Czechoslovakia: "Provisional Czechoslovakia Contribution to Phase I of the International Biological Programme" (Dated and circulated February 1966).

France: "Projet de Contribution Francaise au Programme Biologique International" (dated March 1966, circulated at II General Assembly).

German Democratic Republic: "National IBP Programme of the German Democratic Republic" (circulated at II General Assembly).

Indonesia: "Preliminary Communication on the Indonesian National Biological Programme" (circulated February 1966).

Israel: "Provisional Israeli Programme" (dated March 1966).

Italy: "Proposed Researches of the Italian National Committee for the IBP" (dated February 1966, circulated at II General Assembly).

Japan: "Proposed Researches of the Japanese National Committee for the IBP" (dated December 1964, circulated January 1966).

Republic of Korea: "Research Project of the Korean National Committee for the IBP" (dated February 1966, circulated at II General Assembly).

Netherlands: "Communication to SCIBP from the Netherlands National Committee for the IBP" (dated March 1966, circulated at II General Assembly).

Nigeria: "Biological Research Projects approved by the National Committee for IBP in Nigeria (1965-1969)" (dated August 1965, circulated November 1965).

Philippines: "Programme of the Philippines National Committee" (circulated at II General Assembly).

Poland: "Researches proposed by the Polish National Committee for the IBP" (circulated at II General Assembly).

South Africa: "Proposed South African Programme for Participation in the International Biological Programme" (circulated at II General Assembly).
 Spain: "Draft Programme of the Spanish National Committee of the IBP" (dated April 1966).

Sweden: "The Swedish IBP Programme" (dated April 1966, circulated at II General Assembly).

United Kingdom: "Provisional United Kingdom Contribution of the International Biological Programme" (dated August 1965, circulated September 1965).

United States of America: "Preliminary Framework of the U.S. Program of the IBP" (dated August 1965, circulated November 1965).

Yugoslavia: "Preliminary Information on the National Program of the Yugoslavian Committee for the IBP" (dated April 1966, circulated at II General Assembly).

Brazil: Formed an IBP National Committee 26 October 1966.

Mexico: Has formed an IBP National Committee.

Chile: Has started to form an IBP National Committee.

Argentina: Has started to form an IBP National Committee.

Dr. REVELLE. The Brazilians are beginning now to develop an interest at the level of the National Research Council, which is a governmental agency, and this is very important.

Mr. DADDARIO. Had you finished making your point, sir?

Dr. BYERLY. Yes, sir.

Dr. CARLSON. Mr. Chairman, in reference to Dr. Revelle's comment as to how the proposals will be handled within the agencies, he was right; and I always admire Roger, and he is a physical scientist, but he can handle the biological areas very well too.

Mr. DADDARIO. He is even more flexible than that. I saw him on television Sunday night.

Dr. CARLSON. Oh, really?

The proposals are submitted to the agencies for review, as Dr. Revelle pointed out. They are also submitted to the Academy which distributes them to subcommittees of the IBP where they are reviewed to ascertain whether they fit into the International Biological Program; in other words, do they fit under the umbrella of the IBP.

Within the National Science Foundation—and I am sure this is true with NIH, AEC, NASA—they are put in the same group that are competing for all of the available funds.

Now, at the present time NSF has 33 such proposals. We still have 20 of them pending. We have supported six of these, and six of them did not—seven of them did not compete successfully. So there is a source of funds to support them at the present time. We do not have a line item within the budget structure. It is noted within NSF that the context of the development of the budget, that the International Biological Program should be supported; but there is no specific line item with a number of dollars to the side.

And I think that is all I have to say except one more thing that to me, when we speak of ecosystems this combines the efforts of a large number of individuals, and I know that Dr. Smith can talk to this, and these are very, very expensive programs.

I look at them as a total program rather than a project. And to provide a baseline, the United States as well as many of the other developed nations definitely have to look at ecosystems and look at them fairly soon or man himself is going to destroy our baseline.

The Federal agencies, I should point out, are all involved in this and are participating. For instance, the AEC has their laboratories

and their ecological programs. They in turn I am sure will move in this direction, and the scientists from the academic community will work with them on major problems, looking at total ecosystems.

Mr. DADDARIO. Will you go into just a little bit of depth on this baseline point you raise and man's ability to destroy it and the reason why you believe it is important to give some push to other countries to participate in a program of this kind.

Dr. CARLSON. Mr. Chairman, can I call on the expert to touch on that, and then I will try to go into it.

Mr. DADDARIO. Certainly. Dr. Smith?

Dr. CARLSON. And then I have some talent behind me also.

Mr. DADDARIO. Let's get all the talent we can to participate.

Dr. SMITH. I would like to point out we have been talking about two rather different levels of ecology so far this morning. One concerns physiological ecology, or life history ecology and this is a subject which is well developed in this country and in many others, but is almost totally absent in the undeveloped countries. This is the kind of ecology we can extend around the world at a relatively low cost and rapidly with an adequate organization.

The other level that we have been talking about is the ecosystem, and this is not an aspect of a subject the way ecology is. This is a human study itself. It is a whole new scientific level, like a molecule or an organism.

An ecosystem is a physical entity which can be studied. And I think it is fair to say that in all of the developed countries the functioning of ecosystems has become a matter of national concern in terms of the environment; certainly the semipopular literature of Russia is aware of the problem; they are concerned with water pollution, air pollution, and the destruction of environments. And it has become very evident now that man has become so ubiquitous that he is influencing all natural systems whether he knows it or not. The tragedy is that we do not understand natural systems.

If we put together what we do know or think we know, we produce models that in fact don't operate.

So there is an awful lot that we know we don't know. And these are primarily concerned with the checks and balances in natural systems that allow them to persist. We move into these and the systems are changing. We can see them change, but we don't know how far they are going to go on changing; we don't know what we could do to bring them back where they were if we wanted to, or how to control their change in the future.

It is for these reasons that we need these very intensive analyses of whole ecological systems.

Now, included in these we will have to have natural systems—quote, "less disturbed ones," to find out as best we can how the whole process of evolution solved these problems and then also study some that we could upset pretty badly to see what we have been doing to them.

Mr. DADDARIO. Natural systems less disturbed so you can develop an understanding about the nature of the change because the change is less in certain places than others? Is that the reason?

Dr. SMITH. Well, certainly some regions of the earth are far more disturbed than others. If we study a spectrum of these we can get a better understanding of what man has done.

We are bringing in a system of our own, actually a socioeconomic system, imposing it on top of a preexisting natural system, and somehow we should try to understand how these two get along together.

Dr. REVELLE. These are both ecosystems.

Dr. SMITH. Yes. Well, one of the best things about an ecosystem is you cannot exclude man from it. It is defined as a hunk of material in space and time and man is there, he is part of it. You cannot pretend he is not there.

Dr. BYERLY. Mr. Chairman, I would like to laud Dr. Smith as one of the best systems analysts in ecology.

Perhaps one of the things that has intrigued me is the probability of weather modification during the next few years. He spoke of natural ecosystems, and the time is here or this capability seems about to appear when we need to know the baseline system and to make models which can be tested, perhaps be verified, of what will happen when weather modification comes about. This is one of the big changes that is likely to happen.

Dr. SMITH. That change is practically upon us, I guess, and in this particular case I would hope that we can study the effect of weather modification upon the surrounding ecosystems in the total way, find out what the full consequences of changing the pattern of rainfall are.

As far as the use of models for prediction are concerned, we do not—right now we don't know that much, but models are very useful right now for discovering what we don't know.

As I just said, you put together what you think you know and it produces a model that simply doesn't look like nature at all.

Mr. DADDARIO. And you would hope, however, when you put together those models that you had enough information that it would work in the way that you predicted, but when it does not then you know you are in real serious trouble. Would that be a correct conclusion?

Dr. SMITH. Yes. It gets even worse in some cases. Some of the concepts that we have been trained to have a lot of faith in which operate very nicely if you only look at a single population turn out to be rather disastrous ideas in terms of the whole network of populations. The system has properties that single populations simply do not have.

This is exactly parallel to what has happened in industry, and the whole field of industrial dynamics which has exploded in the last few years is able to cope with this multilevel complex system. And this kind of analysis, the systems approach, seems very suitable to the ecological systems. But it is only the mathematical representation of it; the models are only as good as the information that goes into them. And in order to get anywhere with the models we would have to do an enormous amount of research, field research.

Mr. DADDARIO. This worldwide program helps to put together the information to make it more readily available for the development of such models; you are stepping in the right direction by supporting this program.

Dr. SMITH. Well, particularly since several different countries have proposed to study whole ecosystems. I should point out nobody has ever studied whole ecosystems. It has never been done.

Mr. DADDARIO. You had a point here?

Dr. REVELLE. I wanted to say one of the interesting things to me here is that a good deal of such understanding as we now have has come from areas around atomic energy plants where radioactive materials getting into the biological environment have spread through that environment, and it has been possible to use these as tracers to see how the different kinds of plants and animals are related to each other, who eats what, how much from one level of life gets into the next level of life, and so forth.

And what Dr. Smith is in charge of is an attempt to expand these studies that have been made on quite a small scale to a large scale, to the prototype, the real world, in terms of a whole drainage basin. It is a natural step up from what you might call the bench stage, the breadboard model, to the prototype stage of understanding of ecology.

Mr. DADDARIO. Mr. Lukens?

Mr. LUKENS. Yes, Mr. Chairman. Thank you.

I understand that during my brief absence you did cover briefly some of the cost estimates involved, and I would like to ask you, Dr. Revelle, on page 7 of your statement you have a paragraph next to the bottom that does confuse me. In one sentence you say "The programs and projects so far described total between \$30 and \$60 million for a five-year program." The next sentence says "Within the next 12 months I think that the new work proposed will total between \$50 and \$75 million."

I wonder if you could clarify that for me. It does seem to be on the surface contradictory, new programs and programs "so far described." What is the difference here in the cost estimates, and particularly in the 12-month period, the \$75 million total? I realize this is just an estimate.

Dr. REVELLE. What this means is that the programs that have already been planned and in which projects have been developed come to this first figure of between \$30 and \$60 million, not per year but over a 5-year period.

We are just now right in the middle of developing many new programs and proposals. This document here went out to 50,000 biologists a couple of months ago, and we are getting every day several proposals passing through our office in the National Academy which have been stimulated or elicited by this document and by our various working conferences that we are now right in the middle of having.

Mr. DADDARIO. These figures are not those you will spend but rather they show the nature of the problem because of the reaction there is throughout the world to submit proposals?

Dr. REVELLE. That is right.

Mr. DADDARIO. You would then choose from these the best ones and then the figure will evolve through the choices of these proposals made to you. It would be how much over the 5-year period?

Dr. REVELLE. I would guess now—I can't really tell and I wouldn't like 5 years from now for you to come back at me and hold me to this. But my present guess is the cost over the 5-year period for the American part of the program would be about \$75 to \$100 million.

Mr. LUKENS. The next 5 years? That is about \$15 million a year.

Dr. REVELLE. Yes, sir.

Mr. LUKENS. And I understand along with this our proportionate share of the cost would be about 35 percent of the total?

Dr. REVELLE. Yes, about a third.

Mr. LUKENS. I appreciate the chairman's additional clarification that this is what they would be concerned with but that this isn't all that would be authorized or appropriated.

I have a couple of other questions, if I might. In comparing the information gained from a small and different socioeconomic and social group, such as an Eskimo group, how can you apply that type of information to the major populations of the world? What do we expect to learn from this effort and the money expended in this area of study?

Dr. REVELLE. Well, one of the major—as we all know, one of the major problems about human beings is the question of nature versus nurture. In other words, what is the effect of the environment on human beings and how does this interact with their genetics, with their heredity.

The nice thing about these isolated groups that live around the North Pole is that they each have a gene pool, if you will, a pool of heredity that is different than the others, even though they live in pretty much the same environment. It is different simply because they have been isolated for a long time. So we can find out what their—quite a bit about what is called human genetics, in other words, what is it that is distinctive about them in terms of their genes.

Mr. LUKENS. So basically it would be a genetic or generic study and you would specialize at that stage of the game having in control the environment with a small study group?

Dr. REVELLE. That is right.

Mr. LUKENS. I suppose the same thing would be true of, of course, the study of ancient tribes of Indians along the lines of Incas, Mayas, and Aztecs. I know there are areas in the world where this exists.

Dr. REVELLE. The same is true in the Amazon Basin.

Mr. LUKENS. I'm sorry; this does give me an idea.

My last question is this, and I don't think it digresses. I think it really has to do with human welfare referred to in this resolution.

Basically what kind of commensurate program do we have to match this effort to allow more children to grow up and increase the population? What commensurate effort is there to—in the science field, if you will, on birth control and the problem of having so many children now already being a potential drain on our present food production capability? Is there a commensurate effort of any kind along these lines?

Dr. REVELLE. Yes, there is quite a large effort which I think is going to be well supported both by the Government and by foundations, dealing with reproductive physiology: What actually happens in the process of human reproduction, how does it work. It is an extremely complicated business.

As you may know, there are several kinds of communications systems within the body. One of them that controls the reproductive process is a system of hormones starting with the pituitary hormones, and then the so-called steroid hormones that are produced by the ovaries and by the uterus in women, and the problem is to find out

a great deal more than we now know about how these hormones actually work.

The pills, for example, are simply sort of imitation steroid hormones but nobody really knows what they do at the cellular level. And the question is to find out what they do in order to develop better methods of fertility control.

The other part of the job is primarily a social, anthropological, economic, and administrative problem. In a country like India, for example—what are the conditions which will persuade and help the people in an Indian village to limit the size of their families.

One thing we think is very important here is to reduce infant and child mortality; and this probably depends largely on better nutrition.

Mr. LUKENS. Let me interrupt you. You say one element of the larger families in the lower socio-economic areas is the variation in the average of mortalities: They have five children, in order to have enough assurance that some of them will live. This is really a considered factor.

Dr. REVELLE. That's correct.

Mr. LUKENS. Thank you.

Dr. REVELLE. There are many more aspects to this problem than just this simple and straightforward one—this is the easiest one to talk about, but people want children for many quite human reasons. The question is to get them to want a smaller number of children on the basis that the children will then be happier and better human beings, they won't be ground down by poverty and misery.

Mr. LUKENS. You have been most kind in answering my questions; and I am sure I consumed my time, Mr. Chairman. Thank you.

Mr. DADDARIO. This is a very important subject, which Mr. Lukens brought up. We can point to some very successful areas of the world where they have been able because of level of education, perhaps, more than anything else—Japan and Korea are doing remarkable jobs because they recognized the problem and have been able to put out programs where the people have understood it, and in India, where they are doing a massive job, it just doesn't work so well; it will take a longer time to have people understand not only what the hopes and ambitions of such a program are but how to use the devices and the medicines and the pills and what have you in order to accomplish these end objectives.

Dr. REVELLE. And to find methods that are acceptable.

Mr. DADDARIO. Yes; under their systems.

Dr. REVELLE. Useful under their circumstances.

I might just say one more thing which I have neglected and that is that one of the major aspects of the international biological program is conservation in terms of preserving natural areas. One of our subcommittees under the leadership of Dr. John Buckley, head of the Office of Ecology of the Department of Interior, is preparing a catalog of all of the natural areas in the United States which have scientific uniqueness, which have something about them that makes it desirable to preserve them from a scientific point of view, both on Federal lands and on State lands and on university or other nonprofit private lands. And this is something which will be done throughout the world, to try to preserve natural areas for future ecological

studies. We do not hope in this generation that we will get to the level of ecological understanding that is needed. We know this is going to be a long term job. But we need to keep the areas that will be needed for future study.

Mr. DADDARIO. Well, Dr. Bennett, if you could then give us your statement in any way you like, either by summarizing it or by reading it.

Dr. BENNETT. I would prefer to summarize it and simply leave the statement for the record.

Mr. DADDARIO. Fine.

(The prepared statement of Dr. Bennett is as follows:)

PREPARED STATEMENT OF DR. IVAN L. BENNETT, JR., DEPUTY DIRECTOR, OFFICE OF SCIENCE AND TECHNOLOGY, EXECUTIVE OFFICE OF THE PRESIDENT

Mr. Chairman and members of the subcommittee, thank you for this opportunity to discuss United States participation in the International Biological Program.

Professor Revelle, Chairman of the U.S. National Committee for this program, has already described for you its origins, background, organization, plans, and goals, and it would be redundant for me to rehearse these aspects of the contemplated program.

Since the earliest beginnings of planning for the International Biological Program, the Office of Science and Technology has endorsed and urged a major commitment by U.S. scientists to participation and collaboration and our position on this score remains unchanged.

I believe that I can best serve the purposes of the Subcommittee by reviewing succinctly the major reasons which underlie our support for the International Biological Program. In the course of this, I will try to give point and emphasis to certain aspects of the proposed study which offer unusual and important potential for both academic and practical advance, for understanding and for application to pressing problems.

First, this is a program originated by scientists from several countries and, if pursued as planned, it will undoubtedly contribute to improved international understanding. As conceived, the program is free of political pressures, having arisen from spontaneous, felt needs among biological scientists.

As the Honorable Mr. Fulton, ranking minority member of the House Committee on Science and Astronautics put it, in his opening remarks to the Eighth Meeting of the Panel on Science and Technology in January of this year:

"Science is neither political, sectional, ideological, or limited by country boundaries. Science, as with music, is an intellectual language everybody can understand ***."

"Joint constructive international programs of science, research, and development will cause progress that will construct bonds of well-being and independence among peoples far outweighing the dangers, alarms, and destruction of combat and war."

At that same symposium, other speakers emphasized that the international character of science and its ready acceptance as a part of the universe of discourse among nations was intrinsic in the nature of scientific problems and the phenomena of nature which science strives to understand.

The objectivity of nature is not changed by national boundaries and the criteria against which experimental observations and findings, and scientific principles are judged are not different in different nations.

In short, the transnational character of science is *intrinsic* and is not merely a result of collaborative international agreements or the actions of governments. Hence, science can serve as a neutral portal to international understanding, quite apart from its specific disciplinary and intellectual content.

In view of the fact that this subcommittee is so clearly on record as having an appreciation and understanding for this potential benefit of science on the international scene, I shall not labor this aspect of the International Biological Program further.

Second, the planning and description of broad and general objectives both by distinguished U.S. and overseas scientists have been underway during the past

two years of Phase I. These plans centered upon an array of problems which are of utmost importance from both scientific and human viewpoints, in light of the nature and magnitude of the numerous urgent problems facing the U.S., other industry sophisticated countries, and the less advanced societies of the world.

As you have heard, the contemplated programs will be environmental or ecological and will involve emphasis upon investigations in the field in a variety of contexts and settings.

Again, to elaborate before this subcommittee on the subject of the acute problems, both national and global, involved in intelligent use and conservation of all resources, living as well as inanimate, is rather like bringing coals to Newcastle.

The excellent publication issued by the Subcommittee last year, entitled *Environmental Pollution: A Challenge to Science and Technology* epitomized the general problem:

"Considering the powerful forces for ecological change which are at man's disposal, admitting the impossibility of complete foreknowledge of the consequences of many activities, and granting that a highly technical, over-populated world must continue to take risks with natural resources, an 'early warning system' for unwanted consequences is extremely important."

Also:

"None of our natural resources is in so great a supply that it can any longer be considered inexhaustible or truly consumable * * *. Pollution abatement and resource conservation go hand in hand. The resource conservation problem is essentially worldwide and no one geographical or political area is independent of others."

The report also states:

"There is no need * * * for emotional appeals of naturalists, we freely admit that we have a problem * * *. Making appropriate choices as we proceed will depend on much more knowledge than we now have."

Before this Subcommittee, it is probably superfluous for me to point out that alteration of the environment is itself not a recent development. There are historical roots for our present so-called ecological crisis. The use of fire to drive game created the world's savannah's and grasslands. The Romans deforested and overgrazed the land. The fertile crescent of Mesopotamia is now largely arid, sunbaked, and the soil is permeated with salt. The great cedar forests of Lebanon are gone; only a handful of specimens of these magnificent trees remain, high in the mountains. There has been smoke and smog in London from the burning of soft coal since the last of the 13th century and, for at least 500 years, Parliament has shown intermittent concern about the pollution of the river Thames.

While environmental change is not qualitatively new, today's problems and our concern are new because of the new quantitative aspect of the change—the *scale, variety, and speed of the changes in man's physical and social environment*. Another and most important characteristic of current environmental problems is the unanticipated ways in which changes interact, examples being the simultaneous adoption of the internal combustion engine and the surge to urbanization and untoward effects of advances in our ability to transport large quantities of raw materials as illustrated by the recent Torrey Canyon episode off the coast of Cornwall in England.

It is also characteristic that changes in the environment may proceed slowly until a threshold level is reached, after which the change is no longer confined geographically and becomes a matter of regional or global rather than local concern.

It is predicted that our capability to produce electric power will triple by 1980, reaching 492 billion kilowatts with an output of 2.3 trillion kilowatt hours. Per capita power in the U.S. will double by the year 2000, reaching 20,000 kilowatts per person.

By 1975, it is expected that 105 billion gallons of gasoline will power the 130 million vehicles on our highways.

It is the sheer magnitude of the problems generated by today's technology that makes the acquisition of better understanding a matter of global urgency.

During the past two centuries, most governments of the world have devoted their efforts to the accumulation of material prosperity on an unprecedented scale, all in the hope and belief that social and human adjustments would follow spontaneously as they had throughout history.

What none of us envisioned and what many of us are still in the process of realizing is that, in the space of a few decades, science and technology, quite literally, have made a new world and, at present rates of change, will remake the world at intervals of a decade or two.

As one writer has put it:

"Progress no longer trudges as it did in more sedate eras—mankind had about 1,000,000 years to accept the wheel before the first model-T came along but less than half a century between the air-hopping of the Wright brothers' pepped-up kite and the 600-mile-an-hour jet."¹

In order to walk the tight-rope of technological change and human ecology, we must learn to look beyond the conventional statements of this, that, or the other environmental problem in an attempt to survey systematically the sum of individual actions and interactions.

To return to the report on *Environmental Pollution* issued by the Subcommittee last year, the first recommendation was:

"1. To improve our knowledge of what we are about, scientific activity in ecology and related fields should be immediately expanded to provide:

"(a) Baseline measurements in plant and animal communities and the environment—an ecological survey.

"(b) Continued monitoring of changes in the biosphere.

"(c) Abilities to predict the consequences of man-made changes.

"(d) Early detection of such consequences.

"(e) Knowledge of the environmental determinants of disease."

The ecological sciences, on which the International Biological Program concentrates, require greater world-wide efforts.

The United States, the countries of Europe, and the Latin American countries can all be said to be underdeveloped in this area of research. Man's activities are changing the world's biotic communities and have altered much of the earth's surface. Basic information is sorely needed.

It is urgent that studies be undertaken to define existing conditions, to understand the mechanisms that control the components of so-called ecosystems, and to comprehend interactions among ecosystems. The International Biological Program places a long-overdue and most welcome emphasis on primary productivity and its meaning for man, on trophic structure, energy-flow pathways (food chains), limiting factors, interactions of species, bio-geochemical cycling, species diversity, and other attributes that interact to regulate and control the structure and function of communities.

Encouragement to biologists in this hemisphere is already apparent. Argentina, Brazil, Chile, and Mexico are shaping programs through national committees.

In many respects, the International Biological Program resembles the International Geophysical Year during which, physical scientists worked cooperatively to advance knowledge by giant steps. The cooperation established then continues and additional related international programs have evolved, including the International Year of the Quiet Sun and the International Hydrological Decade.

The International Biological Program offers the U.S. an opportunity to participate in urgently needed studies which require collaboration across political boundaries, to help developing countries in their efforts to avoid starvation, to advance the common purposes of the world's biological scientists, to foster efficient use of resources, and to contribute to world-wide coordination of ecological research.

The research carried out under Phase II of the International Biological Program will add immensely to the knowledge base for future planning in the U.S. and in other countries.

There seems to be a general belief that the way to acquire original ideas or to solve difficult problems is to sit and think intensively, preferably in some quiet place. While this procedure may be extremely helpful in reaching a difficult decision or reviewing existing knowledge, I doubt that it fosters creative thoughts.

Dr. Theobald Smith, the great biologist, once wrote:

"Sharp prolonged thinking is necessary that we may keep on the chosen road, but does not necessarily lead to discovery."

Because we are *now*, and *will be* in the future faced with choosing new roads, the insights and alternatives which the knowledge acquired during the International Biological Program will afford can play a major role in our future courses of action.

¹ News Front, January 1967, p. 87.

The problems of today's society, of course, cannot be viewed as isolated matters of science or technology. There are important and inextricable social, economic, and political factors to be weighed in the decision-making process.

In a sense, however, we are only at the threshold of our appreciation of the important and critical role of living resources in relation to man's perception of his needs and desires. It is ironical that only when we have come to realize that the biological world—the biosphere—which sustains our lives and gives them quality by bringing pleasure to our minds and delight to our senses—is threatened, have we come also to realize how primitive and rudimentary is our knowledge of the biosphere.

We have learned a great deal about isolated biological systems, but we know next to nothing about the macro-systems of the real world.

In knowledge, we are very much space-age men in the physical world and stone-age men in the biological world.

To survive and to flourish—in comfort and in dignity and in peace—it is imperative that we increase our understanding and come up into the very real world of today—and of tomorrow.

The International Biological Program offers an opportunity to take a giant step forward and upward.

For these reasons, the Office of Science and Technology supports strongly House Concurrent Resolution 273.

Thank you.

Dr. BENNETT. The purpose of this statement is really to dwell on the two reasons that the Office of Science and Technology supports this program and this resolution.

To cite these reasons before the subcommittee is like bringing coals to Newcastle, because the statement is well larded with quotations from publications of this committee.

The two reasons are these: That first, we feel very strongly that quite apart from its specific content and discipline that science is a neutral portal to international understanding, and I can quote what Mr. Fulton said at the eighth meeting of the Panel on Science and Technology in January of this year where he stated, and I quote:

Joint constructive international programs of science, research and development will cause progress that will construct bonds of well-being and independence among peoples far outweighing the dangers, alarms, and destruction of combat and war.

Our second main reason for being so strongly in support of this program is the fact that it does emphasize ecological sciences, and as this subcommittee has portrayed so well in its publication last year, from which you, Mr. Chairman, quoted, there is a real need for an emphasis on field studies of the type that this particular program will arouse an interest in and will begin to execute during the next 5 years.

I would like to quote from that publication, if I might, the publication on pollution, as follows:

None of our natural resources is in so great a supply that it can any longer be considered inexhaustible or truly consumable * * *. Pollution abatement and resource conservation go hand in hand. The resource conservation problem is essentially worldwide and no one geographical or political area is independent of others.

So I think I can really summarize the feeling in the Office of Science and Technology by saying that we feel very strongly that at the present time we are very much space age men in a physical world and stone age men in the biological world, and that while we have learned something about a few ecological systems, that our depth of understanding

of the macrosystems mentioned by Dr. Smith is really quite superficial and that in order to make important decisions about resources, about the use of technology within the very near future, the type of information that will be stimulated by the International Biological Program on a worldwide basis will be useful not only to the developing countries in helping solve their problems, but will be of very great use to us in understanding the problems that have been created by the thrust of technology in our fast-moving society.

For those two reasons, the nonspecific reason that international scientific programs form a neutral portal to international understanding, and for the specific thrust of this program in the direction of ecological sciences and field studies as opposed to sitting down in a quiet corner and thinking about problems, the Office of Science and Technology reaffirms its strong support of this program and of the resolution under consideration by the subcommittee.

Thank you.

Mr. DADDARIO. Thank you.

Mr. Lukens, do you have any further questions?

Mr. LUKENS. No. Thank you, Mr. Chairman.

Mr. DADDARIO. Mr. Waggonner?

Mr. WAGGONNER. Dr. Bennett, do you suppose this program will really bring about any new studies, or is it going to be a recordkeeping process that is going to simply compile information already available?

Dr. BENNETT. Mr. Waggonner, I am sure that it will bring about new studies simply because of the thrust and the emphasis of the program.

The fact that such a program exists will mean that individuals who have had ideas that they have not brought forward before will bring them forward. We have that evidence in view of the propositions and the proposals that have come in. And it will offer an opportunity for individuals to think big, so to speak. So really I think in terms of programs rather than individual projects, and the evidence thus far is more encouraging along these lines.

Mr. WAGGONNER. Where do you anticipate the people will come from who will make contributions to the underdeveloped countries? Will they be people in those countries, or will it be necessary for us or somebody else to supply these people?

Dr. BENNETT. One aspect of the program that is contemplated and is planned within the program—and I believe it is mentioned in the statement submitted by Dr. Revelle—is the fact that it will be a long program, 5 years, and will undoubtedly continue beyond that, and that one aspect of the program will be to train individuals to work in these specific fields.

Additionally, there are trained individuals in systems analysis, for example, who can turn their attention to problems of this type.

The types of studies that are contemplated really multidisciplinary studies: they are team studies. And so in addition to having individuals with skills that can be applied to this, turn their interest to this, it will be necessary for there to be additional training.

At the present time in this country, difficult as it may be to believe, one of the problems of training individuals in a discipline such as ecology is to find interested individuals, because in a sense we are manpower short. So that the creation of this program will turn the interest

of potential scientists and scientists to this. Scientists, believe it or not, are human beings and they like to be a part of an on-going progressive program, and while there has been a lot of discussion of ecology and the ecological sciences in the past few years, not much has happened because it hasn't been given the kind of thrust this would give it.

I would digress to say that I feel very strongly the same thing is true in the whole field of pollution abatement and with the development of programs at the Federal level, the setting of standards, this area will begin to move, and individuals who have been frustrated in the past, who may have left this field for other areas will get back into it because it will begin to move and they will feel a sense of purpose and accomplishment.

Mr. WAGGONER. Dr. Bennett, you are not trying to tell me that this program is aimed, among other reasons, to remove the frustrations from some scientists, are you?

Dr. BENNETT. Yes, I am. I am telling you that, because that is the only way that you can get scientists who are, in the last analysis, human beings to pitch in and to work in a certain direction, yes, and get them to return to the field they were originally interested in.

Dr. REVELLE. Mr. Chairman, may I add a bit to what Dr. Bennett said. I think Mr. Waggoner has raised a very important and fundamental question.

I would phrase it like this: What is this program going to do that isn't already being done? Why are we going to so many people when they were already engaged in science? What are they going to be able to do under this program that they are not now doing?

The main thing they are going to be able to do is work together. We are hoping to provide the theater and the opportunities for them to talk to each other and to think jointly; as Dr. Bennett said, to think big about their problems.

A marvelous example is our Hawaii project. The people who describe and classify and study the life histories of organisms are all busy doing something along these lines, but here they will be doing it together to attack a big problem which is much bigger than any one of them could work on, but which all together will bring a new level of understanding. The same thing is true of Mr. Smith's study of drainage basins, of entire drainage basins. It has just never been done before because it has never been possible to get the different scientists to come together, to each take pieces of the job in which the sum will be more than—in which the whole thing will be more than the sum of its parts. The understanding they will get when the system is studied as a whole from many different aspects, will be more than the understanding that could be obtained by individuals studying each of these aspects.

The other big thing is that this is an opportunity for people in the United States to go to other parts of the world and work with scientists there under the banner of the International Biological Program, which in many cases might be quite difficult otherwise, and to take advantage of the wonderful diversity and complexity of the earth, to choose the areas where they can learn the most.

Dr. BYERLY. Mr. Chairman, I should like to add a comment to what Dr. Revelle just said.

In all the developed countries of the world there are scientists who are native to the underdeveloped countries and who have not gone back in many cases because the opportunity and the challenge for research was not there. We hope that in some instances at least this program will identify opportunity and challenge in those countries and that their trained people will do their work there.

Mr. DADDARIO. I would not like to leave it, however, that this program is designed to relieve some of the frustrations of the ecologists and as a result attract them back to their field.

Isn't it rather, Dr. Bennett, recognition that this is an area about which we need to know more, otherwise man himself may in fact perish from this earth, and that we will supply also incentive to younger people of great talent to direct their efforts during their formation years and to prepare them for a life's work in this area?

Dr. BENNETT. Mr. Chairman, I hope that that goes into the record. It is far better statement than to say that it will relieve the frustrations of some individuals who work in the field. I agree.

Dr. REVELLE. Mr. Dirksen said something in another context a year or so ago which is very much applicable. He said there is nothing so powerful as an idea whose time has come. And this is exactly the case that we are facing here. The time has come for ecology. This is a device for pushing ecology and for formalizing our support and our interest in ecology among all the scientists of the United States.

Mr. DADDARIO. Well, it is not that we are putting up a warning signal just to scare people. We have had some of the great animal species of this earth just disappear. They continue to disappear at an ever-increasing rate. If this can happen to them, it can happen to us.

Dr. REVELLE. That is right. It undoubtedly will sometime.

Mr. DADDARIO. You will have the floor, Mr. Waggonner.

Mr. WAGGONNER. Really, Dr. Revelle, when you project for 5 years the anticipated costs in a rather rough way—and that is about all you can really do—aren't you really just talking about the initial phases of a program such as you anticipate and this money will really be used for coordinating and planning in preparation for doing some of those things that you feel the world needs to have done?

Dr. REVELLE. I think if we could judge by our experience during the IGY and our experience to date in this International Biological Program, the costs of coordination and of planning will be relatively small.

For example, the central office of the IGY had a budget of not more than about \$250 to \$300 thousand a year. The American office, which was much bigger actually, had a budget of around \$1 million, as I remember it, for 2 or 3 years, something like so. So far at least, the planning phases which are leading to real research projects have cost—or will cost I would guess less than a million dollars total. Wouldn't you think that is right?

Dr. CARLSON. That is right, yes.

Dr. REVELLE. So I think overall over the 5-year period the costs of administration, planning, conferences, getting people to work together and to think together and to train, and then to help to plan training programs—that the total cost over the 5-year period will be not more than 5 percent of the total cost of the research.

Mr. DADDARIO. Mr. Waggonner raised a very important point, and I think it ought to be pursued a little bit, because questions will be asked this year, above all other years, about the funding of such program.

You all have indicated that this is not just a concern of scientists in the United States, but that the documents coming from other countries support this concern. Around this concern, we have a program which is to be developed through the auspices of the international organization so that the time may be set aside for this to bloom out in the right way.

I have not had a chance to read as many documents on this subject as you gentlemen have, but I have had people come to my office from a multitude of countries, greatly concerned about this problem. In some instances they don't know what they are in fact concerned about. I think that it must be spelled out so that we can recognize the complexities of it. When you talk about such things as disturbing the web of life it sounds simple, and yet it is not. It is complicated, it is going to need a great deal of public attention, and it does need it certainly in an international way. You just couldn't possibly be successful in coming to an understanding about this problem and doing something about it if you were to confine it to one area of the world.

It does give us greater opportunities for international participation. Other people, other scientists, and their leaders must recognize that this is one place where there is no alternative but for international action.

Dr. REVELLE. That is right. I think that is a factor—one can make the very significant statement that in all countries these problems of man's relationship to the environment are becoming more and more critical, and they are perhaps most critical, surprisingly enough, in the less developed countries because they are so desperately in need of food and so desperately in need of improving their means of subsistence and of living. They are liable, out of desperation, to take actions that are irrevocable and which may be very destructive, so that they need the knowledge and the know-how, the knowing what to do in some way even more than we do.

Dr. BYERLY. Mr. Chairman, I think relevant to the question, Mr. Waggonner has raised is the fact that UNESCO and FAO have already initiated planning for intergovernmental action with respect to the continual conservation of the resources of the earth to follow after and to build on the basis laid by the IBP. I think the question he asked implied that this will never stop. I am sure he is right about that.

Mr. DADDARIO. Mr. Waggonner's point probably should be followed through a bit. It might be helpful to supply for the record some figures on the overhead costs and administrative costs, of the IGY.

Dr. REVELLE. Of the IGY?

Mr. DADDARIO. Yes.

As I understand it, it was a very low cost, and it shows how you can really have effective action in these areas in the international field.

Mr. WAGGONNER. They should be relatively comparable.

Dr. REVELLE. I will see. I think we can do something on it. I think we can get that out of the National Academy of Science's records.

Colonel Oliver back here, I will volunteer his services to submit those.

Mr. DADDARIO. If he has them available—

Colonel OLIVER. I do not have them available, Mr. Chairman, but we can certainly get them.

(Information requested is as follows:)

DISBURSEMENT FOR THE INTERNATIONAL GEOPHYSICAL YEAR

The following information is taken from the Report on the U.S. Program for the International Geophysical Year, November 1965, National Academy of Sciences—National Research Council (IGY—General Report Number 21).

Distribution of funds.—Funds appropriated by the Congress were apportioned to purpose: a.) scientific and related programs and b.) technical direction. Total planned distribution for a.) amounted to \$41,864,000; for b.) \$1,636,000 which included (i) \$1,351,000 for support of the U.S. National Committee for the IGY and the Academy's IGY staff and (ii) \$285,000 for the National Science Foundation administrative staff that had been established for the purpose. Total requested disbursement of funds during the IGY amounted to \$43,176,261 of which \$41,798,995 was for support of the scientific program and \$1,377,266 for technical direction. The requested disbursement for each program is shown below:

<i>Program</i>	<i>Amount</i>
Aurora and Airglow	\$1, 718, 022
Cosmic Rays	1, 164, 812
Geomagnetism	1, 580, 894
Glaciology	1, 137, 905
Gravity	505, 289
Ionospheric Physics	3, 237, 816
Longitudes & Latitudes	24, 100
Meteorology	2, 226, 800
Oceanography	2, 007, 054
Rocketry	2, 598, 952
Seismology	877, 049
Solar Activity	307, 378
World Days	229, 050
Interdisciplinary Research	1, 750, 526
Earth Satellite	19, 843, 210
World Data Center	1, 513, 587
General Scientific Support	1, 076, 551
Technical Direction	1, 377, 266
Total	43, 176, 261

Mr. YEAGER. Mr. Chairman, Mr. Rumsfeld, who was here earlier, had to leave and asked me to ask some questions on his behalf in connection with the cost data.

I think that most of these may have been answered, but I would like to ask permission on his behalf that he be allowed to submit some additional questions to the witnesses and that the answers be incorporated in the record, if this is agreeable.

Mr. DADDARIO. Additional questions may be submitted by Mr. Rumsfeld or any other members. As we look the record over, there may come to mind some points which need to be clarified further.

Mr. YEAGER. May I just ask two questions of Dr. Revelle.

Do you anticipate any of the funding either in this country or from others will come from private sources, foundations, this type of things? And if so, do you have any estimate of what percentage?

Dr. REVELLE. I hope that we will be able to get some foundation support, particularly for training of scientists from the less developed

countries. This is a difficult thing to do at the present time with Government funds. We have not so far applied for such foundation support.

On the other hand, it is quite obvious, knowing the way science works in this country—I mean you will recognize it, that a great deal of the support of the program will come from the funds already available to universities and to research institutions in terms of the salaries of their professors, their laboratories, their facilities and things like that.

Typically, in these basic research fields, the universities contribute from their own funds quite a bit in addition to Government support they get.

Dr. BYERLY. Mr. Chairman, if I may add: The President of the International Council of Scientific Unions did obtain a foundation grant for the International Council, a portion of which was used for the IBP.

Dr. REVELLE. That is correct.

Dr. BYERLY. It was not a large sum, but it was used in support.

Dr. CARLSON. Mr. Chairman, within the United States the universities do cost-share on research programs which these would come under, so there would be that aspect of the private sector sharing in the cost.

Mr. YEAGER. This would be on top of the figures that you are estimating now.

Dr. REVELLE. Yes, sir.

Mr. YEAGER. Just one last question.

Dr. Revelle, in your statement you indicated that to achieve a decent life for the living generation, further change in our biology may be needed. Then you say we may have to reduce fertility as reckoned on a worldwide basis to levels below any previously attained.

Now, as head of the Center for Population Studies at Harvard, you have done about as much thinking in this area as anyone. The question is, is there any doubt in your mind that there will have to be some change in the ratio between the birth and death rates, and if so, what time scale would you think would become critical at this point, looking at it from an ecological point of view?

Dr. REVELLE. Well, I think that in the long run—and by “long run” I mean 30 to 75 years—we must bring birth rates down to the level of death rates. If we can bring them down to the present level of death rates, this means a very, very low—a much lower birth rate than human beings have ever had on a large scale before.

Death rates are now—well, in the long run death rates will come up to about 15 per thousand per year, because men live to be about 70 years old on the average. We have to bring birth rates down to about 15 per thousand per year over this time span of the next 30 to 75 years. Otherwise, I think any knowledgeable calculation of the earth's available resources will tell you that people cannot raise their standard of living above what it is now if population increase continues beyond that time.

During this period we are going to have a big increase in population. This is typical of the history of human populations I think for the last 10,000 years, that there have been several periods of rapid in-

crease, then society found a way to bring birth rates and death rates into balance. But each one of these transients resulted in a big increase in population. This was true when agriculture was invented. It was true in northern Europe when the horse collar was invented and the three-field farming system. It was true in the industrial revolution in the Western World.

The population of Europeans and European descendants overseas increased from 100 million to 500 million from the year 1600 to the year 1900. Now the European populations everywhere are bringing their birth rates down pretty close to this level of 15 per thousand. Our rate now, for example, is 18 per thousand; Japanese, about 17; Hungarian, about 13.

The reason why we have to do something about this now—there are two reasons. One of them is it takes a long time to get results.

Dr. Bennett and I have been involved with a study which shows that over the next 20 years almost nothing that you can do within what seem to us likely possibilities will make much difference in the size of the population in the underdeveloped countries in 1980 or 1985. Maybe about a 10-percent difference. But beyond that time it will make a much bigger difference.

The other important consideration is that rapid rates of population growth themselves, quite apart from the ultimate total size of the population, have a seriously deleterious effect on economic growth because of the disproportion between children and adults, and because the per capita income does not go up very fast.

I am not sure if I am quite answering your question, but I myself am completely convinced that we will have to bring birth rates down below a level that they have ever been before on a worldwide basis; and this is a scientifically cautious statement.

Mr. YEAGER. I think that is very responsive. It just seems to me that there must be some point where, if the trend is not altered rather dramatically, you might go beyond the point of no return.

Dr. REVELLE. That is correct, just simply because we will have so many people that the resources will not really give a chance for raising standards of living.

Mr. YEAGER. Thank you, Mr. Chairman.

Dr. CARLSON. Mr. Chairman, I am the chairman of the Interagency Coordinating Committee of the IBP, and I have a brief statement I would like to have permission to insert into the record.

We have discussed throughout the conservations here everything that I had included in it, so I do not think I need to say anything further.

Mr. DADDARIO. Thank you.

(The prepared statement submitted by Dr. Carlson is as follows:)

PREPARED STATEMENT OF DR. HARVE J. CARLSON, DIVISION DIRECTOR, BIOLOGICAL AND MEDICAL SCIENCES, NATIONAL SCIENCE FOUNDATION

My name is Harve J. Carlson and I am representing the National Science Foundation and the Interagency Coordinating Committee for the International Biological Program.

In November 1965 the National Science Foundation was designated as coordinating agency for Federal participation in the International Biological Program by Dr. Donald Hornig. In the authorizing letter, Dr. Hornig states that

"* * * the resources, facilities, and staff of the Federal agencies must be involved in this program if it is to be a success." He further encouraged the exploration of "* * * the optimum nature and extent of Federal agencies involvement with this program."

In order to carry out this responsibility, the Foundation established the Inter-agency Coordinating Committee for the International Biological Program (ICC) with Dr. Harve J. Carlson as Chairman. The ten Federal agencies who have appointed representatives to this Committee are: Atomic Energy Commission, Department of Agriculture, Department of Defense, Department of Commerce, Department of Health, Education and Welfare, Department of Interior, Department of State, National Aeronautics and Space Administration, and Smithsonian Institution and the National Science Foundation.

The interagency committee has met a number of times in Fiscal Years 1966 and 1967 and accomplished several important tasks. First, it has reviewed requests from the National Academy of Sciences U.S. National Committee for the IBP (USNC) for the support of the costs of planning and development of the U.S. component of the International Biological Program, and has worked out multiple agency funding for these Academy activities. Second, it has inaugurated a survey of research within the various Federal agencies that might be congruent with IBP objectives, and encouraged the U.S. National Committee to consider whether the Federal agencies activities might contribute to a stronger U.S. participation in the IBP. In addition, discussions have been held to explore mechanisms whereby several of the pertinent Federal agencies may participate in the support of the actual research projects that will make up the U.S. program.

It is generally agreed both by the ICC and many of the USNC members that the U.S. program can be handled best by multiple agency involvement rather than centralizing support in a single Federal agency. Thus, IBP is not visualized as solely an NSF program although it is the responsible coordinating agency and will continue to play a strong role. Specifically, the arguments for encouraging multiple agency involvement are: 1) some projects will be best performed at existing major facilities of such agencies as: Atomic Energy Commission, Bureau of Commercial Fisheries, U.S. Department of Agriculture, Water Pollution Control Administration, Public Health Service, etc.; 2) scientific talent is scarce, and in many cases a current research project can, by adding international components, be effectively incorporated into the IBP to the benefit of both the overall IBP Program and the ongoing research project; 3) the skill of Federal agencies in international collaboration will be a useful asset; and 4) the coupling of fundamental and applied research in some cases can work to their mutual advantage. Where the world food supply and human adaptability are the underlying themes, it seems particularly important to have close communication between specialists representing the basic sciences and those concerned with the application of science and technology to specific problems.

Representatives of participating agencies in ICC have expressed interest in IBP, are aware of the potential role of their agencies in IBP, and are making efforts to insure that a strong U.S. program will be developed. The interest of Federal agencies in the IBP has been expressed by the submission to the USNC of lists of current intra-mural research activities for review and possible inclusion in the U.S. program. A soon-to-be-released report of USNC will be devoted largely to the listings of IBP-approved on-going projects in Federal laboratories.

A report by the USNC on the proposed plan of the U.S. IBP program, has been released to some 50,000 U.S. biologists. The dissemination of information on IBP on such a wide scale has brought notice of this activity to the attention of a number of U.S. scientists who, heretofore, have had little knowledge of its aims or objectives. That U.S. scientists are responding favorably to the U.S. concept of IBP is evidenced by the number of research proposals now being received by not only the Foundation, but a number of other granting agencies as well.

A joint attack on specific major problems by the Federal agencies and the U.S. scientific community at large warrants serious consideration. For example, we all recognize that one of the most pressing problems facing man today is the undernourished condition of large segments of the world's population. Foremost among the objectives of the IBP is the consideration of the biological basis of world productivity. This includes the study of "* * * organic production on the land, in fresh waters, and in the seas, so that adequate estimates may be made of the potential yield of new as well as existing natural resources * * *."

An international cooperative attack on world productivity would yield information on the relative value of biological communities to mankind, and would point out measures which might be taken to increase food production. This program would provide world political and economic leaders with the knowledge essential to a meaningful attack on the population-sustenance crisis.

Neither the Federal agencies nor the academic scientific community functioning alone can draw on the maximum problem-solving capabilities of the United States. Potentially, IBP could bring these two groups together. Outstanding investigators concerned with basic scientific questions might be induced for short periods to work on their particular specialties within the larger context of pressing problems, working together with personnel of existing mission-oriented Federal laboratories and utilizing the superior facilities and equipment at the installations. This creative interplay of groups on IBP problems may suggest ways of making our problem-solving arrays more powerful.

With the identification of intra-mural programs within Federal laboratories, and with the formulation of IBP research proposals by the scientific community-at-large, a sound U.S. IBP program can be expected to develop.

Dr. REVELLE. I think the same thing is true of Dr. Smith. May we have permission to insert his statement in the record also?

Mr. DADDARIO. The record will remain open for a reasonable period of time for statements or for any additions you would like to submit. (The prepared statement submitted by Dr. Smith is as follows:)

PREPARED STATEMENT OF DR. FREDERICK E. SMITH, SCHOOL OF NATURAL RESOURCES, UNIVERSITY OF MICHIGAN

THE UNIVERSITY OF MICHIGAN,
SCHOOL OF NATURAL RESOURCES,
DEPARTMENT OF WILDLIFE AND FISHERIES,
Ann Arbor, Mich., May 11, 1967.

To: Committee on Science and Astronautics, U.S. House of Representatives.

Attached are portions of two reports on trends in ecology. The first, 4/15/65, is a part of the summary report by the Ecological Study Committee to the Ecological Society of America. The second, 5/5/67, is a part of the first (rough) draft of a report on ecology to be a chapter in the summary of Research in the Life Sciences sponsored by the National Academy of Sciences.

These reports reflect nation-wide excitement among ecologists over the rapidly growing feasibility of powerful, large-scale, complex studies. This new potential results from several factors: technological advances in equipment, especially in meteorology and in physiology, better-trained ecologists and more of them, the availability of high-speed computers, and the rapid development of systems analysis and related techniques. For the first time, the complexity of analysis in ecology can approach the complexity of the system under study.

The major U.S. programs in the IBP are implicit in these reports. The IBP, however, has focussed on studies that require extensive coordination and team effort. Such studies would otherwise be slow to develop, despite their immediate relevance to human welfare, because ecologists have tended to avoid team research.

Support for the IBP is growing throughout the profession, and now includes many of the best minds available. Every week more investigators ask to participate. Most of these are keenly aware of the test that is being made of the team approach in ecology.

Also attached is an address to the National Research Council, 3/13/67, on the expected effect of the IBP upon Ecology.

Respectfully submitted.

FREDERICK E. SMITH, *Professor.*

FROM THE SUMMARY REPORT OF THE ECOLOGICAL STUDY COMMITTEE (PREPARED BY R. S. MILLER) TO THE ECOLOGICAL SOCIETY OF AMERICA

RECOMMENDATIONS

The following tentative recommendations are based on the Study Committee's "Interim Report" submitted December 18, 1964 and subsequent discussions of the report at the Study Committee meeting January 23-25, 1965:

1. Greater emphasis on modern ecological principles is needed in high school and undergraduate biology courses, especially the latter. Leading college texts have been quick to upgrade material at the molecular level but have failed to do the same at the ecological level. Suggested solutions: (1) encourage qualified ecologists to take more active part in preparation of elementary texts, or (2) establish a "writing committee" as was successful in upgrading ecology in high school biology texts.

2. The quality of graduate work and training should be increased and more universities should develop strong coordinated programs, i.e., interdisciplinary programs between botany, zoology, microbiology, and applied biological disciplines (forestry, agriculture, conservation, etc.). Very few universities now have such graduate programs. Many universities with the highest overall academic ratings have practically no graduate work in ecology, even though such schools have outstanding individual ecologists on their staffs. Suggested solutions: (1) encourage heads of biological science divisions to get individuals in different departments to work together in graduate training, (2) increase the number of special training grants for interdepartmental programs (see item 6b below) and (3) develop special interdepartmental facilities as described in 3 below.

3. Large scale permanent year around experimental facilities for ecology are a critical need for the future. These should include both laboratory and field facilities for experimental work with entire ecosystems as well as for populations and individual organisms through their entire life cycles. Such facilities are expensive and require large scale government support and, in some cases at least, the cooperation of several institutions is desirable. Since large scale expensive facilities for applied ecology (for example, water pollution laboratories or forestry research laboratories) are now being supported in large numbers by the Federal Government, it is imperative that equally adequate facilities for basic ecological research be similarly supported to avoid an imbalance between basic and applied research that is in danger of developing when basic principles underlying the applications are imperfectly known. Such imbalance in any field leads to a tragic waste of money as trial and error procedures predominate, and expensive equipment and buildings are assembled to be manned by inadequately trained personnel.

The few large experimental facilities for basic ecological research existing at present are associated with a very few universities and with several of the Atomic Energy Commission installations. Three types of facility now operating that might serve as models of arrangements which would be feasible in the near future are: (1) a facility completely administered and supported by a government agency; (2) a facility organized and operated by a single university, with all senior research personnel being university faculty members, but with financial support from Government agencies and/or industrial contractors, as well as by the university itself; and (3) a facility operated by a group of universities. While there are many advantages to each arrangement, a strong association between facilities and universities is highly desirable to insure freedom of research and to promote training at both the predoctoral and postdoctoral levels, where such training is now inadequate (see item 2).

4. Field stations devoted to a broad spectrum of biological study (not to be confused with the specialized environmental research facilities discussed in paragraph 3) need to be increased in number in some geographical regions and especially in quality. The field station provides one of the most effective devices for stimulating interest among students in environmental problems. There should be major teaching and research field stations in each major ecological region or "biome" such as tundra, northern coniferous forest, temperate deciduous forest, tropical forest, temperate grassland, tropical grassland, chaparral, montane, freshwater and marine situation, etc. Many such stations should operate the year around, not just in the summer.

At present, the number of marine stations along the U.S. coast is adequate that many stations are inadequately staffed and badly need financial support. Terrestrial and freshwater stations are far too few in number, and too many of the few which now exist operate only in the summer, and are inadequately staffed or equipped or both.

It is especially recommended that field stations seek funds for immediate purchase or lease of large areas of land and waters surrounding the field station adequate of long range field research. Otherwise, stations will find themselves without any environment to study as a result of population pressure or pollu-

tion, as has happened with many other laboratories such as the Marine Biological Laboratory at Woods Hole where either lack of funds or lack of vision prevented acquiring such field areas when it was possible to do so.

Furthermore, it is recommended that foundations give serious consideration to proposals submitted by well staffed field stations and ecological institutes for regional inventories, which include functional as well as descriptive and taxonomic measurements and which consider the "biological basis for human welfare" as emphasized in the proposed International Biological Program (IBP).

5. Theoretical research in ecology should be increased in the future and more theoretical papers should be solicited and published in ecological journals. At present, many theoretical papers of great importance to ecologists are published in non-ecology journals, partly due to a mistaken widespread impression that *Ecology* and *Ecological Monographs* consider only "data" papers, and partly due to lack of space in these existing journals. While one hesitates to suggest increasing the number of biological journals, there is no question that the field of ecology is not represented by a reasonable share of journals in the USA.

6. Since manpower is perhaps the most critical shortage in the ecological area the following recommendations are considered especially important.

a. An immediate inventory of existing manpower in ecology, and a forecast of manpower available four years hence is essential. The existing Scientific Rosters are not adequate of such inventories because (1) many persons who list themselves as ecologists are, in reality, primarily interested in taxonomy, evolution or other areas, and (2) many scientists (for example, soil scientists or entomologists) who are doing important work in ecology do not always list themselves as ecologists.

b. An increasing number of training type grants for coordinated ecology programs (see item 2 above) should be strongly recommended to NSF, NIH and other granting agencies.

c. The number of senior fellowships should likewise be increased to enable ecologists who lack experience with recent techniques and ideas (as for example radioisotope procedures or ecosystem theory) to spend sabbatical, etc., at centers where modern experimental facilities are developed (see item 3 above).

d. Financial support for a series of "in depth" symposia and special "institutes" (patterned after the summer training institutes which have done much to upgrade many fields of biology) should be sought so that much needed exchange of ideas can be facilitated. Such symposia and institutes should deal with both basic and applied aspects in balance as suggested by the following titles: "systems ecology", "population biology", "environmental biology for civil engineers", "environmental biology for urban planners", etc.

e. The National Academy of Science should be encouraged to develop some kind of effective interagency environmental committee in Washington to aid in the coordination of the many environmental science groups. At present there is much duplication of effort by these groups and all too frequently an unintentional development of contradictory policies.

7. It is recommended that the establishment of a national center for environmental sciences be seriously considered.

FROM THE FIRST DRAFT OF A REPORT ON ECOLOGY (CHAIRMAN OF PANEL, A. H. HASLER) TO BE A CHAPTER IN THE SUMMARY OF RESEARCH IN THE LIFE SCIENCES, SPONSORED BY THE NATIONAL ACADEMY OF SCIENCES

IV. FUTURE TRENDS

Ecology is already a rapidly growing discipline. Since it does not yet show the effects of the added stimuli that come from a new ability to cope with complexity, and from the new national emphasis on environmental problems, it will grow even faster in the future. The additional impact of the International Biological Program, which will relate ecology to the problems of man on a worldwide basis, produces an immediate need for at least doubling the number of ecologists in this country.

There is an increasing awareness among scientists and non-scientists alike that the most difficult problems facing mankind in the question of his continued, harmonious existence on this planet are at a level of biological organization and complexity that also provide the greatest, and perhaps the ultimate, challenge in biological research. All biological events occur in an ecological context, and it is at this level of organization that such events have their ultimate meaning.

the reality of nature. These facts, coupled with a new ability to cope with events at the ecosystem level of organization, portend an exciting and rapidly expanding future for ecological research. There will be rapid growth in interdisciplinary research, with emphasis on the design and analysis of complex experiments on natural ecosystems.

The greatest single change under way is a shift toward the study of whole ecological systems, such as lakes, forests, estuaries, or drainage basins and landscapes. Projects on all of these are being designed. Each one requires a variety of investigators and a degree of interdependence among investigators that has previously been rare in ecology. Ecology will become deeply interrelated with such allied areas as meteorology, hydrology, soil science, geology, and physical oceanography. The theoretical framework surrounding our understanding of whole systems will involve cybernetics, information theory, game theory, decision theory, topology, general systems theory, stability control theory, and open systems theory. Despite this mixture of disciplines, the goal is the development of a unified theory of ecosystems.

A major problem hindering the study of large systems is the difficulty of designing both controls and replicates for experiments. Both are needed, however, and a trend toward large-scale manipulative experiments that include these safeguards must take place. This requires large study tracts that are guaranteed against disturbances, in a variety of major biomes (some of these can include strong human influences such as agriculture, providing the systems are relatively unchanging over a decade). It is axiomatic that, the larger the system under study, the fewer there are (fewer molecules than atoms, fewer populations than organisms). The relative paucity of ecosystems requires a greater emphasis on techniques that rely on less replication than is usual in other fields. This places a greater burden on the reliability of the data, in systems where reliability in the past has been poor. Thus, even if large study areas are obtained, their instrumentation must nonetheless be intensively developed.

The synthesis of a theory of ecosystems, and also the development of programs for their management, require large increases in research at the level of physiological ecology. We know far too little about too few species. The field is young, and has so far stressed organisms in extreme environments, such as the desert or the arctic, where environmental variables test the capacities for survival. In the future, species in benign environments will be studied more, even though this will require greater precision in measurements of both physiological and environmental factors.

A second impetus to greatly expanded work at the level of physiological ecology derives from the worldwide need for more and better food. The present array of cultivated crops and domesticated animals were collected while man exploited only the most favorable parts of the world. There is no reason to suppose that these species are the best for the rest of the world. Indeed, a number of recent studies have shown new species to be superior. Thirteen species of wild game in Southern Rhodesia have proven to be more profitable than cattle as sources of meat. Red deer, on the island of Rhum, off Scotland, are much more productive than the combination of sheep and cattle that previously were grown there. On the Siberian tundra, the Saiga Antelope is superior to sheep and goats, while in many regions of Australia kangaroos are superior to sheep. Man has only begun to examine this millions of other species around him for those that may be useful.

Another trend in physiological ecology will be a shift away from small samples of genetically homogeneous material to larger samples encompassing the full range of genetic variability present in the population. Although this adds another dimension of complexity to such research, it is the only way that such studies can become useful in the synthesis of information at the level of populations and ecosystems.

To date very few studies have combined competent study of both population genetics and population ecology in natural populations. Recent laboratory work shows that the two are strongly interdependent, that genetical composition changes rapidly under varying ecological pressures, and that ecological capacities alter greatly with varying genetic composition. The role of reversible changes in gene frequencies as one of the ways populations adapt to environmental variation (in addition to changes in behavior, physiology, etc.) will be studied much more extensively. Here again, the integration of the two subjects, both mathematically complex, is feasible only with development of systems methods.

The role of microbiology in ecology will increase greatly. In all natural systems, the cycling of matter and the degradation of energy includes the fungi and bacteria, a biological component second only to the green plants in quantitative significance. We know shockingly little of the physiology and ecological roles of free living microorganisms, and even less of their taxonomy. In both soil and water, we need technological breakthroughs in methods for separating dissolved material from particulate material, living material from dead material, producer material from decomposer and consumer material. The standard methods of soil chemistry, for example, are entirely unsuited to an analysis of nutrient cycling in ecosystems. In natural ecosystems, up to a third of the animal protoplasm exists in small soil organisms, and an equal proportion of plant protoplasm may exist in soil decomposers. These are lumped with the "humus" in most studies.

Finally, the greatly increased public emphasis on problems of the environment will alter the role of ecologists to include, in addition to their basic scientific activity, an unavoidable involvement in human problems. Some of the problems that ecologists will have to think about, and act upon, include the following:

1. The nature and degree of restrictions on human and other populations and environments that are necessary and desirable.
2. The extent to which a laissez-faire philosophy of politics (national and state) and economics have ecological survival value.
3. The necessity for and means of regulating modification of climate and the chemistry of the biosphere.
4. The most equitable or efficient use of the world's natural resources on an international basis, e.g. open sea fisheries, continental water resources, land types.
5. The minimum and optimal requirements of space, natural environments, and recreation in human environments.
6. The amount of undisturbed natural environment and species diversity required to minimize disturbances from endemic and introduced pest species and to provide population stability.
7. The evolution of legal mechanisms for regulating environments on a national, or international basis with respect to scientific evidence, and without regard for political or short-term economic expediency.
8. Application of ecological analyses and concepts to human affairs, including use and conservation of renewable and non-renewable resources, landscape design, land policy and agricultural and urban development.
9. Relation of ecological principles to the management of water, soil, native plants and animals, and the management of wilderness areas so as to maintain their integrity.
10. The course and effect of pollutants on plant and animal communities, the chronic effects of domestic toxicants, the causes of eutrophication and means of reversing the process.
11. A national ecology center to store and classify ecological data, to provide data to government and industry, and to act as a scientific manpower center for consultation on regional and national ecological problems.
12. Direct cooperation with local, state and national governments in the design and regulation of human environments, including town and regional planning, pollution control, planned use of natural resources, etc.
13. Ecology training programs for non-professionals concerned directly or indirectly with the application of ecological principles and knowledge to problems of human environment.

ANNUAL MEETING OF THE NATIONAL RESEARCH COUNCIL, MARCH 12-14, 1967

The following paper was delivered at a plenary session entitled: The Biological Basis of Productivity and Human Welfare.

THE INTERNATIONAL BIOLOGICAL PROGRAM AND THE SCIENCE OF ECOLOGY

(By Frederick E. Smith)

Much of the International Biological Program is based upon the development and application of ecological principles. Success in the program requires a strong acceleration in the growth of ecology, especially in those areas of knowledge

needed to solve problems already facing mankind. These problems exist in both the emerging nations and in advanced countries such as ours. Having such information now, not later, or in 1970, not 1980, may have profound effects upon decisions to be made as man completes his domination of the earth.

Ecology is the study of interrelations among organisms and their environments. It is a minor science, primarily because science as a whole has developed in countries that have had little need of ecological knowledge. All of these countries have adequate natural resources such as soils, water, and fuel. Utilization of these resources has been very profitable, and the capacity of the land to support mankind has been greatly increased, even though the methods used may have been wasteful or ecologically unsound. In such areas as forestry and agriculture, for example, we have been able to afford methods of production and of pest control that make little use of ecological knowledge.

From the point of view of natural selection and evolution, man has enjoyed an unprecedented excess of competitive advantage over other organisms, and has been able to exploit the world rapidly without need of ecological efficiency. It is obvious, however, that as man continues to expand, the value placed upon efficiency will increase. Eventually, also, man must reach a workable agreement with the biosphere as a whole that will guarantee him the necessary supplies of renewable resources.

Most of us know that the fossil fuels we are using were produced through geologic time as a byproduct of biological activity. In addition, major features of our environment are controlled by organisms. All of the soils of the world are produced by organisms, and their maintenance depends today upon a protective mantle of vegetation. Without life, we would have only thin layers of rockdust washing rapidly to the sea. The atmosphere, also, is greatly changed by biological activity. All of the molecular oxygen is of biological origin. In a literal sense, for every gram of free oxygen in the air and in the sea, there is somewhere on earth an equivalent amount of organic matter. If the total mass of organic matter is reduced, the amount of oxygen in the air must be reduced correspondingly. When the major forms of life first became abundant in the ocean, the air seems to have been about 10% oxygen. When life covered the land surface, it had risen to about 15%. Now it is about 20%, but the destruction of forests and the burning of fuel raises doubt on the levels of oxygen in the future. Finally, the world distribution and movement of many elements such as phosphorus, nitrogen, potassium, iron, and sulfur, is affected strongly by the accumulated activities of organisms.

Man is beginning to have a significant effect upon the biosphere as a whole. It is essential to his future that he does not upset the biogeochemical cycles that have made the earth an inhabitable planet.

As human expansion continues, problems in two major areas of human welfare place increasingly strong demands upon ecology. First, now that the most favorable environments of the earth are occupied and developed, man is concerned with the development of the remaining countries. Their environments are far less tolerant of inefficiency and error, and we will have to know much more than we do about the capacities of organisms and the limitations of environments, if such ventures are to succeed. Second, in the advanced countries where the problems of existence are not so pressing, man is concerned with the quality of the environment. Constructive participation in environmental processes requires the application of ecological principles, often in systems that are exceedingly complex. The greatest challenge will be to find methods of improving the environment that do not conflict with human activity. Getting rid of people is not an acceptable recommendation.

Thus, the need for ecological knowledge is increasing, and the International Biological Program is designed in part to fill this need. Particular contributions to ecology are more easily discussed if the nature of the field is summarized.

Quantitative ecology is a young science developing within a subject that has a long and rich history. Modern research can be classified in three levels with respect to the complexity of the unit of study: the individual, the population, and the ecosystem.

At the level of the individual, physiological and behavioral ecology is concerned with the environmental needs of organisms, the environmental extremes they can tolerate, and their capacities to function well in a broad spectrum of conditions. Such capacities often are based upon homeostatic mechanisms as complex and subtle as those that regulate the internal physiology of organisms. They may operate rapidly through behavioral mechanisms, such as the way leaves control

evaporation by curling, or the way a lizard controls body temperature by shifting between sun and shade. They may operate more slowly through physiological adjustments, as shown by the way fish adapt to changes in water temperature by changing the temperature characteristics of many of the body functions.

These studies at the level of the individual require many of the tools of physiology and behavior, as well as those of micrometeorology. Both in the laboratory and in the field, this is a well-instrumented and highly technical subject. In the last few years, problems of instrumentation have begun to interest Bio-engineers, while problems of complex data analysis have attracted those proficient in computer techniques.

Many contributions to this field are made by scientists searching for better ways to produce crops, or better strains of animals and plants. Today many different strains of a few species of cultivated or domestic organisms available, each able to function most efficiently in some particular environment.

At the next level of complexity, population ecology is concerned with processes that influence population density, production, and persistence. Interactions between populations from competition for common needs, or from predation of one upon another, are included. A generation ago as many mathematicians as biologists participated in the development of population ecology, and a strong mathematical orientation persists today. The major analytic tools are those of demography and statistics. Population models that have emerged are used in the construction of most management programs of fish, game and forests throughout the world. Many of these models have mathematical analogues in molecular kinetics, or in economics and sociology.

More recently, physiological and behavioral measures have been extended to the population level. In one line of application, the behavioral and physiological traits of individuals are integrated into predictive models of population behavior. Such systems may be very complex, and depend upon computers and systems analysis. In a different approach, physiological measures are made directly on populations, producing estimates of population metabolism and population feeding efficiency. In both cases, population analysis is brought more forcefully to bear upon the interactions between populations and their environments than has previously been the case.

Population genetics and population ecology have only recently been combined to any extent. It is now evident that gene frequencies can change much more rapidly than previously believed, so that genetic change can be important during ecological studies. It is also evident that selection factors involve more complex ecological relations than previously considered, so that changes in density or age structure can be significant in genetic studies. Most of the mathematical models used in each of these areas can be handled with ordinary methods of analysis, but their combination in genetic ecology leads to cumbersome expressions that require the computer.

Many of the principles of population ecology have been derived from applied areas, including forestry, fishery management, wildlife management, and insect pest control.

Within the last generation population ecology has become popular in biology departments. Because population ecologists are mathematically oriented, and because geneticists have changed from a mathematical to a chemical orientation, the teaching of biometrics in many departments has shifted from the geneticist to the ecologist.

The largest unit of study in ecology is the ecosystem. This is the total system of populations together with all non-living components that interact in a defined region of space and time. Hundreds or thousands of species are included. These systems are open at their boundaries, some more so than others. An ecosystem can be a whole lake, or only the open water in the center. It can be a woodlot, or a large forest. In the laboratory it can be a microcosm in a jar. The entire earth can be treated as one ecosystem, with biogeochemistry serving as the basis for studying nutrient cycling.

In the past, ecosystems have been regarded primarily as backgrounds against which studies of components are executed. Ecosystems may be identified by naming dominant characteristics, such as a beech-maple forest, a short-grass prairie, or an alkaline pond.

Several aspects of ecosystems have been the subject of quantitative analyses. Those encountered most frequently are the total densities of different groups of organisms; the diversity of species present in different groups; the pattern of

flow of energy through the food web; patterns of distribution among the dominant species; vegetational structure; and the relation between ecosystem organization and stability. No one ecosystem has ever been studied from all of these points of view.

Within the last decade the quantitative analysis of all aspects of ecosystems has been considered seriously. For the first time in history this has become a realistic goal. We have new and powerful tools in systems analysis and in the availability of high-speed computers. It is only with such tools that the complexity of ecosystems is expected to yield to analysis. The application of these methods has already begun. Since ecosystems are a family of systems distinct from those of economics, or industry, or aeronautics, systems analysis will have to be tailored to fit this new area of application.

Systems analysts often find that systems have properties not easily predicted from the study of components. Some components can be varied greatly without appreciably affecting the performance of the system as a whole, while small changes elsewhere can change the entire pattern of performance. Ecosystems appear to have similar properties. The effect of the total system on the dynamic state of many of its component populations may be far more pervasive than previously thought. Thus, the conviction is growing that ecosystem analysis should be at the forefront of ecology, rather than serving as a background. Although the analysis of the whole may be cumbersome, ecosystem principles may emerge that will simplify our view of ecosystems, and possibly even simplify management programs of biological resources.

At this time, despite some courageous attempts, we have no way to interrelate different aspects of ecosystems, such as energy flow, species diversity, and vegetational structure. Furthermore, although we can describe each of these quantitatively, we have only the haziest of ideas on why some particular set of results is found. In this sense, ecosystem analysis is a discipline that is just being born.

Among the contributions that the International Biological Program will make to ecology, the most significant immediate achievement will result from the establishment of ecology and ecologists in countries that now lack them. These are emerging nations that do not yet have scientific and technical facilities capable of supporting intensive research. They do have formidable problems to be solved. If we estimate the *per capita* value of all natural resources available to each country, we find that these values run at least one hundred times higher in the advanced countries than in many of the emerging nations. It is certainly difficult for the United States, which has resource assets of about \$30,000 per person (U.N. survey, 1963), to visualize how to solve problems of development in countries where the figure is less than \$100 per person. Producing food with poor soil, poor nutrients, and inadequate rainfall is only a small part of the problem. The cost of power for mechanization is prohibitive; the cost of transportation of the food to cities is prohibitive; and the cost of food preservation is prohibitive. Our system of agriculture, which now consumes as much energy in fuel as it produces in food, is applicable only to a few favored regions of the earth. We have achieved high efficiencies of production per acre, and per man-hour, but probably have the lowest efficiency in the world per unit of power. Many of the emerging nations have excesses of land and people, and little or no source of power.

Large areas of Asia, Africa, and South America can reach a good standard of living only if they achieve an efficiency of conservation and re-utilization of natural resources that is at least an order of magnitude greater than ours. With such formidable problems, these nations must explore every possible resource. To do so they must know much more about the characteristics of their environments, their capacities for alteration, and the potential of native species of plants and animals for man's use. The kind of ecology they need has already been developed in other countries, and can readily be applied locally.

The I.B.P. will make major contributions through its programs for the training of foreign scientists and for the international exchange of personnel. We are exploring already the possibility that native game in Africa such as Thompson's Gazelle or the Impala may prove to be more productive than cattle or sheep. Other countries, whose trees are little known to ecology, are asking that their forests be studied. Several nations hope to assess the productivity of estuaries, boundaries between fresh water and marine water that appear to be good sources of fish protein.

Similar studies of all faunas and floras on land and in the sea involve the intensive development of physiological and behavioral ecology throughout the world. Not only will the number of such ecologists be greatly increased, but the subject matter will become a comprehensive survey of species characteristics that will lift a minor subject to a position of major status.

A second contribution to ecology results from a program to assess the capabilities of this planet as a life-support system. By mapping and characterizing all of the environments, both terrestrial and marine, and by making suitable measures of present and potential production, the I.B.P. hopes to obtain some practical measure of the earth's capacity to support mankind.

The contributions to ecology will be, first, the gathering of such information on a comparative basis, and, second, the development and standardization of techniques for making such measurements.

The latter is the larger task. In biology, comparability of data involves more than the problems of scale of measure. Respiration, for example, can be measured as the rate of oxygen consumption, or the rate of carbon dioxide production, or the rate of heat production, or the rate of loss of weight. It can be measured per unit wet weight, per unit dry weight, or per unit calorie on combustion. In order to compare such measures, conversion scales must be worked out for every organism in every set of environmental conditions. Obviously, in the I.B.P. some one method of measuring respiration will have to be chosen. Whether or not the units of measure are metric is a trivial problem.

The I.B.P. is contributing already in the several international handbooks being prepared for us by all participating countries. These establish standards techniques for the estimate of thousands of parameters. Some of these handbooks will be available this year.

As these standards become used, the comparative ecology of the earth becomes a realistic goal. The wide range of ecological systems that will result, complete with estimates of present and potential production, will emphasize the vast array of environments on this planet.

In cooperation with the human adaptability section of the I.B.P., the biological aspects of human ecology can be incorporated into this analysis. A variety of patterns of human behavior and physiology, each in harmony with particular sets of ecosystems, may emerge.

A third major contribution to ecology lies in the programs of several subcommittees, in several countries, designed to study whole ecosystems. Terrestrial, freshwater, and marine areas are included. The management of whole systems has already started, and it behooves us to supply alternatives to the trial-and-error approach as soon as possible.

The quality of the environment is a national issue in all of the industrialized countries. Most of the problems relate to the deterioration of the environment caused by erosion, pollution, and urban sprawl. These causes of environmental decay involve whole systems of human activity as well as whole systems of ecological activity. Systems analysis is already being used in some of the engineering solutions to pollution problems. The development of ecosystem analysis to the level at which it can be incorporated into such solutions will improve greatly the chances of success in such programs.

Although programs of ecosystem analysis will be organized primarily by the advanced countries, the only nations able to afford them, the results may prove applicable with little additional effort elsewhere. These methods may prove invaluable in countries where inefficiency and error may be disastrous.

As a contribution to ecology, the development of ecosystem analysis will provide an exciting stimulus as well as producing a much needed set of concepts and principles. For the last twenty years many ecologists have been frustrated in their attempts to understand ecosystems, or even to understand single populations in ecosystems. The older methods required a simplification that produced designs known to be naive or unrealistic. We are now eager to explore this new approach, which does not impose such limitations, and we hope to see a whole new level of ecology emerge. It is evident already that, in the piecemeal approach to ecosystems we have been using, we have missed some vital components that must be operating, since a synthesis of what we think we know does not produce systems like those we find.

At present, the "game" of systems analysis differs in ecology from its form in other applications. Ordinarily, a known system is simulated, and the effects of various alterations is explored, the goal being an improved profit margin or an

improved stability. In ecology, the system is unknown. We know something of the overall performance, and of some of the components, but we do not know all the parts, or how they are put together. Thus, at least in the early stages, simulation will be used to test whether hypothetical systems produce known outputs. In this way we can guide the analysis of real ecosystems. Eventually, of course, the management of known ecosystems becomes a conventional problem in systems analysis, although an unusually large number of links in the system may not be open to modification.

We estimate the total cost of learning to understand one large ecosystem to be several million dollars. A number of different systems in different environments must be worked out before the subject of ecosystem analysis can be generalized. The efforts of the I.B.P. offer the best hope that such a program can be completed in the next decade.

A final contribution I wish to discuss is an effect of the I.B.P. on ecologists, rather than on ecology. There is, even now, a worldwide shortage of ecologists with modern training. The I.B.P. adds to this problem, since the full implementation of all of its projects requires a doubling of the world's population of ecologists. Training is a prominent and necessary feature of the I.B.P. Interest in ecology has been increasing steadily among graduate students in biology, and the opportunities offered by the I.B.P. will hasten this growth.

Parenthetically, an even more serious shortage in the I.B.P. is the lack of taxonomists in the world. All of the ecological work, from the physiological to the systems level, requires a knowledge of the species present. No more than a tenth of the species in existence have been identified, and in no natural system have all of the species been named. Here, also, the I.B.P. includes an urgent training program.

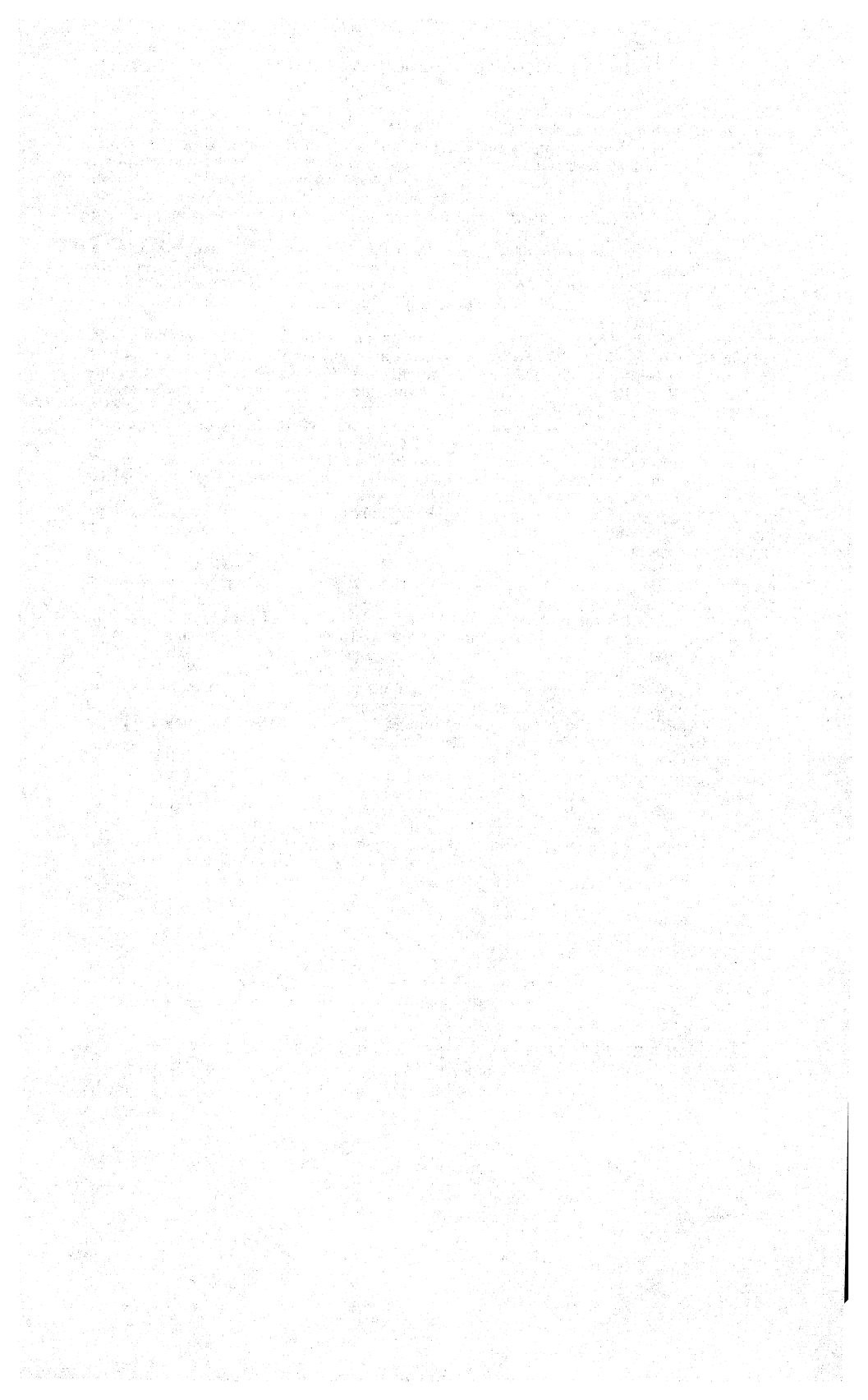
The participation of many ecologists in the I.B.P., and the training of increasing numbers of new ecologists within the I.B.P., will have a profound effect upon the orientation of the profession. The goals of the program are human-oriented, and will promote the acceptance of man as a part of ecology. Ecologists have, in the past, tended to exclude man, and to concentrate on the study of natural or undisturbed systems. Their whole concern tends to be for the welfare of such systems. Their recommendations, when problems arise, tend to favor the preservation of nature, or to favor management programs that optimize only the biological side of the problem. It is not surprising that man, in self-interest, has often chosen instead the recommendations of the economist or engineer, who is trained to optimize the human side of the problem.

Now that man has a serious concern for the environment, the ecologist may show a greater concern for man. This change of orientation has already begun, perhaps in response to congressional interest in ecology. Within the last year the sudden support of the I.B.P. by a large number of ecologists, after a strong initial resistance, has been surprising. The ferment that took place at Williams-town last October had all the excitement of a "happening". Interest in the applications of ecology, and in man as a part of ecology, is very strong among our graduate students. Many of them are motivated by a sense of responsibility for social action.

This change of view produces new optimism that the goals of the I.B.P. can be reached. Large numbers of American ecologists are willing to participate. There is even hope that we can be induced to accept such radical concepts as team research and data sharing. If this is accomplished, the eventual effect of the I.B.P. will be to establish an integrated profession, based on regional centers of study, dedicated both to the development of basic science and to its application to human welfare.

Mr. DADDARIO. If there is nothing further, this meeting will adjourn subject to the call of the Chair. Thank you all.

(Whereupon, at 12:05 p.m., the subcommittee was adjourned subject to the call of the Chair.)



HOUSE CONCURRENT RESOLUTION 273—CONCURRENT RESOLUTION EXPRESSING THE SUPPORT OF THE CONGRESS, AND URGING THE SUPPORT OF PERSONS AND ORGANIZATIONS, BOTH PUBLIC AND PRIVATE, FOR THE INTERNATIONAL BIOLOGICAL PROGRAM

TUESDAY, JUNE 6, 1967

**HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND ASTRONAUTICS,
SUBCOMMITTEE ON SCIENCE, RESEARCH, AND DEVELOPMENT,
Washington, D.C.**

The Subcommittee on Science, Research, and Development met at 10:30 a.m., in room 2325, Rayburn House Office Building, the Hon. Emilio Q. Daddario (chairman of the subcommittee) presiding.

Mr. DADDARIO. The meeting will come to order.

Dr. Ripley, good morning, sir.

Dr. RIPLEY. Good morning.

Mr. DADDARIO. Our first witness this morning is Dr. S. Dillon Ripley, the Secretary of the Smithsonian Institution.

We are continuing our hearings on the International Biological Program, as you all know, and we are happy to have you before us, Dr. Ripley, as we always are, and we are anxious to listen to you.

STATEMENT OF DR. S. DILLON RIPLEY, SECRETARY, SMITHSONIAN INSTITUTION

Dr. RIPLEY. Thank you, Mr. Chairman.

I am very pleased to be here this morning. I have prepared testimony which I would like to hand over to the committee, if I may.

Mr. DADDARIO. You may.

(The prepared statement of Dr. Ripley is as follows:)

PREPARED STATEMENT OF DR. S. DILLON RIPLEY TO THE HOUSE COMMITTEE ON SCIENCE AND ASTRONAUTICS ON HOUSE CONCURRENT RESOLUTION 273

Mr. Chairman and gentlemen of the committee, it is indeed a privilege for me to be invited to testify before this distinguished Committee on proposed House Concurrent Resolution 273. During these troubled times when men and nations are beset with many urgent social and economic problems, it is most encouraging to note the collective effort of the biologists of the world to foster an International Biological Program designed to acquire new knowledge for the improvement of mankind and his earthly environment. House Concurrent Resolution 273, as referred to the Committee on Science and Astronautics, would recognize that the International Biological Program, as sponsored in the United States by the National Academy of Sciences and the National Academy of Engineering, provides a unique means for responding to the increasingly urgent

need for a scientific research designed to improve human welfare in our changing world environment. The biological sciences community of the United States surely would feel heartened by the encouragement that would be given through the proposed House Concurrent Resolution 273 in calling upon all organizations, both public and private, to support and cooperate fully with the United States National Committee of the International Biological Program and its administrative counterpart the Interagency Coordinating Committee.

As the Secretary of the Smithsonian Institution, I feel quite encouraged by House Concurrent Resolution 273. Historically, the Smithsonian Institution with its mission to encourage the increase and diffusion of knowledge has served both the national and the international community of biologists almost from its inception. Our Institution along with other museums throughout the world has assisted biologists and other scientists by providing representative collections of the plants, animals, as well as the mineralogical indices of man's rapidly changing environment. These collections and our small cadre of dedicated scientists provide the national scientific community with a kind of bureau of biological standards or national referral center that is used in many diverse ways to identify and evaluate the changes both natural and man-created that are taking place throughout the world. The Smithsonian Institution has already pledged its full support for the worthy objectives of the International Biological Program and stands ready to assist biologists and other scientists engaged in IBP investigations who need to rely on our national collections and our faculty for assistance in carrying out their individual investigations. Our science faculty is deeply committed to the support of the International Biological Program and is conducting a series of research projects in close collaboration with colleagues located in many countries throughout the world. These investigations include studies of the tropical environment (in many ways, one of the last of man's frontiers on earth), in the marine sciences including both marine biological and geophysical projects in the Atlantic, Pacific and indeed most of the oceans of the world. In addition, our faculty is engaged in efforts to characterize solar radiation in relation to primary productivity on earth. Time does not permit a full description of our many International Biological Program investigations. However, if the Committee desires, I will be happy to provide it with a summary report for the record.

In conclusion, Mr. Chairman, may I express the hope that the important objectives of the International Biological Program as enunciated by the United States National Committee of the IBP will receive the basic financial support so essential for success. Although I recognize that many federal agencies are contributing some support for our national efforts in the International Biological Program, I recognize also that the current level of financial support may not be sufficient to realize the full potential of this exciting and important program. I look back with a feeling of pride in the accomplishments of the United States national program in the International Geophysical Year. Indeed, the dividends from our modest investment in the IGY will continue to accrue for many years to come. It is my sincere hope that our national effort in support of the International Biological Program will receive similar encouragement not only from our Federal Government but from private foundations and scientific institutions in the United States.

Dr. RIPLEY. The testimony concerns the fact that we at the Smithsonian are most encouraged in the collective effort of biologists at least to talk about the International Biological Program.

I must say that I am rather disappointed by the general reaction of biologists in this country to the International Biological Program, and I am also somewhat disappointed by the rather bland approach of this concurrent resolution, which seems to me to reflect the general uncertainty in the minds of both biologists and people concerned with legislation of what the problem really is.

I do not think that, speaking for the Smithsonian, I am prepared to be a hair-raiser or rabble rouser, but I do think that speaking for the Smithsonian I would like to speak for biologists who represent a vestigial link with 19th century biology, and as such are more central

to these problems than most biologists in the country today. I refer you to specific aspects of our program as presented in the addendum to our written testimony.

The realm within which we should be concerned with the International Biological Program is a vast, rather formless, dirty area of biology. It is dirty in the sense that it represents the kind of problem which most biologists do not like, because it is not clean and tidy. The vast concern, the vast thrust, you might say, to use a popular word, of biology today is toward experimental biology which is performed under more or less controlled conditions in laboratories. The vast concern of the biological profession today, then, is to get away from large, dirty, formless, intellectually highly complicated problems having to do with the general environment. And yet at the same time, unless we can concern ourselves with our general environment within which we live, this thin shell, as Dr. Revelle calls it, we will inevitably be called to a reckoning in the future which, ostrich-like, I think most people are tending to avoid.

I would hope that this problem could be thought of in its real context.

Dr. Revelle and others have talked about the population pressures in the world and our consumption of natural resources. These are things which all of us read about in headlines every day in the newspapers. Strangely enough, human nature is such that we do not really get concerned about it until the war is on us, in effect.

I think perhaps that to many biologists this general program seems diffuse. They are concerned with immediate research problems in their own laboratories, they are concerned with their immediate careers, they are concerned with grantsmanship, promotions, activities in their universities or laboratories which are entirely personal to them, and so their reaction to the program is much the same as the man on the street's: That it is something that is not quite going to concern them in their time.

It is particularly difficult to come to grips with broad environmental problems; many of them sound overly intricate, highly theoretical. Some of the ecologists who work on these problems are people who are concerned with such things as mathematical models, and various sorts of stochastic processes in biology which are fringe-like; they are not really central to the main efforts in biology today, and they certainly have no large political voice within the scientific establishment.

I have been, as I say, somewhat depressed by the lack of response and interest of the biological fraternity in general in this really basic and highly necessary program.

I speak for the Smithsonian because this Institution in the 19th century pioneered with other Government agencies in the exploration of our frontiers in the West.

This in a sense is a 20th-century version of that, and the Smithsonian represents one of the few voices in the Government that can speak to this issue and that today is carrying on with frontiers of information about biological organisms around the world.

There is still a vast frontier, particularly in the tropics, which consists of attempting to delineate and understand the populations of organisms, plants and animals, many of them still undescribed and still unknown to science.

Much of biological science has grown away from the attempt to merely tabulate and delineate organisms. The Smithsonian is one of the leaders in the continuing attempt to do this, and to relate this information to ecology and geography. Therefore, to us, it quite readily seems to be a matter of great importance. Just as in the 19th century there was general understanding in the United States that we should undertake to measure and understand the dimensions of our own Nation, our own territory, our own land, so today we feel this is now an international cause: the world has shrunk in size and we must concern ourselves with this.

But how to marshal other biologists, how to get them in general to be aware of the urgency of this?

As I say, the average biologist today is working in a laboratory, in a controlled situation on materials which are essentially repetitive, the systematics or known taxonomic dimensions of which are understood, and almost classically understood. He works on a pigeon or a cat or a bit of a rat, or a bit of a dog or a bit of an insect, and these are all common, garden things that are essentially around him and simply provided as laboratory material. He is not any more interested in knowing about soil bacteria in the emerging nations of Africa than any of you are, but in fact it is, of course, vitally important that someone should know about these things. And this is what in a broad sense this International Biological Program is about.

And until there is some understanding of the urgency of this, I do not see that we have a hope of attempting to assess the problem correctly or to approach it with the proper responsibility from a fiscal point of view.

So that I approach you gentlemen this morning with the plea that you would take time seriously to concern yourselves with the approach which this Nation should take to an area which is rather vast, rather formless, still rather little understood even by the men whose counsel you seek, and which represents a frontier in biology itself, just as much as it represents a frontier in general knowledge, and which we should be prepared to support in the small way that the figures indicate as part of our responsibility to ourselves and to our own population.

Mr. DADDARIO. Dr. Ripley, that is a very stirring message you give. It is one which touches really to the heart of what this committee's concern has been over a course of time, and to which we have directed a great deal of our effort seeking the advice which could bring us to some more definite pattern as to how to make an impact in these areas.

I cannot recall it having been spelled out in such good language ever before, even though we have had a great deal of discussion. You have referred to this resolution as being bland, which causes you concern. Because it is before us to be examined and because we have no commitment to keeping the language of this resolution in its present form, how do we put some salt and pepper to it to make it a little bit more tasty for the whole biological community, and how could we stir up some better appetites for these people who you say are not as involved in these vital areas as they should be?

Dr. RIPLEY. Well, thank you, Mr. Chairman.

I feel that there are two things to which perhaps this resolution should speak. One is that it should urge the biological community in the United States to pay greater attention to the program. There must be ways of making the language a little more hortatory. And the other is there should be some mention of budget.

I think the heart of the matter really reaches to that. As I tried to say in my testimony, the average, let us say, young biologist on the way up in his career profession lives so much by the rule of grants and grantsmanship, whether the money eventually comes from the Federal Government or State programs or foundations, that he is roughly the way the new doctor was, the new medical man, in the 1940's: worrying about his immediate bread-and-butter problems.

The nub of the problem here is represented by the fact that there has been no identifiable budgeting for this program so far, and that means that many of the most active minds among young biologists will think that they have to go where the grants are. And there is something to it, I must say, if they are married and looking forward to a successful career.

There is an inevitable lack of enthusiasm by even the brightest biologists for activities which are not going to be financially or career rewarding. And that is why I called it dirty and formless, because environmental problems are not necessarily those which have an accepted and relatively easy success pattern, as, let's say, molecular biology has at the moment.

It is easy to be in molecular biology, especially if you are very bright, and to have an assured future. It is not easy to be in environmental biology if you are very bright, and to have an assured future. And it is simply a fiscal problem in a sense as well as a career advancement problem.

The average young man today wants to go, especially if he is bright, where his career will be probably the most assured and into an area of research where he will presumably have the most success, the most patting on the back eventually, and the most esteem by his colleagues. And this is a pioneer area, and pioneer areas are no more fashionable among highly skilled, trained professional men than they are to people who have to go out and meet any other kind of pioneer trial. So there is money involved in the lack of success of this program so far.

I think that those two things are perhaps simple explanations of the lack of ability of this program to catch on.

Mr. DADDARIO. Are the private foundations doing work in this area? You mentioned, for example, soil bacteria in the emerging nations.

Dr. RIPLEY. Rockefeller Foundation has pioneered for years in medical aspects of studies abroad, diseases and so on, as you know, and has done an enormous amount to develop a small but persistent tradition for American scientists, ranging from social scientists to medical scientists, to step out of their traditional role as career people inside the territorial confines of the United States and do pioneering work in everything from soil bacteria to various sorts of zoophytes to animals concerned with diseases, micro-organisms, and so on.

This is a somewhat diminishing field. I think their programs are gradually changing and evolving a little bit away from this. I think

perhaps they are hoping that the Federal Government will take more interest in this.

It is to me absurd that we do not, because our involvement in areas around the world is continuing and developing and, as I say, we have now an international frontier which we are concerned with. These areas of the world which we concerned ourselves a little with when the Rockefeller Foundation was beginning and pioneering are now a commonplace to us as the world shrinks, and our citizens are involved in terms of knowledge in Central and South America, in Africa, in Asia, and parts of the tropical world where perhaps we little thought we would ever be involved. And so we should know about this part of the world. It is quite urgent and vitally important, and simply a matter of how you put the pressure on to get things done. It is a matter in which I take great pride being involved with the Smithsonian, as I say, because historically this is the sort of thing we do, although the mainstream of biology has passed us by.

Fortunately, we still preserve these 19th-century traditions of inquiry, on which perhaps much of the future will depend.

Mr. MOSHER. Mr. Chairman.

Mr. DADDARIO. Mr. Mosher.

Mr. MOSHER. If the Congress were to authorize increased expenditures for this type of effort, in which agency budget would the money go? Would it go into the Smithsonian's?

Dr. RIPLEY. We have approached it—the members of the committee of the international biological program have approached it in the first instance, Mr. Mosher, with the idea that the cost would be shared through several agencies, and we would each try to find some money out of various funds which in effect were central to these issues of the program. The total amount thus garnered, however, would scarcely cover initial planning expenses and would not be a source of continuing research funds.

We found in our own budget an amount I think of \$34,000 to \$35,000 which we could ill spare. For being a very small agency this was something quite substantial for us.

The model which perhaps might be more successful than this, which seems to me to be made out of bits of string and not really a very impressive model in this case, would be the International Geophysical Year model in which actually money was assigned in a line-item-like category to a budget for these activities, thus not detracting from other budgets and forcing us to compete with our own missions for these funds.

Mr. MOSHER. It would be difficult to write legislation, would it not, to authorize appropriations that would go into a pool, a little bit from one agency and a little bit from another agency?

Dr. RIPLEY. Yes.

Mr. MOSHER. And I am not a skilled writer of legislation, but I should think this would be very difficult. Wouldn't it, Mr. Chairman?

Mr. DADDARIO. Well, I would think, although Dr. Ripley is here to answer questions, this is a good point to discuss because this has come up during the last set of hearings on this subject. Apparently there is present authority to find funds and then to pool them at the inter-agency levels. That is correct, is it not, Dr. Ripley? This \$35,000 or so

that you have found, because you in a sense delete it from other activities you have going. Somehow all the agencies do the same thing and you do have the authority to put the funds in one place.

Dr. RIPLEY. Right. My understanding was that the National Academy set up this general holding committee, and this is to be administered by the National Science Foundation and funds in effect were to be threaded together in this complex way and made available—rather small funds for preliminary planning. I still do not believe that this is the right way to approach the total program.

I think that there should be a line item of some sort and an approach to funding on its own for such a program in the same way that there was for the International Geophysical Year. This is then not in competition within the agencies for very hard to get small amounts of money.

Mr. DADDARIO. As you look at the whole picture, what kind of funds would it take to do this job in the right way?

Dr. RIPLEY. That is a very hard figure. I would say that if we started off over a period of, I think, a 5-year projection with \$10 million, something of this sort, we would be talking about the kinds of money which would then elicit the kinds of interest which would then be able to focus enough bright minds in biology on how to get the liaison with the foreign scientists, who are very keen to undertake this—in many cases far more keen than we are—to get the program going.

Mr. DADDARIO. You are talking about \$10 million total over the 5 years?

Dr. RIPLEY. No, I would think at least \$10 million a year. You would hardly be able to get going for much less. Any one of the programs which Dr. Revelle speaks about which have already been very extensively thought out by the committee will involve several hundred thousands or millions of dollars to get underway, these sample programs.

Mr. DADDARIO. Have you been able to estimate how much we will be able to pool together in this method presently before us?

Dr. RIPLEY. I am very pessimistic about how much money we will be able to provide at this stage or how much you will be able to invoke the enthusiasm of the other participants to make an earnest effort to provide even more. It seems to me to be a sorry way to look at the programming of this urgent thing that we are talking about.

Mr. MOSHER. Mr. Chairman.

Mr. DADDARIO. Yes, Mr. Mosher.

Mr. MOSHER. Dr. Ripley used the term "grantsmanship," I think.

Dr. RIPLEY. Yes.

Mr. MOSHER. You are implying there, and in your other remarks, that when Congress sees a need in the field of research and development that needs to be fulfilled, and wants to initiate or innovate in some scientific area, the best way we can initiate and give direction to effort is by appropriating funds that are then available for grants, and that these grants will attract scientific talent and stimulate the direction of scientific effort. In general is that necessarily the process that Congress has to follow in its direction of scientific policy?

Dr. RIPLEY. I would think so.

This Congress procedure is about the only one that is realistic nowadays. It has derived from a long history of pioneering by foundations and by land-grant institutions and by States, and has eventually come to be accepted as a major component of Federal involvement. And so the grants tend to be the most effective way of stimulating the research.

And as you know by the writings and discussions of many scientists of the present day, there have been analyses of the necessary self-perpetuating quality of research and the relative popularity of certain areas of research, and the phenomena associated with the total mass of scientific research which has resulted from this kind of grant activity.

I would say, of course, that it has been one of the healthiest things that we could possibly have done, because we have produced an enormous number of keen and highly trained people as a result.

The problem of it is, though, how you distribute this small pool of highly trained end-product people among the difficult areas which still require basic and innovative research.

This is a very serious problem, and here is a perfect example of it, because here is a vast formless area where research should be performed as part of our national responsibility, and indeed to safeguard our population for the future. And yet it is difficult, first without money, and second, because the problem itself is difficult, formless, dirty, as I describe it, to awaken the interest and concern of enough highly trained people.

Mr. MOSHER. Well, Mr. Chairman, I think Chairman Miller came in at just the right time. Because we were beginning to agree that the concurrent resolution, of which you are the author, is bland and rather lacks substance, is rather meaningless, and unless it provides some funds it really does not give much direction or impetus.

Mr. DADDARIO. We have to stir some French sauce into it, Mr. Chairman.

Mr. MILLER. Well, I will tell you, this is just the reason I am here and very happy it is turned over to this committee. I have given you the structure now on which to build and invite you just to put all the hot sauce you want into it, including money.

Mr. DADDARIO. We had already, Mr. Chairman, told Dr. Ripley that it was the responsibility of the committee to take the resolution before it and then to see what needed to be added to it.

Mr. MILLER. That is right.

Mr. DADDARIO. Doctor, following Mr. Mosher's line of thinking, without going into the requirement of supporting our present grant position or coming up with a new way to do it—the admonishment you make to us here is, isn't it, that we at least allow there to be granted in this area that you talk about so that there will be competition?

Dr. RIPLEY. Right.

Mr. DADDARIO. So that a young man will be allowed to look in this direction because there is something for him and not be pulled in other directions because it is not only popular but also being supported.

Dr. RIPLEY. Right; I quite agree with you.

Mr. DADDARIO. Whatever the system might be.

Dr. RIPLEY. Yes. Unless you can somehow attach a tag, a price tag.

or lures, a little bait to it, I do not think this resolution is really going to be effective or helpful.

Mr. DADDARIO. We talk about something now in the order of \$50 million, which is, as we understand it, of course, an estimate on your part. What is the danger to us and to civilization generally if we do not look into these areas? You have used words such as "vital" and "urgent," and not to wait until the war is on before becoming involved. You set our sights looking to the future certainly with a purpose. What are we in for if we do not do something about this?

Dr. RIPLEY. It is difficult to talk about this problem without using well-worn words which perhaps sound exaggerated, like "vital." I happen to feel it is.

I spend a great deal of my time, and have spent, traveling in tropical parts of the world. And the process of destroying the tropical environments is going on at a continually accelerated rate. I believe that under this program there will be certain brakes put on this process. We may even be able to improve some environments in the process.

Potentially, or actually, the conservation section, for example, of the International Biological Program calls for a very sophisticated attempt to document what are known as biotopes, that is, particular types of environment, and to set aside sample areas throughout the world, either in the polar regions or in the Tropics, for future study. This is of—sorry—"vital" importance, because by the time we really get to it, if we do not hurry up, many of these areas will have ceased to exist. There will therefore, as a followup to that, be no opportunity for us to study relatively undisturbed conditions in the world environment which still remain and to tabulate and document the contents of those environments and to derive certain kinds of formulas which may well have to do with applied problems for human kind, ranging from health to various sorts of environmental conditions which tend to provoke stress and so on.

I think this is true. I feel this way, and I feel convinced that this is true. This is perhaps a really significant and important part of the program.

There are other aspects to the program which are of equal interest, and these involve environmental relationships. We in the Smithsonian, as well as others, are particularly concerned about studying environments such as the marine environments on either side of the Isthmus of Panama.

If the proposed sea-level canal is dug by one way or another, there will be a change in the components of the maritime environments on either side of the canal which has not existed for 25 million years. All of a sudden, the Caribbean side of the canal will be invaded by perhaps more aggressive, more active organisms adapted to a rougher and more competitive environment on the Pacific side. This is a phenomenon which we know to exist. This invasion will perhaps change rather dramatically the composition of parts of the Caribbean fauna and flora and may have long-term effects on the whole economy of middle America which we can only guess. But we should at least attempt to study this and find out something about it in advance.

I just happen to believe it is practical. Here is the type of study which could have tremendous—I just cite it as an applied problem—tremendous long-term effects for the United States. We might have to revamp our whole food program and things of this sort.

There are, then, certain types of soil bacteria, certain types of soil organisms of which we still know very little in the Tropics, which may have basic long-term effects on human health and welfare.

There are all sorts of problems in the population explosion of human kind which are tied into studies in the Tropics and elsewhere which could be done by biologists. This is just a small tabulation at random of things which I feel are of great importance for the future.

There is, then, an ancillary problem in conservation. That is, that many species will become extinct without our ever having known that they existed, unless we continue to press forward the kinds of systematic work which the Smithsonian specializes in and which we attempt to build and keep a cadre of responsible scientists concerned with, in joint efforts with other museums and laboratories in this country. This kind of work in systematics, in tabulations, documentation of species, is something which, again to use the word "vital," I feel is absolutely vital to our culture and to our knowledge of the world.

And unless somehow we can continue to support systematics and make it a viable branch of the biological profession, I believe that we will have lost a very significant part not only of our culture but also of our ability to assess and expand our knowledge of our environment. This seems to me to be a vital program.

MR. DADDARIO. Mr. Waggonner?

MR. WAGGONNER. Dr. Ripley, I came in after you had finished your prepared statement, but I came in at a point that you were mentioning the lure, or some bait to attract some people to this program.

That disturbs me because on another occasion when we were hearing about this matter, another of the witnesses testified in generally the same vein, that we needed these grants to ameliorate some of the people who might be involved in this program. You are not taking the same approach are you, that we need this to try to pacify some people who might be interested in this biological program?

DR. RIPLEY. Mr. Waggonner, I am not interested in pacifying anybody, but I am interested in giving some people an incentive, the same way as you must pay a lawyer or a doctor to look into your questions of your will or your health.

If you want to get biologists interested in a program, I am sorry to say that you have to attach some sort of bait to it. This seems to me to be simply a function of human nature. And in order to make the program meaningful, we have to divert the interest and energy of biologists who otherwise have a great deal to do into what they may happen to think, because of public opinion within their own professional field, is a matter of somewhat less significance than the particular type of career that they are following at the moment. This is merely human nature.

MR. WAGGONNER. When you are saying human nature, it is simply that that does not involve any dedication to any particular effort or

results. You can take a man who has been specifically dedicated to one particular field and if you pay him enough money, you can move him.

Dr. RIPLEY. That is a challenging statement to me. I happen to feel that is not entirely true.

Mr. WAGGONNER. But do you admit it is partially true?

Dr. RIPLEY. I think in terms of career setting, for people in a profession it is somewhat true. That is, I remember as a young teacher in biology that most of our bright minds as graduate students went on into medical school simply because they knew they could get married a little quicker and they could perhaps have a little more successful, fiscally speaking, career when they might have wanted to stay in biology, when it was not well paid, when there were no grants and very few fiscal attractions.

Mr. DADDARIO. Would the gentleman yield?

Mr. WAGGONNER. Sure.

Mr. DADDARIO. These grants which we are talking about have been developed through support which as a matter of policy the Congress has funded. Therefore it has determined what fields ought to be supported or not. You are saying now that somewhere along the line we have missed the boat and that there are some areas which deserve support where a vacuum exists. You are asking that this be filled so that at least they can be competitive.

Dr. RIPLEY. I am afraid I am.

Mr. WAGGONNER. I am afraid the chairman put it in much better words than you have, Dr. Ripley.

Dr. RIPLEY. Well, I am delighted. That is the chairman's special prerequisite.

Mr. MOSHER. Mr. Chairman.

Mr. DADDARIO. Mr. Mosher.

Mr. MOSHER. Dr. Ripley has made the point that this type of information and research is of vital concern to our culture. Now you still have to make a case; don't you, for this being a concern where taxpayers' money should be used? You have to make a case for a priority for this in relation to the many other governmental priorities, including Vietnam.

Dr. RIPLEY. Of course.

Mr. MOSHER. Just to use the thing that hangs over all of us. And you do make that case?

Dr. RIPLEY. Yes.

I believe that over the period of years encompassed in this program, this relatively small investment is of equal significance to that of the International Geophysical Year.

We assume that they made a case then and thought that this was a worthwhile thing, and I believe it was.

Mr. MOSHER. Fifty million dollars is relatively small, to be sure, as related to many other governmental expenditures. Yet to the constituents of every one of us here, \$50 million is a tremendous sum of money. We have to justify such an expenditure for the purpose you are suggesting. And I know—even as sympathetic as I might be personally—that it would be quite difficult to try to persuade my constituents that this is of crucial national interest.

Dr. RIPLEY. I believe, Mr. Mosher, there is a general uneasiness in the minds of the average constituent which perhaps includes myself, but there is a general uneasiness about the erosion of living conditions which are cultural, of course, and which are so closely allied to changes in the environment. I believe there is a general malaise, a general sense of concern among our citizenry today about the deterioration of conditions of life, and these are inevitably conditions of the environment, and that therefore it would be relatively easy to make them understand the fact that until we understand what some of the basic components of our environment are we will not be able to correct that condition, if we can at all, and that this is a very central issue and that our citizens are concerned and worried about their children's future, and that in this sense the environmental stresses that we are speaking of here are very close, very much closer than they were a generation ago.

Mr. DADDARIO. Mr. Mosher brings up a very important point, of course, when we are struggling with these funds. But at the same time, isn't there some probability, at least, that the answer to future problems such as Vietnam depend on our having a better understanding about the whys, wherefores, and the living conditions in such areas as the tropics? You are advocating that we develop knowledge of these things before they become climactic in nature?

Dr. RIPLEY. I quite agree, absolutely agree with you, Mr. Chairman.

Mr. DADDARIO. Mr. Brown?

Mr. BROWN. I have a number of questions. I don't know whether it is necessary to go into all of them or not.

You are talking in terms of supporting programs which would focus careers into this area. Now, to what extent are there programs of this sort now? For example, the National Science Foundation, which distribute quite a large number of million dollars per year in grants and so forth, what portion of that funding is going into the fields of biology which might contribute to this?

Dr. RIPLEY. I would have to defer to officials of the National Science Foundation to tell you exactly what proportions of their budget are being used in the area of ecology, for example. I know that they have a section or division on ecology. I don't know what proportion of their budget goes into that. It is a part of their total grant program.

Mr. DADDARIO. Mr. Brown, we could get that for the record from the National Science Foundation. It is a good question.

(The information requested follows:)

1. Does NSF fund grants which can contribute to the IBP?

NSF will fund IBP proposals that fall within the area of responsibility of the Foundation (i.e. basic biology and anthropology) and that satisfy criteria for excellence.

2. What specific grants and contracts of NSF supported research relate to the IBP?

The grants on the attached list have been approved for inclusion in the IBP by the U.S. National Committee.

3. The amount approved and funded for the U.S. National Committee on IBP by the Interagency Coordinating Committee for IBP for FY 1967 is as follows:

AEC	\$50,000
PHS	30,000
USDA:	
ARS	20,000
CSRS	5,000
FS	13,000
Interior	20,000
DOD:	
AFOSR	10,000
ARO	10,000
NIH	35,300
Smithsonian	5,000
NSF:	
BMS	103,800
Anthropology	10,000
Total	312,100

4. For the information of the Committee, the U.S. National Committee for the IBP at the National Academy of Sciences is scanning SIE (Scientific Information Exchange) summary sheets on grants awarded by Government agencies and making a list of research that could be considered IBP-oriented. This list will appear in Report No. 3 of the U.S. National Committee.

Mr. BROWN. We discussed, in connection with the National Science Foundation, the distribution of their funds.

Mr. DADDARIO. We have the whole biological support, but we would have to get it broken down into categories.

Mr. BROWN. Obviously, to succeed, this International Biological Program does require the channeling of professional careers into this area over the kind of time span that we are talking about.

I could be a little facetious and ask if you have perhaps sought through the CIA to get some funds for this, because in areas which they have felt the public would not support they have been willing to support them in the past. Perhaps you could make a connection there.

It seems to me that what is needed in connection with developing a sense of the importance of this program is something similar to what occurred in the physical sciences with the sputnik. You need something that will focus attention and set goals. You have, in your statement, pointed out two or three areas which seem to me to have this kind of importance, but, I am not saying this in any derogatory sense, there has not been created a sense of glamour around these things which would tend to make it important.

For example, just in the field of marine biology, we have had a great deal of attention paid to the general problems of the developments under the ocean. There is the possibility that we have here an environment which can produce a major part of the food for mankind and may ultimately produce an environment in which much of mankind may be able to live under some conditions.

It seems to me that if we could create a feeling for the vast significance of this one thing out of the several that you have mentioned, it would help produce a feeling of need to put some money into it.

Similarly with this matter of the study of the tropical environment. I know practically nothing about this, but I have seen some studies indicating that in the Amazonian rain forest, for example, there are some real problems which exist in terms of our conventional

attitudes toward the development of this for agriculture or other purposes in which we are completely wrong and we don't have the information required.

Why don't we have this information? What can be done to make the most productive use of this vast underdeveloped portion of the world's surface?

Your study here under the International Biological Program should be able to provide answers to this sort of thing.

Dr. RIPLEY. Certainly.

Mr. BROWN. Who is responsible for generating this kind of an intense focus upon the significance of these things that create what you might call glamorous goals which could be used to attract public support and, through that, congressional support?

Dr. RIPLEY. Not a very glamorous body, but a distinguished body, at least: The International Union of Biological Sciences, probably quite unversed in glamour mongering.

Mr. DADDARIO. For a moment I thought you were going to say that undistinguished body of the Congress.

Dr. RIPLEY. I feel, Mr. Brown, that there is an implicit weight of public opinion in this country which could easily express itself in some way in favor of this program. I do not quite know that the position has been explained to them and, as I say, one of the problems is that many biologists themselves for a variety of reasons, partly because of time, partly because of the business of their own careers, find it difficult to divert themselves into an area which is not central to their own career specialties, which involves a self-replicating chain of events in any specialized profession. It is my conviction that this problem is central to the welfare of the American people and that over a period of time this will become evident. Environmental problems are so central to the welfare of every one of us that there should be no basic difficulty of the Congress to relate to the citizenry and to explain the circumstances of concern which are manifest here.

On the other hand, we do have the problem that there is no money for this to speak of, and there is no thrust in present-day biological research toward this particular field of enterprise.

Mr. BROWN. Well, I hate to delay a program of this sort which should be able to stand upon its contributions to basic human knowledge and to the security needs of this country. But in a situation just as in Vietnam—and this may be repeated if we become involved in situations in Central Africa, for example, or in the Middle East, where we are spending today in Vietnam the \$50 million that you are talking about for a 5-year program every 12 hours over there, we could have justified that expenditure for this total program if it had been in effect and produced knowledge about the ecology of Vietnam, because it is costing us far more than that to find out some of the things about the environment in Vietnam, such as the different kinds of diseases, plant life and agriculture which can be supported over there. These things we find all of a sudden are very important to us, and we don't know anything about them.

Dr. RIPLEY. Quite true.

Mr. BROWN. And in many other parts of the world there are similar situations.

As I say, I don't personally like the idea of relating this to the security needs of the United States, because I think if anything it may exceed, in the long run, the immediate day-to-day security needs. But somehow or other, there has to be created on the part of more people an awareness of how this is related to the total needs of our society, including the security needs.

Dr. RIPLEY. Yes. I quite agree, Mr. Brown.

I should point out, Mr. Chairman, that one of the sections of the proposed program, human adaptability, is directly contingent on the ability of anthropologists and others to study human populations still relatively undisturbed, as you might find them in Southeast Asia today, for their kinds of adaptability to the environment before this evidence is lost in the general blending and homogenizing process that goes on with the advent of Western civilization.

Mr. BROWN. Why are they resistant to various stages of malaria, for example, that we are not resistant to?

Dr. RIPLEY. Yes, indeed; and how they are responding to aging processes and so on that are quite different from ours; they are quite aside from the disease factor.

Mr. DADDARIO. Mr. Chairman.

Mr. MILLER. Doctor, I think you could draw some parallel with oceanography here. Just about 10 years ago—and I see a very good friend of mine shaking his head down here—wasn't oceanography in about the same condition that biology is in today?

Dr. RIPLEY. This sort of biology; yes, indeed, Mr. Chairman.

Mr. MILLER. No one paid any attention to it. It found itself in seven agencies of Government, and it has come along pretty well.

I think that the biologists themselves were a little reluctant, perhaps, to get together. Within the general framework of the biological community, there were those who specialized. Sometimes it got to the place this was my specialty and yours wasn't worth a thing, and there was not the unity that I believe is being arrived at today.

I think, when you get that unity within your profession, it will be much easier to do the things that are required, because, unquestionably, this is perhaps the most important phase; this deals with life. And what are we doing in—well, smog relates itself to this problem.

Dr. RIPLEY. Yes.

Mr. MILLER. Pollution.

Dr. RIPLEY. That's right.

Mr. MILLER. What are we doing about those problems?

In order to grow more cotton and better cotton, we use certain insecticides that get into the waters of the Mississippi. What happens to the shrimp? These same pesticides that kill the boll weevil also kill shrimp.

Dr. RIPLEY. Yes.

Mr. MILLER. What are we doing about this?

I just came back from Europe, as did Mr. Brown—we had been over to the airshow, which isn't a show after all. But while there I had the opportunity to talk to a man whom you know, Dr. Edward Perret, our science adviser, telling me about what took place in the oil-tanker disaster. What is this going to do, biologically, to that part of the world? Now the French are concerned because this was one of the places where they got oysters.

Dr. RIPLEY. Yes.

Mr. MILLER. The residue after they get rid of it, what effect is this going to have on the oysters?

Dr. RIPLEY. That is right.

Mr. MILLER. And I must say that some years ago in this country the Atomic Energy Commission filed a report on the disposal of atomic waste on the Atlantic and gulf coasts; they were thoroughly examined by a committee, which I served on. They never filed one on the disposal of atomic waste on the west coast because by that time the fishery people had become alive. It is interesting to note that when in Japan they talked about using big reactors and disposing of wastes, the Japanese contracted with the Picards to make a study for them. They found upwellings of water where they never imagined they would be and which brought this stuff to the surface. As a result no development has taken place over there.

Isn't this what you are telling us, that we have to do to support biology?

Dr. RIPLEY. Very much so, Mr. Chairman.

I am afraid that technology is not enough. And we cannot engineer our environment unless we know what it consists of. And we won't until we get an impetus to understand it.

Mr. MILLER. As I understand it, using a lot of these pesticides that at one time were effective, they become less effective when the earth mutations have taken place, and what is this going to do to the future?

Dr. RIPLEY. Quite true; yes, sir.

Mr. DADDARIO. Well, Dr. Ripley, thank you ever so much. I hope we might be able to forward some further questions to you or contact you for informal discussions.

Dr. RIPLEY. Be very happy to.

Mr. DADDARIO. We appreciate it. Thank you.

(Information requested is as follows:)

SMITHSONIAN INSTITUTION,
Washington, D.C., June 12, 1967.

HON. EMILIO Q. DADDARIO,
House of Representatives,
Rayburn House Office Building,
Washington, D.C.

DEAR MR. DADDARIO: In accordance with your invitation, we are submitting the enclosed addendum to our written transcript submitted to your Committee on Science Research and Development on June 6. This addendum describes the research being undertaken in the various bureaus of the Smithsonian Institution which relate to the IBP programs, and which if expansion were possible, could serve as major contributions in a number of study areas. The talents and experience of scientific staff of the Smithsonian are also being well applied in a variety of guidance functions.

During the hearing several references were made to the International Geophysical Year (IGY). Since that program may possibly serve as a useful pattern for the IBP in some organizational respects, you might be interested in knowing how it operated. Congress provided a grant of money to the National Science Foundation (NSF) specifically marked for support of the U.S. program of the IGY. The NSF contracted with the National Academy of Sciences to set up evaluation panels, review committees, and management councils to determine the relative merits of research proposals and their relevance to the program as established by the National Committee. They also functioned to stimulate interest among the scientists to participate in these program areas. Thus, the Academy served as the scientific and management agency, while the NSF served as the administrative agency. The success of the program was owed in large part to the excellent coordination between the two groups.

Such an arrangement, based on the allocation of a line item is of course the preferred approach. If expediency does not permit this however, a second possibility could be a pooling of funds of agency contributors which could be assigned to a central office (probably NSF). The central office would then grant the money to research projects on the specific advice and recommendations of the U.S. National Committee.

It should be noted that the present pool of monies is for housekeeping and planning activities of the National Committee.

If we can provide any additional information, I hope you will not hesitate to call on me.

Sincerely yours,

S. DILLON RIPLEY, *Secretary*.

SUMMARY OF RESEARCH AT SMITHSONIAN INSTITUTION RELEVANT TO THE INTERNATIONAL BIOLOGICAL PROGRAM

Most, if not all of the biological research that goes on in the Smithsonian is relevant to the IBP as defined by the U.S. National Committee. The Smithsonian Tropical Research Institute, the Radiation Biology Laboratory, the Museum of Natural History, the Office of Ecology, and the Office of Limnology and Oceanography all contribute to the program within their special scientific domains. Several members of the staff are serving either on the National Committee or on various of the national subcommittees for planning and development of the Program. One of our ecologists actually serves half-time for the International IBP in an area of very considerable concern to the Institution generally, that of the scientific bases for halting or at least slowing the appalling rate at which environments of the world are being modified or destroyed by man. Some Smithsonian projects have already been approved as a part of the U.S. program, and others are moving in this direction.

In the Museum of Natural History zoologists and botanists are concerned with biological variation in place and time. Biogeographic studies of community distributions, variation within populations and genetic success are investigated in relation to the macro and micro environments in which plants and animals live. Short-term temporal changes (diurnal, seasonal) and evolution provide another dimension for the development of a valid exploration of the constantly changing balance of nature.

Specific examples of this research include checklists and analyses of the flora of North America, flora of the Neotropics, Reptilian and Amphibian fauna of Latin America, electron microscope studies of the structure of living and fossil Foraminifera in relation to systematics and biogeography, study of mollusks with respect to salinity, temperature, substrate and associated vegetation, serological and ectoparasite survey of North American migratory birds.

This department also administers two IBP-approved handbooks—one a preliminary manual for the identification of mammals in Africa and the second for the identification of reptiles and amphibians in Latin America.

With respect to insect research, a large number of projects are being carried on or anticipated in many parts of the world: Dominica, Southeast Asia, Philippines, and in most parts of Latin America. Some of these are essentially basic inventories of insect faunas, but in most cases are strongly related to ecology and behavior.

Anthropologists at the Smithsonian are working on aspects of human adaptability, one of the principal areas of concern to IBP. The relationship in ancient man between disease and skeletal modifications as it relates to human evolution is a long-time study. Correlations of the form of nose and form and size of face with the level and range of temperature, as well as other climatic factors, over the total range of monogoloid and American Indian racial stocks is also under study.

These are only a few examples of the many concerns of Smithsonian scientists which are currently related to IBP objectives. Many, such as the Floras and Faunas, are concerned with developing baseline information on the kinds of organisms present in various environments and their characteristics. The undergirding nature of this sort of biological investigation was emphasized by the creation of a unique subcommittee in the U.S. National IBP Committee—that concerned with systematics and biogeography.

The Radiation Biology Laboratory carries on studies of biological responses to solar radiation as it can be measured by flowering behavior, pigment synthesis, seed germination, stem elongation, and heat expansion. The biologists in this bureau are also concerned with quality of light in various environmental situations and the influence of latitude on light quality. Also, they are concerned to discover whether or not there is any correlation between the quality of sunlight and the growth and development of plants; projects are presently underway or planned for Poland, Israel and India, using excess foreign currencies.

Research at the Smithsonian Tropical Research Institute is concerned with the ecological and behavioral characteristics of tropical animals especially those on Barro Colorado Island and the adjacent mainland of Panama. The program there also includes studies of fish genetics and distributions and the possible consequences to biological populations of the construction of a trans-isthmus canal.

In the Office of Ecology the following projects are underway or planned:

Development of an ecological research program in Korea in cooperation with the Korean National IBP Committee. The long-range program includes both training of Korean scientists and research and it provides the basis for the Korean National IBP Program. It is aimed at both biological inventory and in utilizing unique opportunities provided by the Demilitarized Zone for determination of basic ecological principles, with special reference to the productivity and wise use of the Korean countryside.

Surveys of ecological needs and opportunities in selected countries of Europe, Africa, the Middle East, and southern Asia. These IBP connected surveys are to provide information on the status of ecological knowledge and the available scientific personnel and facilities, which will provide the foundation for developing comprehensive ecological research programs in each country in cooperation with that country's IBP program and personnel (already initiated).

Establishment of comprehensive ecological research programs, usually involving development of ecological research and training institutes, in representative major ecosystems in various parts of the world. Where possible the base facilities or field research stations will be located adjacent to research reserves, such as that at Guama, Belem, Brazil, and at Ujung Kulon, Indonesia. These programs are cooperative with the local scientists, and carried out within the local IBP programs (in the planning stage).

Ecological research centering on large vertebrates and vegetation succession in southeast Asian lowland rain forests, based on the Ujung Kulon IBP Reserve, Indonesia, in cooperation with the Indonesian National IBP (continuing).

Ecological research into savanna vegetation succession and influences, and into ecology of the populations of wild and domestic mammals, in IBP research areas in East Africa (continuing), and the Congo (planning stage).

Assisting with the initiation, organization, research planning, and staffing of a system of IBP research reserves in tropical southeast Asia (continuing).

Participation in the development of a worldwide inventory of major ecosystems and a system of research reserves to protect for science adequate examples of them; and in development and execution of appropriate researches on them both in the form of biological inventories and studies on the ecological processes.

Assistance with the development of national IBP programs in conservation in developing and developed countries throughout the world, especially in Latin America, Asia, and Africa (continuing).

Ecological research on tropical ungulates, bearing on their conservation (in the case of threatened species such as the Kouprey and Tamarau of Asia) and on their potential use to human populations as sources of animal products (continuing).

Participation in planning, coordination, and execution of research of the U.S. National IBP program in the Conservation of Ecosystems (continuing).

Participation in organization and planning of IBP conferences and other meetings as appropriate. Scientific meetings include ones in Thailand and India (concluded), Latin America (planning stage) and Mediterranean Region (organizing now).

Preparation of "atlases" of environmental information for selected tropical IBP research reserves, to provide the base information needed for IBP ecological researches into basic processes, conservation, marine, terrestrial and freshwater productivity (initiated).

Preparation of a comprehensive IBP handbook and guide to the study of large mammals (continuing).

Assisting with the preparation of a comprehensive flora of the Americas, a long-term IBP program involving a compilation of the identifications, descriptions, distributions, and ecology of all American plants. This is a cooperative IBP program with botanists from countries throughout the Americas. (planning).

Preparation, in cooperation with scientists involved in national IBP programs from many African countries, of a handbook of African mammals. (continuing).

Ecological studies in the Middle East involving wildlife, history of land use, and historical ecology and the impact of human activities (planning).

It should be noted that although some of these above programs or projects have been initiated, almost none are completely funded, and the continuation and development of them will depend upon provision of adequate finances.

Some of "service" facilities for the biological community which will be instituted when funding is available will be a rodent identification center to provide rapid and accurate identification of rodents, important as part of the environment as well as to medicine; a national primate center to provide information and training in non-human primate studies of great importance in terms of health and adaptability of human primates; increased activities and functions of the Oceanographic Sorting Center which assists in the international oceanographic effort by processing biological and geological specimens for scientists throughout the world and through its activities marine collections that otherwise might go unnoticed are made available to systematists everywhere. Thus, collections made under the marine portion of the IBP could be sorted into groups for which there are specialists in various parts of the world, so that they may receive adequate scientific attention. It is also conceivable that a similar service-facility for sorting, and perhaps identification, of terrestrial organisms might be organized, if funding becomes available.

It has been recognized repeatedly within the IBP, both in the National Committee and elsewhere, that all too few ecologists and systematists are available for the studies anticipated by the US/IBP. Extensive training programs at many academic institutions will be required to furnish the necessary manpower, and the Office of Ecology's Chesapeake Bay Center has been proposed as a site for advanced field training of ecologists who have received their academic training elsewhere. However, this will require additional funding to accomplish.

Although the localities in which IBP research will be carried out are not limited, the Smithsonian has proposed to develop a series of standard sites in the tropics around the world. In such sites, teams of biologists, geologists, and meteorologists would collaborate to develop descriptive handbooks of the flora, fauna, the soil, meteorology, and local facilities for research. Such investigations and the resulting handbooks would be carried out with the close cooperation and collaboration of scientists in the countries in which the sites were located. The existence of such sites, it is felt, with so much background data available, would incline productivity biologists to center their studies in these localities. The development of such information, however, is beyond the resources presently anticipated for the Smithsonian.

It should be emphasized that Smithsonian Institution biologists, like those of other institutions over the U.S., will unquestionably evolve projects of critical importance to the mission of IBP when funds specifically designated for IBP research are available. Such funds must also be made available for the training of biologists in much larger numbers than are presently produced by our educational system. The Smithsonian Institution is not willing, but uniquely capable of joining in educational activities which will result in the very large cadre of systematic biologists required for the US/IBP.

Mr. DADDARIO. If I might be able to go out of order here and ask Dr. Blair to be our next witness, because he is from out of town and we do want to take advantage of him while he is here.

Dr. W. Frank Blair is professor of zoology at the University of Texas, and comes here to appear before us as a member of the Ecological Study Committee of the Ecological Society of America.

Dr. Blair, we are very happy to have you with us.

Dr. BLAIR. Thank you, Mr. Chairman. I have a prepared statement which I would like to submit for the record.

Mr. DADDARIO. You may, sir.

(The prepared statement of Dr. Blair is as follows:)

PREPARED STATEMENT OF DR. W. FRANK BLAIR, MEMBER, ECOLOGICAL STUDY
COMMITTEE OF THE ECOLOGICAL SOCIETY OF AMERICA

The International Biological Program is largely a program in environmental biology. Hence, ecologists have been intimately involved in its planning and they see more clearly than any other professional group the urgent need for this program and its importance to our people and to the peoples of the world. Now (as projected under IBP), for the first time in the history of man on this planet the pertinent scientists of nearly all countries are joining together for a unified and coordinated look at man and his environment on a world-wide basis. The objectives of this program are several fold:

(1) to understand the functioning of the complex ecosystems (on land, in fresh water, in the seas) of which man is a part and on which he depends for food and other necessities.

(2) on the basis of this understanding to be able to predict the effects of specific human activities on the quality of these ecosystems. The question is, can we provide for this and for future generations the kind of baseline information that will enable them to maintain environmental quality at a level that will make life worth living.

(3) to identify, around the world, representative stands of ecosystems and unique biota so that samples of these may be preserved for posterity. This is an important aspect of the program, but it does tend to confuse some people about what ecologists do. Ecologists are not people who just want to maintain the environment in a natural condition; instead, they aspire to provide the kind of information that will permit the human population of the world to live in the greatest harmony with the world's ecosystems. Ecologists deal constantly with change and with systems in dynamic equilibrium and which can have their values shifted by change in the pressures on the system. It is the aim of ecology to predict the effect of these changing pressures on ecosystems and hence provide man with a scientific basis for his use of and interactions with these ecosystems. The alternative is continued haphazard alteration of the earth's environments and acceleration of the present trend toward deterioration of the quality of these environments.

(4) to increase the manpower pool in ecological biology, and as a part of the over all picture of international cooperation to increase the competence in environmental biology in developing countries.

It seems unthinkable that the United States, as the technologically and scientifically leading nation of the world could abstain from participation in this program and from taking a position of leadership in it. And, in fact, the United States biologists have taken such a position in the planning stages. Now that the program is approaching the action phase, it is essential that it receive commensurate support from our Federal government if our U.S. biologists are to continue to play an important role in this international undertaking. The reasons why it is in our national interest to participate vigorously in this program are simple and straightforward.

(1) Only the kind of programmatic research such as that being planned under auspices of the PT and PF subcommittees of IBP will be able to provide answers concerning the functioning of ecosystems and hence will provide baselines for assessment of the effects of man-made changes on the ecosystems. Under this particular project all of the complex interrelations of a whole watershed will be studied by many scientists; such an undertaking is beyond the capabilities of any one individual or any single institutional team.

(2) The cooperation of various biologists in diverse countries of the world will permit solutions of basic biological problems that transcend national or continental lines and that would be insoluble without such cooperation. There are plans for inter-continental studies of the adaptation of man to various kinds of environments. There are plans for intercontinental studies of the attributes of the various species of plants and animals (weed species) that are able to colonize many parts of the world and hence become pests from man's viewpoint. There are plans for intercontinental studies of convergent evolution under similar environments to better understand the processes acting to form and maintain the characteristics of ecosystems and of their component species.

(3) The cooperative programs involving advanced countries, such as the United States, and developing countries, such as many in Latin America, will permit the export of scientific "know-how" to countries where it is desperately

needed. As one small example, I have been able, in connection with IBP activities, to put the director of a small limnological laboratory in a Latin American country in touch with a U.S. scientist who is willing to go to that laboratory to teach the people there the use of C₁₄ techniques in the study of primary production.

(4) Much of the international aspect of the U.S. program under IBP is oriented toward Latin America. The integration of the research efforts of U.S. and Latin American biologists and the promotion of training that involves students from both groups cannot help but pay off in increased understanding between these different peoples as well as in increased biological knowledge that could not have been obtained without cooperative intercontinental research effort.

(5) Finally, the IBP effort of the United States can serve as an effective umbrella for many of the proposed activities related to environmental quality and baseline environmental studies with which the Congress is currently concerned. Virtually all of the bills before the Congress that are directed toward problems of environmental quality are directed toward problems that are encompassed by the U.S. plan for participation in the International Biological Program. Strong Federal support for this program is therefore dictated by these considerations as well as by the others that have been set forth above.

STATEMENT OF DR. W. FRANK BLAIR, MEMBER, ECOLOGICAL STUDY COMMITTEE, ECOLOGICAL SOCIETY OF AMERICA

Dr. BLAIR. I would first like to take some issue with my good friend, Dr. Ripley, concerning the present health of the International Biological Program, and I would like to give you a little personal history, if I may, to indicate my involvement in this program.

I have been very much involved with it from the time of the first ad hoc committee to decide whether this country wanted to get involved in this program. On this committee I was a devil's advocate because it looked like a very, very impossible job at the time. This was in 1963.

Dr. Ripley is very right in saying that for a good many of the past 4 years this program has limped along without any particular direction. But within the last 6 months, as we really got into the planning stage, broke down the big committee into smaller committees that started talking about specific research projects, the picture has changed completely in my own view. I think it is not a matter of money to entice people into the program; I think it is a matter of a good program which is emerging—there are several aspects of it that I would like to speak to—which will attract good people into the program. It is attracting good people into the program.

However, this does not remove the necessity for financial support if we expect to do what we set out to do under IBP.

Dr. Ripley mentioned \$50 million. In some of our meetings we talked very seriously about \$80 million in 5 years to put it on a realistic basis if we really want to do what IBP has set out to do.

Also I would like to say that I think that the matter of the present pool money should be made very clear in the record, that this is simply an operational bookkeeping sort of thing. It is not supporting any research and cannot hope to support the kind of research we expect to have going under IBP within a year if IBP can develop as it now seems to be developing.

Mr. DADDARIO. Are you saying, Dr. Blair, that you think the pooling process that we are talking about to get the program off the ground in the succeeding years would then need definite support?

Dr. BLAIR. Yes, sir. The pooling has enabled us to hold planning conferences. There are additional ones at least planned, but it does not support research. It has been very successful in getting people together to make plans for research, but it certainly cannot support the research projects themselves.

Mr. MOSHER. Mr. Chairman.

Mr. DADDARIO. Mr. Mosher.

Mr. MOSHER. Is there any specific reference in the present budget proposals for fiscal year 1968 to this program?

Mr. DADDARIO. My understanding is there is not, Mr. Mosher, but we can check it to make sure.

Dr. BLAIR. Also I would like to say that in my own opinion the most logical place for this money—assuming money does become available as a line item—is the National Science Foundation. The National Science Foundation as the one organization for basic science in Washington would be the logical place for this.

I know there is strategy in perhaps sort of spreading it out but perhaps NSF is the place for it.

Mr. BROWN. May I interrupt at this point?

Mr. DADDARIO. Mr. Brown.

Mr. BROWN. Isn't it quite within the realm of the National Science Foundation to spread this money out through their systems of grants and contracts to provide for its maximum use in connection with this program? This would conceal it, and I don't think we are interested in concealing it.

Dr. BLAIR. No.

Mr. BROWN. But we do want to make sure that it gets out to the stream where it can be the most useful. Isn't it perfectly feasible that NSF can provide this kind of channeling?

Dr. BLAIR. Yes, sir. This is what I had in mind, really.

Mr. DADDARIO. Well, if that is so, it is something we have to take into consideration. I thought this was one of the points Dr. Ripley touched on very well. That it would simply be detracting from funds being used presently in what we feel to be worthwhile enterprises.

Dr. BLAIR. I am thinking of new funds. I am referring specifically to new funds.

Mr. DADDARIO. Although Mr. Brown's point is well taken, the National Science Foundation could swing some of its grant support in this direction.

Mr. MOSHER. Could the National Science Foundation, if it had a line item appropriation for this mission, enter into a contract with the Smithsonian for some specific research?

Dr. BLAIR. I expect Dr. Ripley could speak to that better than I can, but it is my understanding they do this now.

Mr. MOSHER. I don't think Dr. Ripley heard my question.

Dr. RIPLEY. I am sorry.

Mr. MOSHER. My question was: If any new money, say \$80 million or \$50 million for 5 years, became a line item in a Presidential budget, and if it was assigned to the National Science Foundation, are there precedents which would allow the National Science Foundation to contract with the Smithsonian Institution to do a certain amount of work with that money?

Dr. RIPLEY. Yes. But it would have to be maintained as a line item separate. Otherwise, it would be lost and it would not appear as a particular sum of money allocated for this program.

Mr. MOSHER. But you do have working relationship with the National Science Foundation where you use some of their money occasionally?

Dr. RIPLEY. We receive money under contract from the National Science Foundation; yes, sir.

Mr. DADDARIO. Dr. Blair, you talk about this money being best placed in the National Science Foundation. Would you give the National Science Foundation the control over the whole program as well?

You see them as a bookkeeper, or as an actual administrator?

Dr. BLAIR. Presently the administration is through the National Academy. I would see it rather difficult to change this without a very radical operation, and I see no particular reason to change it.

You see, the overall direction of the program, the general coordination, is under a national committee with a large number of subcommittees. But out of these subcommittees there has emerged a group of very specific programmatic operations that are now in the planning stage, and this is why I disagreed with Dr. Ripley's rather pessimistic look at where we are now.

Mr. DADDARIO. Let's go into that a minute.

Dr. BLAIR. All right. I will speak to one that is now in the planning stage. This is a watershed study which will involve a large number of scientists working on a watershed in its entirety, using the whole ecosystem approach.

Dr. Frederick Smith from the University of Michigan—I believe he testified at your earlier hearing—is the coordinator of this program. A large number of people will be participating.

The movement of minerals, nutrients, pesticides, everything else in this watershed will be monitored. It will be probably the first real ecosystem study, the study of all of the interrelations, all of the interactions within or among the organisms that live together in a very broad area. This is an exciting sort of thing. It is something we have been talking about for years. It is something that could not be done unless we had the kind of programmatic approach that we are planning under IBP.

Mr. MOSHER. Where does that study stand for the moment?

Dr. BLAIR. There has been a planning conference in which the ground rules have been worked out. There is a committee, a steering committee. There is—

Mr. MOSHER. It is still merely a concept?

Dr. BLAIR. It is a concept. It needs money.

Mr. MOSHER. It lacks money?

Dr. BLAIR. It lacks money, as do all these other programs. This is one kind of thing that can be done under IBP which could not be done otherwise.

Another kind that appeals to me personally, because I am deeply involved in it, is the kind of international study that could not be done without cooperation of the sort—and again on the programmatic basis—that we can get under IBP.

In November, under the Environmental Physiology Subcommittee of IBP we will hold in Caracas, Venezuela, two conferences to plan cooperative research between biologists in this country and biologists in Latin America. One of these will be directed toward the characteristics of the so-called weed species. I am using this in the sense of the English sparrow as a weed, eucalyptus as a weed; any kind of organism that has been able to follow man around the world and therefore become a pest is a weed in this concept.

We don't know very much about the characteristics of these things that make them weeds, and South America is full of these weeds just as is North America; in many cases, the same species.

We will have South Americans and North Americans identifying their counterparts in planning actual research programs. My own interest is in something related to this: the problems of convergent evolution in similar environments.

This gives us an understanding of how these ecosystems we are talking about have evolved, because in South America we have areas that are very comparable in their physical environment to North America. Many of the organisms that live there are very similar in ecological niche; but the ecosystems may be developed in part from different stocks of plants and animals that have evolved similar niches.

Again we are going to set up cooperative programs with the people in South America. Again this will take money. But I think there is a big spinoff from this kind of effort in simply upgrading biological science in Latin America, upgrading our own relations with Latin American biologists. The relations with South American biologists are fine; the communication is terrible in my experience. The people there are enthusiastic about these programs, and I see a chance to perhaps avoid a possible Vietnam, for example, in South America simply by developing this kind of liaison with these people, this kind of cooperative research. I am not talking about expeditions down there to collect specimens. I am talking about working cooperatively with these people. There is a big difference in their minds.

These are the kinds of things that are in the mill right now and this is why I am more optimistic than Dr. Ripley about IBP.

Now there are aspects of IBP that have simply consisted of identifying on-going research and the Government agencies identifying individual projects by various people and saying well, this fits what we are talking about in IBP so this is part of our IBP program. Well, I think this is pure bookkeeping.

What I am thinking about is these programmatic things that could not be done without a big effort, and these are the ones that will cost money.

Mr. BROWN. Mr. Chairman.

Mr. DADDARIO. Mr. Brown.

Mr. BROWN. Looking at the statement which Dr. Blair has submitted—on page 4, the last paragraph—I think we have a key which, in my opinion at least, can establish the base for justification of the funding when he says that:

The IBP effort of the United States can serve as an effective umbrella for many of the proposed activities related to environmental quality and baseline environmental studies with which the Congress is currently concerned. Vir-

tually all of the bills before the Congress that are directed toward problems of environmental quality are directed toward problems that are encompassed by the U.S. plan for participation in the IBP.

Isn't what we are talking about, then, the R. & D. component of this tremendous program of environmental control which is involved in air pollution, water pollution, ground pollution, and many other things which has become catastrophic in its nature but about which we have lacked any kind of a consistent understanding of the basic factors?

If the IBP will provide the R. & D. component, it could, in itself, save many times the amount of money that would be required to justify the action which is going into these things.

Mr. DADDARIO. Well, the watershed study falls within the recommendations this committee made when it completed its pollution reports last year: That we needed to in fact do just this kind of thing in order to be able to meet some of the challenges which face us.

Dr. BLAIR. Yes, sir.

Mr. DADDARIO. And I think we are talking about baseline environment studies, early warning systems and technology assessment, the side effects, positive and negative, that should come about, all fall within the purview of these recommendations you make.

Mr. BROWN. We are talking about standards for air pollution today, but we cannot establish standards without an understanding of the ecology of the particular area or the whole airshed that we are concerned about. And this seems to me moving in the direction of providing that kind of information to justify the regulatory function of the Congress.

Mr. DADDARIO. Dr. Blair, how do you see the availability of people of the kind who will either come into being or who will be attracted from present endeavors so that this program can be in fact successful?

I wonder if you and Dr. Ripley are so far apart. You are really both talking about getting the kind of support by which you will attract the types of people who can do this kind of work. The only difference is you think you need more money.

Dr. BLAIR. Mr. Chairman, whether we have a successful IBP or not, we are going to be concerned with environmental quality. This problem is with us and I think it is going to be solved. Either way, we are going to face a shortage of trained people.

I recently made an estimate—and I think your staff has a copy of this estimate—of our current production of ecologists in this country. This is sort of a shotgun estimate on the basis of a questionnaire that has a large standard error, but it comes to not much over an average of a hundred Ph. D. level ecologists produced per year.

Now, however the concern for environmental quality develops, this is not going to be adequate, and this in time is going to call for increased training of ecologists. I canvassed the top institutions, of which about 20 produce most of the ecologists in this country, and the universal answer was that naturally if we are to double our production of ecologists it is going to cost twice as much in facilities and faculty, and this is a reasonable estimate.

So the answer is we cannot right now, on the basis of current levels of support to training in this field, adequately supply the demand whether we have IBP or not.

Mr. DADDARIO. There is the foundation upon which you can base this support?

Dr. BLAIR. There is a basis upon which we can build. And one of the problems—Dr. Ripley touched on it—is that there are always glamour areas in science, and the glamour area in biological science in the last 20 years has been molecular biology. They have made some remarkable breakthroughs; they have attracted good people. And in addition, they have attracted the money. And ecology has fared very poorly in terms of the kind of support we need to produce the graduate students we need, the Ph. D.'s, the ecologists we need.

The training grants that have been available through NIH, for example, were not available to the ecologists, or at least essentially not. There has been much more support in these other areas. So I think a part of the picture is increased support in environmental biology.

Mr. DADDARIO. You are optimistic about the work these special committees have done. You mentioned a few of them. I am sure we can get provided for the record the activities of each of those subcommittees?

Dr. BLAIR. Yes, sir.

Mr. DADDARIO. So we can see what they intend to do.

Your optimism about the kind of a program doesn't appear to be diminished in any way by the lack of people who could capably run these programs. Are you optimistic because there are available in some of these foreign countries people who on this cooperative basis can help make this program successful?

Dr. BLAIR. Yes, sir.

In the last 6 months I have made two trips into South America. The first time I went for another purpose, and incidentally started talking about IBP and found such enthusiasm among the younger biologists there that I later went back under the sponsorship of our national committee to simply talk about IBP in some seven countries. And there are many young biologists in Latin America who would certainly contribute to this program. And we would be contributing greatly to the broadening of their outlook if they could be brought into it.

Now, I have equally as much interest and equally as much concern about the tropics and Latin America as Dr. Ripley does. It is of the utmost importance in my own opinion that the kind of biology we are talking about under the IBP be broadened in Latin America.

Now, Latin America traditionally has gone for the medical sciences and law. These are the two respectable professions. The biologists for the most part have gone into Dr. Ripley's field, systematics.

Now, systematics is a base for ecology, but it is not ecology, and there are very, very few competent ecologists in the Latin American countries. There are a few good ones, but there is very little training of ecologists in Latin America.

Now, the problems of Latin America are ecological perhaps just like our own. For example, I was sitting in a meeting the other day where we got to talking about what would happen if we cleared all the forest of the Amazon Basin, and one suggestion was what would this do to the oxygen in the atmosphere. This monstrous green forest

is photosynthesizing and producing a tremendous amount of oxygen, and it could conceivably change the balance of the oxygen in the atmosphere if it were destroyed. We don't know this. But the problems that Congressman Brown referred to of looking at the Tropics in a different way as we consider agriculture and exploitation are real ones. We don't know what one might do by clearing the Amazon forest. It is possible that it will be cleared, but I suspect it would be disastrous.

Mr. DADDARIO. But it is amenable to solution?

Dr. BLAIR. It is amendable to solution, but there is not the competence now in Latin America or probably even here in the United States to do it, to provide the solution, because we have not worked there adequately, as Dr. Ripley says.

Mr. DADDARIO. And this program would be one step in that direction?

Dr. BLAIR. This program would be directed toward such problems—part of it would involve this particular problem.

Mr. DADDARIO. Mr. Chairman?

Mr. MILLER. I was a little bit surprised, Doctor, when you referred to eucalyptus as weeds. I have an experience I would like to relate to you where sometimes people in science throw these terms around and they shock the nonscientist.

I used to be executive officer of the California Division of Fish and Game. We had quite a problem in Carmel country with deer coming. Deer have a great affinity for roses. You know this. So we had a mass meeting to see what could be done about the deer. One of our biologists in discussing deer with all of the good ladies, the rose growers present, said deer are ruminants, they don't eat grass, they browse, they eat weeds. And one woman said, "Yes, but they eat my roses." And he said, "Well, madam, as far as the deer are concerned, your roses are just weeds." And the meeting was all over. Politically and otherwise, we could never lick this statement. Roses were weeds.

Dr. BLAIR. The best definition I have ever heard of a weed is that a weed is a plant or animal out of place. And I think this is probably our working definition of a weed.

Mr. MILLER. Do you agree with him that the roses as far as the deer were concerned are weeds?

Dr. BLAIR. Well, I will admit it.

Mr. MILLER. I am very happy, Doctor, to hear you. I think what you have said and what Dr. Ripley has said merely points up some of the problems that are going to confront us in the future. It is high time we take hold.

I have no other comment.

Mr. DADDARIO. Do you have any further comment, Mr. Brown?

Mr. BROWN. No.

Mr. DADDARIO. We certainly appreciate your appearing before us this morning. Thank you.

Dr. BLAIR. I appreciate the opportunity.

Mr. DADDARIO. Thank you for coming all the way from that great State of Texas.

Our next witnesses are Dr. John Olive and Dr. Theodore Sudia.

Dr. Olive is the executive director of the American Institute of Biological Sciences, and Dr. Sudia is associate director. We are

happy to have them both, and we do understand, Mr. Chairman, that you have some connection with this organization.

Mr. MILLER. Well, I haven't a connection. I have a very great interest in it. I know Dr. Olive knows and the other gentleman sitting over here. I want to welcome him here. He is a very fine and old friend. And without injecting anything else into the subject, we will let it go at that.

Dr. OLIVE. Thank you, Mr. Chairman. We have prepared a statement which I would like to deliver to you at this time.

(The prepared statement of Prof. J. R. Porter, presented by Dr. Olive, is as follows:)

PREPARED STATEMENT OF PROF. J. R. PORTER, PRESIDENT, AMERICAN INSTITUTE OF BIOLOGICAL SCIENCES, PRESENTED BY DR. JOHN R. OLIVE, EXECUTIVE DIRECTOR, AMERICAN INSTITUTE OF BIOLOGICAL SCIENCES

Mr. Chairman and Members of the Subcommittee, on behalf of the members of the American Institute of Biological Sciences (AIBS) I am pleased to present a statement concerning the participation of biologists from the United States in the International Biological Program (IBP).

The AIBS through its 44 adherent societies with a membership of approximately 60,000 biologists, and with about 14,000 individual members, has long been aware of the role biology plays in productivity and human welfare throughout the world. For example, the *Congressional Record* (14 June 1960, A5007) shows that one of our members realized the importance of the many biological problems facing mankind some years ago. At the time he proposed a "biological decade". A radio program on the topic was also sponsored by Georgetown University Forum of the Air. And, most of the members of the U.S. National Committee, and its various Subcommittees, for the IBP hold membership in the AIBS. Thus the important aspects of the International Biological Program are endorsed and supported by the American Institute of Biological Sciences.

We are aware of the great technological developments that have occurred since the Industrial Revolution. But unfortunately with all these advances man has been unable to adjust properly to his environment, although he has come to appreciate more and more that the earth is a closed ecological system.

Ecologists and others have warned for some time that our food and natural resources are limited, and that our casual methods for the disposal of wastes are unsatisfactory for a closed system. Furthermore, if we do not make certain adjustments soon, natural forces will take over, as they inevitably have in the past, and bring about the "biological steady state." As rational human beings we cannot leave the restoration of the steady state to such natural forces as the great equalizers of the past—War, Famine, and Pestilence.

We know that some of the most perplexing ecological problems today relate to (i) how to improve health, education, and commerce—in brief, the standard of living—of people in the emerging countries of the world, and (ii) how to provide food and water for all people on the earth.

To illustrate the significance of these problems a few statistics may be cited. In 1750 the world's population was around 728 million and at the turn of the 20th century about 1600 million. Roughly, the population doubled in 150 years. The United Nations' census for 1960 recorded 3,000 million as the world population, or a doubling in the past 60 years. The increase was 500 million during the 1950's and it was 75 million more people than predicted by demographers. The best estimates now are that the world's population will double before the turn of the century, and again before 2040, unless unforeseen catastrophes interfere or radical countermeasures are adopted. In the United States the birth rate is now declining slightly, and during the past 20 years the rate of growth has not been alarming. But this has had little influence on the world population.

With more than half the people of the world now suffering from hunger and serious disease, the ingenuity of man will be strained severely during the next several years if social and economic conditions are to be improved. The International Biological Program offers one of our few hopes for solving some of the important problems.

Production of food increased in the world from 1950 to 1965 but, per capita, yields remained essentially constant. The Food and Agricultural Organization

estimates that 10,000 deaths occur daily in the world from starvation. Furthermore, with continued growth of population the FAO says it will be necessary to double the world's supply of food by 1980 and treble it by the turn of the century, if even minimal standards are to be met. This is a depressing prediction, considering past production and the increase in population in certain food-deficient nations.

The increased production of world food in recent years has been most notable in calorie-rich crops. From the scientific standpoint, this means that man has constantly pushed the carbon-nitrogen ratio in his diet toward higher values for carbon and lower values for nitrogen. People in the Western nations have compensated for this by massive supplements of milk, meat, and eggs, whereas those of the Eastern countries and Oceania, have resorted to fish to restore a reasonable carbon-nitrogen ratio.

But if improvements and advances are not made soon the maintenance of agricultural productivity will be difficult. Furthermore there is some question whether the current rate of removal of protein foods from the oceans and lakes can be continued indefinitely without proper adjustments in the environment. Perhaps one of the most important contributions modern biology can make to relieve the world's food shortage is in the area of preventing destruction of crops and the deterioration of foodstuffs. Such important matters are under consideration in the International Biological Program.

The earth's total water supply remains relatively stable and the "water crisis" is perhaps more related to availability than to supply. About 93 to 98% of the water on earth is unfit for drinking or irrigation because it is too salty or is frozen in glaciers or icecaps. The usable 2 to 7% that remains is not only unevenly distributed, but man is rapidly depleting it and disturbing the normal distilling hydrological cycle. This is a result of urbanization and concentration of industries in our modern technical civilization. In doing this man has created deserts, waterlogged, or salinized areas larger than all the irrigated lands of the world.

The total available fresh water supply in the United States is estimated to be 700 billion gallons per day. In 1960 we used on the average 320 billion gallons a day, estimates for 1980 are 560 and for 2000 slightly under 900 billion gallons. Thus, it is imperative that something be done about water in this country, to say nothing about areas of the world where less water is available.

We are told that soon nearly 50% of our water needs can be filled by desalting the oceans. But if this procedure is to supply more than the fringes of the continents, tremendous technological developments and sources of power will be necessary to produce and to distribute the enormous quantities of water involved. For example, for each kilo of dry matter in green plants, some 150 to 225 kilo of fresh water is required. In manufacturing, one ton of newsprint paper requires 900 tons of water, and fabricating aluminum or steel demands large quantities of water. To make a ton of synthetic rubber 2500 tons of water are needed.

Studies indicate that man can exist on 5 to 6 gallons of water a day, but in all civilized areas much larger quantities are being used. Modern sewage disposal uses much water, and in the United States the average volume is over 100 gallons per capita per day for personal use. Add to this the other domestic and industrial demands in some areas and 200 to 1500 gallons per capita may be required (Chicago uses 256 gallons a day per person).

The idea of sewage plants and industrial wastes as an integral part of man's food-producing garden may seem obnoxious to many people. But the human race cannot afford to persist in practicing the present disposal system based on a maximum both of river and lake pollution and of water demands.

Intensive work is needed on the possibilities of improving varieties of plants and microorganisms that will grow in the brackish-water areas of the world. Many halophilic or salt tolerant bacteria are known. Some of these may fix nitrogen and alter the mineral and osmotic relationships of salt water so that a variety of plants can be cultivated in these regions.

Some people have advocated increased marine farming by utilizing the microflora of the oceans as a source of food. But Borgstrom and others have emphasized that the productive chains in marine environments are more lengthy than those of the land, and that the potentialities of the sea may be grossly overrated. More research is needed however, in biooceanography because data indicates that the fertility of ocean waters may be increased in some regions.

Man has many enemies in the world, and some agents such as those responsible for plague, enteric diseases, tuberculosis, smallpox, influenza, yellow fever, malaria, and schistosomiasis have been of great ecological importance in the past.

In the Western World controls have been developed gradually for many human communicable diseases. But in the newly emerging nations the developmental stages have been by-passed and measures of controlling disease have been imported in the form of improved public health measures. It took 70 years to halve the death rate in England, 40 years in Japan and only 7 years in Ceylon. Life expectancy on the Island of Mauritius was raised from 33 to 51 in just 8 years after World War II; it took Sweden 130 years to achieve this same increase. As a consequence, the steady state between the available food and number of human beings has been disturbed in the emerging nations, and larger and larger numbers of people are living in a state of malnutrition.

Perhaps the most unique, important, and far-reaching work that is developing in biology now, and which has great human significance, is termed "genetic engineering", "molecular grafting" or "molecular architecture." There is strong evidence that the final secrets of heredity lie in the coiled structure of the deoxyribonucleic acid (DNA) molecule and the complex arrangement of its atoms. The genetic messages of DNA are written out in a four-letter code, each letter being a specific chemical substance. Scientists are able to read the genetic code in some cells and to perform certain unique experiments. As soon as the "reading" becomes more refined it may be possible to give specific genetic instructions to cells or tissues, which in turn will carry out many desired synthetic reactions. Perhaps there may be no limit to such molecular grafting at the nucleic acid level. We may be able to cure some of our defective-gene diseases, such as, hemophilia, by appropriate injection of some relevant piece of DNA, which has been biosynthesized by a bacterium. Several scientists feel that the biochemical mechanisms of tumor tissue can be reverted to a normal metabolic pattern by injecting normal cellular DNA along with antibodies specific for tumor cells. The antibodies would retard the growth of the tumor cells, so the normal DNA could adjust the cell to its regular pattern. Such speculations may seem fantastic, but they are distinct possibilities.

From these brief remarks I have tried to illustrate that new biological knowledge is a necessary prime natural resource of all mankind, regardless of national boundaries. The International Biological Program provides an excellent opportunity to expand our knowledge concerning the most elementary of all human needs—food, water, and health.

We hope that in the near future the President and the Congress will consider a special line item budget for the United States' portion of the financial support of the International Biological Program. Recent discussions within your Committee indicate that to maintain a healthy science in this country, increased financial support of 10-15 per cent each year is necessary. These goals are not being achieved. Thus it appears that it will be impossible to superimpose an extremely important five-year International Biological Program of \$50-\$100 million on agency budgets that are now being strained to the utmost.

Although scientific inquiry needs more and more help from governments if essential achievements are to be reached, there is a danger in the situation if a growing proportion of research is officially directed. Part of the great success of the International Geophysical Year may have been due to the relative financial independence of the organization. We hope that sufficient flexibility can be maintained in the International Biological Program so that proper adjustment can be made if its tempo is impeded in any way.

May I conclude by thanking you for the opportunity to make a statement, and even more for the willingness of your Committee to endorse Concurrent Resolution 273.

STATEMENT OF DR. JOHN R. OLIVE, EXECUTIVE DIRECTOR, AMERICAN INSTITUTE OF BIOLOGICAL SCIENCES, ACCOMPANIED BY DR. THEODORE SUBIA, ASSOCIATE DIRECTOR, AIBS

Dr. OLIVE. On behalf of the American Institute of Biological Sciences I wish to thank you for the opportunity to appear before the House Subcommittee on Science, Research, and Development. And my remarks are brief, and a partial summary of what we have in our written statement.

We wish to address ourselves, then, to some of the concepts which are contained in the international biological program. Dr. Sudia has already been introduced as the associate director of AIBS. He is also an ecologist and plant physiologist on leave from the University of Minnesota where he has been actively engaged in biological research, at an international level.

The American Institute of Biological Sciences has a membership of 44 adherent societies. That is to say, our organization is a federation, of learned and professional societies. The institute also has an individual membership. We are therefore directly or indirectly representing about 60,000 biologists in this country.

The AIBS has as part of its stated purposes, "to further the advancement of the biological sciences and to cooperate with local, national, and international organizations concerned with biological sciences." It is therefore appropriate, that we are wholeheartedly behind the effort of the IBP. And we have endorsed the program since its inception.

Actually, some of the roots of the international program were developed within the framework of AIBS at a very early time. Actually, there are statements in the Congressional Record going back as far as 1960 when this was indeed an embryonic program, and from those roots and others have developed the program as we know it today.

The gentlemen before me have touched upon the concept of rapid technological developments. Indeed, these developments have moved very rapidly since the industrial revolution, and that unfortunately some advances have outdistanced man in being able to cope properly with his environment. Another way of saying it is perhaps he has "painted himself into a corner," and now has these very real problems facing him.

We must, it seems to me, make adjustments of these natural forces before the natural forces catch up with us. Our planet as we know it as a closed ecological system and, there are only so many natural resources, so much atmosphere, and these are confined to the "envelope" in which we move about. I would place the world population problem at the top of the list, as the number one problem facing man today and the explosion can be heard round the world.

One of my colleagues defined the population explosion as being every bit as dangerous as an atomic weapon or explosion, the only difference being that it had a longer fuse. With more than half the people in the world suffering from hunger and serious disease, the ingenuity of man is going to be seriously taxed during the next few years. We envision the International Biological Program as one of the few hopes for solving these very important problems.

I agree with Drs. Ripley and Blair that the interaction of scientists becomes one of the most important things that can come from an international program. We are concerned with the mobility of scientists, actually being able to take them to various parts of the globe and to grapple with the problems that are facing them. It spills over into the teaching and the training of biologists, wherever they may be, and to early identifying individuals that can interact.

The program that Dr. Blair mentioned a moment ago, that of pulling people together in conferences, is likewise endorsed by the AIBS. We

know that it is an important stride toward solving some of the problems, once they get together within the framework of an organization.

I might mention the Food and Agricultural Organization estimates that about 10,000 deaths occur daily in the world from starvation alone. Furthermore, with continued growth of population, FAO says that it will be necessary to double the world's food supply by 1980, and to triple it by the turn of the century, if even minimal standards are to be maintained.

I would submit that this is a depressing prediction, considering past production and increase in population in certain food-deficient nations.

Water supply has been mentioned before this committee, and I think it is only fair to bring it into focus one more time by saying that the earth's total supply remains relatively stable and the water crisis is perhaps more related to availability than supply.

About 93 to 98 percent of the water on earth is unfit for drinking or irrigation because it is either too salty, is frozen in glaciers, or it is actually not potable water. In many instances, one man's sewage becomes another person's drinking water—a very dismal concept. So that the 2 to 7 percent that remains is not only unevenly distributed but is rapidly being depleted, and the normal hydrological recycling of water is being upset.

Of course, as was pointed out before, much of this is related to urbanization, concentration of industry in our modern technical civilization. In doing this, what has man done to his environment? He has created deserts, waterlogged in some instances or salinized areas larger than all of the irrigated lands of the world.

Again, a major problem which is confronting us.

About 700 billion gallons of water are available today. And we figure that—well, in 1960, for example, 320 billion gallons were used, and estimates by 1980 go to 560 billion. And for 2000, slightly over 900 billion gallons of water. Thus it is imperative that something be done about the water of this country, to say nothing about areas of the world where less water is available. In other words, we are saying here that the available stock of water is going to be outstripped by the turn of the century.

Another problem area. Intensive work is needed on the possibilities of improving plants and micro-organisms that will grow in brackish-water areas of the world. Many salt-tolerant bacteria are known, for example. Some of these may fix nitrogen and alter the mineral and osmotic relationships of salt water so that a variety of plants can be cultivated in these regions.

We are looking, then, to aquiculture, or a type of aquatic farming that has been envisioned and stated many times before. However, this is probably not as simple as it seems, because it is emphasized that the productive chains of the ocean, the actual organized organisms utilizing other organisms, may be longer than we know at the present time, and certainly are more lengthy than those on land. The potentialities, then, of the sea may be grossly overrated.

More research is needed in the area of marine biology because data indicate that the fertility of the ocean may be increased in some areas and are poorly known in most areas of the ocean.

In summary, I wish to say that the International Biological Program does indeed provide an excellent opportunity to expand our knowledge concerning the most elementary of all human needs: Food, water, and health. We hope that in the near future the President and the Congress will consider a special line-item budget for the U.S. portion of the financial support of the International Biological Program.

Recent discussions within this committee indicate that, to maintain a healthy science in this country, increased financial support of 10 to 15 percent each year is necessary. These goals are not quite being achieved. Thus, it appears that it will be impossible to superimpose an extremely important 5-year international program of \$50 to \$100 million on agency budgets that are now strained to the utmost.

Although scientific inquiry needs more and more help from Government if essential achievements are to be reached, there is danger in the situation if a growing proportion of research is officially directed. Part of the great success of the IGY may have been due to the relative financial independence of that organization.

We hope that sufficient flexibility can be maintained in the International Biological Program so that proper adjustment can be made if its tempo is impeded in any way.

I wish to conclude by again thanking you. Furthermore, to commend the committee for its willingness to endorse Concurrent Resolution 273.

Thank you.

Mr. DADDARIO. Dr. Olive, on the matter of financing.

Mr. MILLER. Mr. Chairman.

Mr. DADDARIO. Yes, Mr. Miller.

Mr. MILLER. I have an appointment downtown and have to leave. Would you permit me to say that I appreciate the testimony Dr. Olive has given. I want to commend him for it. I think he has given us a great deal to think about. So if you will excuse me, I will leave.

Mr. DADDARIO. Thank you, Mr. Miller.

Dr. OLIVE. Thank you, Mr. Miller.

Mr. DADDARIO. Dr. Olive, you have agreed in total amounts of dollars needed for this program with the other witnesses who have appeared this morning, falling within the range for the 5-year period of \$50 to \$100 million.

Dr. OLIVE. Yes.

Mr. DADDARIO. You all seem to jump, 50 to 80, to a hundred. So we are not doing too badly.

But I do think it does give us a good idea of what you are all thinking about the funding requirements necessary to carry out a meaningful program.

Now, you then go on from there to talk about how this is to be handled. If these funds can be obtained, where should they be placed? And once they are there, should that agency or those agencies in some way perform an additional function of being the supervisor of these programs, or do you contemplate it being an accounting procedure really and that the control would be somewhere else? How would that work?

Dr. OLIVE. Well, it is probably a portion of the latter; that is, the latter part of your statement. As I intimated I believe that probably the

success of IGY was the fact that it did have this pool of money available to it.

As you know, the IGY was launched from a fairly small and simple operation. I mean by that the science with which it dealt was not as complex as biology. Furthermore, the IGY did not have to grapple with the very complex structure of an international organization. I feel that this is one of the real headaches with which the IBP is going to be faced, the complexity of its own structure.

Certainly, as Dr. Blair pointed out, progress is being made as a result of splitting and splintering off into subcommittees. The actual community of biologists is rallying behind this concept now. But I am still not speaking to the point of where this fund or pool of money should be—where it should actually rest. I would defer this question to those who are better qualified in understanding financial structures as large as this one will hopefully be.

Mr. DADDARIO. Dr. Blair suggested—and I am sure we are not going to hold him to it, but I do think it is helpful for discussion—that the National Science Foundation would be the logical place from which these funds could be obtained for the purposes of supporting this program. But he also then indicated that the control ought to be in the National Academy. Your statement seems to indicate that there also be a detachment between the funding and the control.

Dr. OLIVE. Yes. This is essentially our concept. But exactly where this would rest, is something that would have to be thought through very carefully.

Mr. DADDARIO. Now, do you also share with Dr. Blair the pessimism which he held in the first instance as to how this whole program could be put together; and that since these last 6 months the progress which has been made through the breaking down of the committee into smaller committees has begun to put together studies and suggestions, and that through this process we can, in fact, develop, if funding can be obtained, a successful international program?

Dr. OLIVE. Yes, Mr. Chairman. I would endorse this concept heartily. There is one thing I believe that is “going” for the program now which did not exist at the beginning, and that is the fairly rapid dissemination of information about the program. I know, going back no more than a year or something to this effect, the “man-on-the-street biologist” knew little or nothing about the IBP; that it was only a term which he had heard, but he had no idea as to its structure or even its objectives.

We have been able to help in our own organization by putting out information through the pages of our journal, and we find that we are indeed getting inquiries which we have in turn referred to the international committee. I find that there is a rallying round the total concept.

Mr. DADDARIO. Now what is your view of the manpower availability to staff these programs in the right way, both domestic and foreign?

Dr. OLIVE. Well, I would certainly have to agree that there is the great shortage of ecologists, but I would be a little bit more optimistic in saying that there are some foreign ecologists available.

Mr. DADDARIO. How do you see this thing working? Dr. Blair has suggested that the programs be cooperative programs which would

not involve large expeditions to, let's say, South America, and that this would not only accomplish the objectives in the right way but that it would not create any antagonisms. How do you see this working in that type of relationship?

Dr. OLIVE. I used the term "the mobility of biologists." In other words, if there were sufficient funds to simply get groups of interested individuals together, I believe that the program would move and move rapidly.

I find in my travels, as Dr. Blair does, that certainly there is a lot of enthusiasm. I find this possibly more on the other side of the oceans than we do here right at the moment. But then when you get back and start comparing notes with colleagues and saying that these possibilities exist, you find that there is this enthusiasm developing. If many small conferences could be organized, I think we would see the IBP actually bloom.

Mr. DADDARIO. What have you got to say, Dr. Sudia?

Dr. SUDIA. I think it might be interesting at this point to indicate some of the things that my home university is doing with respect to providing opportunities for scientists to do research abroad. And I think that in part this is going to affect the way the IBP works.

At the present moment it is possible to be a staff member on full-time service at the University of Minnesota and at the same time carry on a research program at some other part of the world and do this while not on leave, but do this while on university duty in some other part of the world. The problem in doing this at the present time is to find adequate support and be able to convince the granting agencies that one is really not skylarking in Mexico or Peru and that one doesn't really want to go down there every 6 weeks because it is winter in Minnesota, but because there is corn growing there that can't be found anywhere else in the world.

I would say at the present moment that, due to large foundation, teams are going to Europe and other places to go to work with foreign colleagues, on problems of common interest. I think that from this small start, using this kind of experience as a base, that the IBP could not only stimulate this activity, but actually carry it to fruition.

In my meager experience with this, it is possible to get something of a time scale. In some of the early discussions a biological decade was talked about—not an international biological program, but a biological decade. I think realistically this is about the right timespan. If one is really going to get interested in a program involving international cooperation, in Latin America or Africa, or perhaps even Latin America and Africa, that one is committing a 10-year portion of his life.

My personal conviction is that there are many people who would like to do this, but at the present moment, because of the lack of direction and the lack of impetus from funding, that it is indeed very frustrating. I have the further personal conviction that it is possible to carry on research in certain areas—which could be mutually beneficial to the countries involved—and to have a mutual exchange of students that are trained in both countries. It is common to think of foreign students trained in the United States. I think it is an equally valid concept to think of American students trained abroad. I think

that with this viewpoint, it would be possible to develop a concept of international biology which, in a decade, would far exceed even our dreams right now. I think this is a very important subject.

Mr. DADDARIO. Then you see this international program as one which would give the necessary impetus so there will then develop the dedication within which a person will be able to work, if not for 10 years, for a period less, or most likely for a period even longer?

Dr. SUDIA. There were some hints this morning about some of the problems of working in the area of international biology. I happen to be interested in ecology and in plant physiology as well. In our present publish-or-perish attitude toward research and promotion, one has to be realistic in appraising what kind of a contribution can be made within a timespan, in biochemistry or in plant physiology or in some of the more analytic laboratory sciences, as opposed to the ecological sciences. I think that one—in almost all of these cases—one is talking about a considerable expenditure of effort, time, money, working on subtle interrelationships before one ever is near the typewriter for publication purposes. I believe that this type of thinking has to be built in, when one is talking about such things as annual reports on this kind of a subject.

We should talk perhaps about biennial or 3-year reports instead. The sort of things where the cream can be skimmed in international biology, I don't think really exists very much any more.

The purely descriptive study of areas, I think we know very well how to do. Now to get into the subtle interrelations of why corn grown at 12,000 feet won't grow at 3,000 feet in Peru or why certain varieties will grow only within a 50-mile radius of the Cuzco Valley. But these things are going to take a considerable amount of time and perception. The kind of unhurried attention that one simply does not find in a publish-or-perish atmosphere. I think that an IBP which specifically was dedicated to medium-to long-range studies of the biology of the world would accomplish this proper environment in which to pursue this work.

Mr. DADDARIO. How does this enthusiasm generate itself and how have the foreign countries reacted to this international program?

Dr. SUDIA. I think there are others in the room that can answer the question far better than I can. My off-the-cuff opinion is that we have a great deal of enthusiasm but less support. I may be wrong on this.

Mr. DADDARIO. In which area?

Dr. SUDIA. That is in the concept of the IBP itself.

Dr. OLIVE. I would concur, Mr. Chairman, that actually I think it is rather frustrating to us—I mean by “us,” American biologists—to find that we were coming lately in this program; that it had moved rather vigorously in England; for example, and I believe, am I not right, Dr. Blair, that our committee was the last of the national committees to be appointed?

Dr. BLAIR. Of the large ones, probably. The Latin Americans were the last.

Dr. OLIVE. That is what I mean, the large countries.

Mr. DADDARIO. Do you have any thoughts, Dr. Blair, on where the foreign biologists stand from both enthusiasm and from actual programming? Are they ahead of us, or behind us, or is it all at about the same stage?

Dr. BLAIR. In general the European countries are ahead. Now there is a very good reason in the countries of the world. Particularly the Eastern European countries, like Poland, where everything is set up under an institute and the director of the institute says we are going to set up a program for IBP and they immediately start work so they are about 2 years ahead of us. So Australia has a program, and in all there are 55 countries, I believe, now do have programs under the IBP. The last ones behind us were the Latin American countries.

The one purpose of my trip last January was to stimulate enthusiasm, and they now have in Brazil, Argentina, Chile, and probably Peru, national committees, and three and possibly four of the seven countries I have listed now have committees. And they are quite enthusiastic, the people I have been corresponding with.

Mr. DADDARIO. Does this enthusiasm find itself connected with financial support?

Dr. BLAIR. It does not. And I feel this very sincerely. It is a matter of feeling that we have been cut off from you people in the United States in terms of communication. And I will admit that American dollars have been mentioned a few times, but for the most part I really sincerely believe it is just a matter of "we would like to work cooperatively with you people." And in one country, Brazil specifically, I know there will be some program which the President of the National Research Council indicated he would support.

Mr. DADDARIO. I would assume that our part of the participation not only is important to give our scientific people direction and overall support for the program, but because it will stimulate them to get some additional funding and attract additional people as well?

Dr. BLAIR. Yes, sir; I feel this very definitely. And this has happened to a lesser degree in Argentina.

Mr. DADDARIO. Thank you, Dr. Blair.

Dr. Sudia, I didn't know if you had finished when I asked that question.

Dr. SUDIA. Essentially so; yes, sir.

Mr. DADDARIO. Mr. Brown?

Mr. BROWN. Mr. Chairman, as I listened to the presentations this morning, it comes to my mind that this may be a step in the solution of a larger problem of which I had previously been thinking. In western industrialized countries for 50 years or more we have tended to concentrate on the so-called hard sciences, engineering, physics, mathematics, and so forth, at what seems to me to be the expense of the ecological and environmental sciences. We should perhaps be thinking in terms of a framework in which this is not just an isolated event or a 5-year or 10-year program, but a movement toward securing a better balance in the status of human knowledge. If we look at it in this framework, perhaps we ought to be thinking in terms not only of a larger role through the National Science Foundation for the stimulation of additional professional competence in this area, but as is true of the other sciences, the physical sciences, for example, providing a greater amount of support through some of the mission-oriented agencies such as the Department of Agriculture or the Environmental Services Administration for basic research and development related to their particular missions which would be coordinated with the

stimulation provided by the National Science Foundation for the development of manpower and other types of basic activities which would lead to a fundamental shift in the allocation of resources in these areas of science.

Mr. DADDARIO. Well, that certainly is a very good suggestion. Their involvements—they could immediately participate—would be extremely important.

Mr. BROWN. Of course, the function of this type of program is to focus attention upon the importance of this, which in effect will have the very beneficial effect—since you never get anything off the ground and get a lot of money put into it initially—the best program in the world doesn't start out with a lot of funding behind it; it starts out with no funding, and it draws the funds and it draws the attention because in looking at it you begin to see that it has a relationship.

I am impressed by a very modest example which has developed in the physical sciences over the last 5 or 6 years only, and that is the research in the field of lasers, which has gone from nothing up to something like \$50 million a year because it seemed to be a fundamental breakthrough in a particular area.

We are talking about a total sum here which is less than we spend on this one very esoteric field of physical science research.

Mr. DADDARIO. Dr. Olive, there was one question I wanted to ask you. Even though you lay stress on the idea that this ought to be a line item somewhere, would you agree that we could get it off the ground by pooling agency funds as it is presently contemplated for the first year, but that we ought not to look at that as being the solution for the entire 5-year period?

Dr. OLIVE. I would agree with this as a distinct possibility. In other words, as you say, to get it off the ground, and looking toward the future. I would endorse this concept.

Mr. DADDARIO. The reason I bring up this pooling again is that the funds in the first year probably would not be as high as they would be as you went along. If this should be able to be arranged; it would allow us to be committed and give us an idea as to how we ought to be able to fund it over the period of time remaining.

Dr. OLIVE. Subsequent years.

Mr. DADDARIO. Well, I certainly think we have come a long way today. The resolution has been referred to by several as not being broad enough in scope. We had no idea at first as to whether or not it was. We have had a great deal of information now about the size of this commitment and what it can do. It is certainly an exciting possibility, and it does fall in with many of the concerns this committee has shown over these last several years. We certainly are enthusiastic about it. It is our responsibility from the policy point of view to see how it can best be put together. And we hope that as we begin to get to that point we may ask you for advice through the submission to you of formal questions or by some informal discussion or other.

Dr. OLIVE. We stand ready to assist in any possible manner.

Mr. DADDARIO. Thank you.

Dr. OLIVE. Thank you.

Mr. DADDARIO. Mr. Yeager?

Mr. YEAGER. Mr. Chairman, we have a prepared statement submitted by Raymund Zwemer, former director of the American Fed-

eration of American Societies for Experimental Biology. I wonder if we could have it incorporated at this point.

Mr. DADDARIO. Without objection, that may be incorporated. And we will keep the record open for additional papers as we may decide they should be included.

(Information is as follows:)

PREPARED STATEMENT BY DR. RAYMUND L. ZWEMER

Legislators should recognize the importance of the service they can perform for the United States by endorsing this resolution. It will provide a unique and effective means of meeting the urgent need for increased study and research on biological productivity and human welfare. These are being studied in many countries and by many agencies, but now we have an opportunity, with the multilateral support of the International Scientific Unions, to bring these efforts together in a more effective way. The non-governmental international scientific unions need the support of both interdepartmental organizations and of bilateral programs. The funds for each of these are controlled by the legislators of the participating countries. They are the ones who will determine the degree of support and the channels through which this support will be made available. From my own experience, I believe that international cooperative programs can be less expensive for the United States and longer lasting in their effect. Beneficial effects of biological productivity occurred with the introduction of Zebu strains of cattle from India into our cattle raising states, improving drought and disease resistance and meat production. On the other hand, there is the illustration of lack of biological control when with the opening of the Saint Lawrence Seaway certain predatory ocean eels were introduced into the Great Lakes which interfered with both commercial and sport fishing. Only recently has there been a trend toward better control. We do not know what might happen to ocean populations with a sea level canal between the Atlantic and the Pacific Oceans. At present the populations on each side of the Isthmus of Panama are kept apart, although possibly some specimens are transported by adhering to the hulls of ships passing through the Panama Canal. There is the need to add knowledgeable biologists to engineering projects such as space age exploration, in order to predict the destiny of life forms carried to the moon and eventually the planets or back to the earth. Perhaps there should be more effective consultation with biologists in making new ocean, lake and river connections or even locally in planning the building of an atomic power plant on the Patuxent River.

Sometimes the biological control mechanism is unknown and research has to be done to find the natural enemies of living things that have reproduced too rapidly in a new or protected environment. There is the well-known case of the rabbit introduced into Australia which became a pest but is now controlled by the introduction of a disease. Another example is that of the giant African snail which was introduced to the Pacific Islands as an item of food by the Japanese. When they left the islands in the latter part of World War II, the native inhabitants of the islands did not care for snails as food and as a result snails multiplied so rapidly that they devastated the garden products. Under a special research program, U.S. scientists were sent to Africa to find natural enemies; one proved to be a much smaller snail and the other a parasite. When introduced into the islands, these brought the multiplication of the giant snail back to within controllable limits. In the desert country of the Middle East, the camel and the goat because of their eating habits uproot the plants thus contributing to producing deserts. On the other hand, animals that bite off the fodder leave the roots to provide new plants for later forage. A very simple experiment was performed using a peninsula projecting into the Mediterranean from the Egyptian Western Desert. A strong fence was erected to keep out the wandering camel and goat. This was enough to show that in their absence, many plants could grow on desert soil. A program was then initiated to provide fencing in order that the former nomad inhabitants of the Western Desert could grow food and raise grazing animals, if they could be persuaded to change their living habits. During a ride from Cairo to Alexandria on the desert road bypass, I was shown with pride, mesquite trees which had been imported from the U.S. southwest. These provided the only available shade and seemed to be resistant to the conditions of that area. In another case the introduction of a new food encountered different customs and behavior patterns. During World War II, when the Chinese gov-

ernment was driven out of the rice raising country into the mountains, seed potatoes were flown in over the Burma hump and good crops were grown. However, difficulties arose because of the inability of the population to eat boiled or baked potatoes with chop sticks; as soon as rice became available, they reverted to its use.

Genetic selection is not new. Over thousands of years man has not only domesticated many animals but by breeding practices has developed Arabian horses, mules, caracul sheep, the milk cow and dogs for a wide variety of uses. The story in connection with plants is similar with the development of the food grains such as wheat in the Mediterranean countries, rice in Asia and maize in the Americas. In relation to the human welfare, it has been the practice to inbreed ruling families since ancient times. The Chinese adopted the slow but sure way of maintaining their 3,000 years of continuity by use of genetic absorption of their conquerers. This can take place more rapidly in communities that permit customs that result in rapid population increases.

As foreseen in the planning, the International Biological Program will study the local conditions so that similar areas of the world can be furnished plants and animals where the soil and climate are most suitable. Similarly, there are other programs that need to be carried out simultaneously on a broad international scale in order to determine world-wide conditions of certain specific biological phenomena. In the mid-fifties it was discovered, after the fact, that if during the previous year scientists had known more about the breeding habits of that scourge, the locust, it could probably have been for the most part eliminated. In that year only a few local areas existed which would permit the locust to reproduce.

In my testimony I have endeavored to provide a few practical illustrations of the many ways in which proper study of biological material can result in greater productivity. This in turn, with adequate research, could be greatly beneficial to human welfare. It is also essential that the Congress, in calling upon Federal departments and agencies to support fully the program, should bear in mind the fact that bilateral programs with extensive funds should also be brought into this cooperative effort. The production of better plants and animals, reduction in the wastage of food stuffs once they have been produced, adequate distribution for human welfare are in our national interest. Areas where extensive surpluses needed elsewhere can be produced should be encouraged to do so with provision for proper distribution so that the producer will not be penalized for the excess production but instead have the satisfaction of seeing his product channeled into areas of great need. United States participation in the International Program is particularly essential because the United States is already spending money on national and bilateral projects that can easily be adapted to support this very worthwhile International Biological Program.

Mr. DADDARIO. This committee will stand adjourned subject to the call of the Chair.

(Whereupon, at 12:12 p.m., the subcommittee adjourned subject to the call of the Chair.)

HOUSE CONCURRENT RESOLUTION 273—CONCURRENT RESOLUTION EXPRESSING THE SUPPORT OF THE CONGRESS, AND URGING THE SUPPORT OF PERSONS AND ORGANIZATIONS, BOTH PUBLIC AND PRIVATE, FOR THE INTERNATIONAL BIOLOGICAL PROGRAM

WEDNESDAY, JULY 12, 1967

**HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND ASTRONAUTICS,
SUBCOMMITTEE ON SCIENCE, RESEARCH, AND DEVELOPMENT,
*Washington, D.C.***

The Subcommittee on Science, Research, and Development met at 10:15 a.m., in room 2325, Rayburn House Office Building, Hon. Emilio Q. Daddario (chairman of the subcommittee) presiding.

Mr. DADDARIO. This meeting will come to order.

Our witnesses this morning are Dr. Ivan L. Bennett, Jr., Deputy Director of the Office of Science and Technology; Dr. Stanley A. Cain, Assistant Secretary, Department of the Interior, Vice Chairman of the U.S. National Committee for the IBP; Dr. David Keck, Division of the Biological and Medical Sciences, National Science Foundation; and Dr. B. H. Ketchum, Woods Hole Oceanographic Institution; Chairman, IBP Subcommittee on Productivity of Marine Communities.

I would appreciate it, gentlemen, if you would all come forward and sit at the desk even though you will outnumber the committee this morning.

I understand that Dr. Bennett will be coming soon. We will proceed if we might, gentlemen, by first hearing from Dr. Cain, who does have a prepared statement, and then we will sort of proceed to an overall roundtable discussion.

**STATEMENT OF DR. STANLEY A. CAIN, ASSISTANT SECRETARY,
DEPARTMENT OF THE INTERIOR, FOR FISH AND WILDLIFE AND
PARKS**

Dr. CAIN. Mr. Chairman, I believe copies of my statement, which is rather long, were delivered to the committee yesterday afternoon and I would be pleased to have this inserted into the record.

If you wish, I can read it or read from it. If not, we will just accept it as though read.

Mr. DADDARIO. Why don't you give us a quick résumé of it?

The points you would like to draw to our attention, we will have for the record; then Dr. Ketchum could do the same with his.

Dr. CAIN. In the first few pages of the statement, I did return to some points that had been presented to the committee in earlier testimony to add an emphasis and to add my own language for certain points.

I might call your attention to one point on page 3 because the international aspect of this program has come under discussion—I decided to use the word “coaction” instead of a more frequent one, cooperation or coordination or something like that.

I think that deserves a word of explanation. What I wanted to imply was that many of the international aspects of the program will be accomplished by scientists from different nations working under the national program of each. The coaction is obtained by the common understanding of the goals they seek and the methods they will use and to some degree the standards which they seek.

This is a bit looser than cooperation in the usual sense of the word. There is cooperation which comes through the international organization which has parallel subcommittees to our Nation’s and those of other nations. Also there are being prepared on an international basis handbooks which provide descriptions of procedures so there will be common effort.

On the other hand, it is also an inevitable result that in the U.S. national program and in others, there will be work done overseas. This will be researches that will take place in the Arctic or Amazon or some other place. Under these kinds of international research it is proper and necessary to have direct cooperation with the national scientists of the host country. The international concern is extremely important both in the goals of the International Biological Program and the general relationship to human welfare. So many of the jobs that we wish to do are not and cannot be bound by national boundaries and the developing nations are in desperate need of direct assistance by being able to work with scientists of experience. The already developed nations are anxious to put their resources collectively toward a certain goal so for all these reasons I thought the international aspect needed an additional emphasis.

There is another point here that is raised in the beginning of the testimony that I would like to mention briefly. That is that we really believe that the scientific methods and the concepts that are involved are international and transferable. But I, at least, am convinced that the technology—the basis for using the science—has to be adapted to the conditions under which you work, and this includes the natural conditions of environment, because how you proceed in a high tropical rain forest is different from the way you proceed somewhere else.

You have also the adaptation of this to the cultural circumstances; the way people work, so there is great value for us to be gained in this experience as well as for the possibility of important assistance to other countries, important cooperative work with other countries.

I think out of this testimony, too, I would like to draw a distinction between the part of the international biological program that is essentially in-house work. For Federal agencies, such as the Depart-

ment of Interior that I am associated with, we have identified a considerable amount of this on-going research related to IBP.

We have found an interest in the Bureaus of Interior, not only for continuing this but for increasing their emphasis and their activity with respect to the kinds of ecological problems which provide a fundamental underpinning of information that, over the long run, is indispensable to the solution of short term, very practical mission problems. I find in Interior a deep desire to carry on more fundamental interdisciplinary ecological types of investigations as related to production of ecosystems, as related to their structure and functioning, as related to their geographic distribution, and ultimately as economic species that are parts of his system, but we need this fundamental knowledge and the mission agencies recognize this.

Mr. DADDARIO. You are adding here the idea, Dr. Cain, that some of the problems you are seeking solution to naturally could be solved more easily as a result of the experience we will develop through the IBP.

Dr. CAIN. Yes. If we had more fundamental knowledge in relation to the natural systems of the biological community together with its environment on the different scales that are involved—if we had more of this information, we could shorten the period of time between the identification of a problem and its practical solution for the missions of the agencies.

Mr. DADDARIO. We ought to look at this not purely on the basis that we, as a developed country, are using our talents, skills, and ability to aid the developing countries and the emerging nations, but, rather, to use this as an opportunity to improve ourselves as well?

Dr. CAIN. The idea of technical assistance, if you will, is one which we accept as an integral part of the International Biological Program. But I think the point now is that this is important to our own national purposes and this sort of thing carried on within the boundaries of the United States as well as in other related parts of the international sphere is important to us.

Mr. DADDARIO. The point you raise is an extremely vital one of the purposes of effectively acting on this resolution before us, and that is, we show as we attempt to get support for it, not to support just a name, but financially that it serves our purposes as well.

This is not a one-way street that we are trying to build.

Dr. CAIN. Well, I couldn't agree with you more and I think I might say that I don't personally feel that there is any motivation here that is an emotional desire to do good—not that we are against doing good—but this isn't the motivation.

These are hardheaded people—these scientists who want to work on these problems and are getting ready to go. Their hardheadedness is based around a national self-interest. I agree with the point that you are just stressing.

Mr. DADDARIO. It is really your point. I am just trying to take advantage of it.

Dr. CAIN. Thank you for saying it better. I think the work that the Federal Government in its Federal branches can carry on part of the national IBP program as it has developed. Perhaps in more cases it is more loosely connected and we would call it IBP-related.

This must be recognized as only one part of what we are after. The other part—and I would say a larger and in many ways a more important part—is that which will be carried out by nongovernmental scientists.

Undoubtedly, in many cases there will be cooperation between the two sectors of our scientific public. What I would like to make specific here is that the national committee itself, particularly as the nine sections have developed program concepts and as they have tried to put boundaries on them so that they don't sprawl all over the field of biological science, they are developing multidisciplinary programs with good plans and good focus which will in each case be largely nongovernmental—whether it is the polar study or the Hawaiian study or the river basin study, or whichever one of these identified action area programs it is.

Then, the third phase of the program that is developing is a very gratifying interest of individual scientists to put forward projects which are part of IBP. The scientist as an individual feels that here is a place where he can do something along this line, that this is the kind of research he wants to do, and that it is IPB-related.

So, the third part is projects put in by individual scientists or small teams in a given laboratory as distinct from the programs that have been designed by the national committee itself.

And the Federal ability to assist this work of non-Federal science is in some instances very limited.

The ability to make grants or contracts for getting something done that would be part of IBP is limited in some of the Federal agencies and in some cases where it is crucial to their purposes it is limited.

One of the problems that I see is how do we get the money for more adequate support for IBP research for nongovernment science?

Mr. DADDARIO. You feel if the funds are available you would get a strong reaction as to important projects?

Dr. CAIN. We know that. We have had 89 proposals come in in which the authors feel what they proposed to do was part of the IBP program.

Then, judgments are made and out of the whole array there is some whittling down as is normal.

This is by the national committee and then the funding agency as they decide on the comparative scientific merit. The history of the first 5 months of this year is that these 80 proposals were cut in half again, down to about a quarter of the number and dollar value by the granting agencies.

To wind this point up, as far as I wish to make it, I hope that some way or another we will find new money for the traditional granting agencies who are in the business of supporting nongovernmental research. I think that is the point. How it is to be done is another matter. I don't know how it should be done.

Mr. DADDARIO. Without going into it, you have, then, obviously established an evaluating process.

Dr. CAIN. Yes, we have that in the national committee and the granting agencies have their procedures which are well established.

There are some figures on page 10 of my prepared statement, I don't see any particular point in going into them. I would emphasize, how-

ever, if I may, that on page 10, for example, the five programs on which a dollar sign has been placed—these have been planned by the national committee in sections. There are others which haven't matured to this stage of planning. Then when we are referring to the research project proposals which have come in from individuals—small teams—this figure is based on the record of the first 5 months of 1967.

There are two points to emphasize.

One is that the scientific community has gotten tremendously excited about this as something worth doing. These are already all busy men. They have been doing science. That is their life.

Now, they want to do this sort of thing. Some of them have been doing this and see an opportunity to do more. Others have perhaps been in other activities and want to get on this because it is exciting to them and looks very much worth doing.

Then, the bulk of the rest of the testimony is devoted to a few examples of past experience of U.S. and other governments where there have been costly mistakes because of the absence of ignoring of fundamental ecological knowledge. We would like in the future to help avoid these kinds of mistakes.

Then, there is some discussion of cases in which there are current questions before us where I, in my testimony, feel that we are not at the present moment giving adequate attention to the ecological aspects of our operations. For example, there is a case on page 16 in which I referred to the Commission studying a sea-level canal somewhere around the Isthmus of Panama or somewhere around North and South America.

I was very much impressed a year or so ago by the unconcern for the probable consequences of what they were talking about doing, both in terms of biology in the sea-level connection between two oceans that have been separated for millions of years of geological time and the consequences, if they were to dig this ditch by atomic crater.

In other words, the ramifying changes of things which happen when you do something like this simply were being ignored or largely ignored. There are other examples here. I don't want to emphasize them.

I mention one right in our own department and I will say we have been talking about it for several months and that is the weather modification problem. The early work on weather modification and this program as a research and experimental pilot program has grown in the last 4 or 5 years up to \$5 or \$6 million a year and it is only in the last 2 months that any attention has been paid to the consequences, as to whether you can make it operational or not.

I am talking about the consequences with respect to natural vegetation, with respect to agriculture, with respect to the general economy of regions. Heaven knows the legal problems that might arise from it. I feel that there are many things right now in Government that deserve an ecological kind of thinking about the problem, which means that you put your emphasis upon the interrelation among things in systems.

My summary turned out to be as long as my testimony, I am afraid.

Mr. DADDARIO. Well, we are pleased to have it and I might say that you find yourself in complete agreement with your Commission.

During our investigation in pollution we came to the same conclusions. There is no question but that you point us in the direction of showing concern about what happens through these gigantic programs before getting into them and before waiting for the results after they have been accomplished. We note, too, Dr. Ripley brought up the canals through the Panama and the great problems that could arise.

We are pleased to have them because I do think the public needs to recognize that there must be full funding for full programing rather than to just go to the point where it seems that we are accomplishing something and it leaves a lot below the surface that can cause us so much harm.

(Statement of Dr. S. A. Cain, referred to above, is as follows:)

PREPARED STATEMENT OF DR. STANLEY A. CAIN, ASSISTANT SECRETARY OF THE
INTERIOR FOR FISH AND WILDLIFE AND PARKS

Mr. Chairman and members of the Subcommittee, I regret that I was not in Washington at the time of earlier hearings on House Concurrent Resolution 273. I want to thank you for this opportunity to discuss the International Biological Program today, since I have a deep professional interest in its future.

Before I became associated with the Department of the Interior in my present capacity, I was Chairman of the ad hoc committee of the National Academy of Science when the proposal for an International Biological Program made by the International Council of Scientific Unions was under review. This was in 1964, and we made a concerted effort to assess the interest of American scientists in it. Later I became a member of the International Special Committee for IBP and one of its four Vice Presidents. When the U.S. National Committee was formed in 1964, Dr. Byerly and I became Vice Chairmen under Dr. Revelle's leadership. I am happy to say that during the three strenuous years of planning and organization of U.S. participation in IBP, my only regret has been that the press of other duties seems destined to keep me from actively engaging in the forthcoming research phase. There are many exciting and important studies to be made.

I would like to begin my remarks by repeating, with only minimum comment, a few of the main points that have been made about the International Biological Program.

The problems that the Program will tackle are worldwide and their solutions are important to humanity everywhere. The primary focus of IBP relates directly to one of the fundamental issues of the world today: How to provide an adequate supply of food and other essentials to a growing world population.

Every day's net addition to the world's population makes the land-to-man ratio less favorable, and increases the urgency of scientific understanding that will aid sustainable pre-acre increases of useful products in the face of obvious environmental deterioration. It is this basic cluster of problems dealing with the natural and the manageable productivity of terrestrial, fresh and saltwater communities and their constituent species that lies at the heart of the IBP.

No reputable scientists would, of course, claim that solution of these pressing problems will be easy or quick. Nor would he claim that they are scientific problems only. The scientific knowledge that we have painfully amassed and that which we so urgently seek, must be applied in full context of many varying cultures. Hence the importance of other concerns of IBP the emphasis on scientific aspects of human adaptability and the use and management of natural biological resources. So the IBP is being carried out as planned *co-action*, not only among scientists of distinct and often separated disciplines but also among those of many nations. While the criteria for political, social, ethical and economic positions and actions differ among nations, the criteria of science are universal. Scientific concepts, methods and understandings can flow unchanged across international boundaries, and desperately need encouragement to do so. But the technological derivatives from science often must be adapted to different cultural milieus.

The international goals of the IBP do not have to be sold, only facilitated. We know the eagerness for scientific cooperation on the part of developed nations in the areas of IBP interest and, even more importantly, the urgent hunger of the poor nations for scientific assistance.

Without derogating the last two decades of international programs, bilateral and multilateral, the fact remains that our fund of basic knowledge of productivity in ecological system, as sought by the IBP scientists, is woefully small. There is no question that hundreds of scientists in the United States—and thousands around the world—are now joining in the IBP to get on with this kind of fundamental work that certainly will pay off in improved human well being.

We all know the accelerating rate of change in the modern world. These changes have come upon us faster than we have been able to understand them. They have increased uncertainty and multiplied the unanticipated and adverse consequences of our actions. In some cases, these consequences are technically reversible but may be economically or otherwise irreversible. In others, their correction may involve a time-span much longer than that of the processes that are damaging our environment. Not only is the magnitude of such problems increasing at an almost incomprehensible rate but the problems do not remain local. Whatever their origin, they become national and international. I need not amplify these matters. The perceptive report issued by this Subcommittee last year on Environmental Pollution has brought to public attention many of these problems in a concise and understandable fashion.

I believe that we have long had an imbalance in our scientific effort. Biology has made its advances, of course. These are well known in biochemistry, biophysics and genetics, and through their pay-off in medicine and health. But it is the "hard sciences" in support of physical technology that have done more to change the modern world and its industrial processes.

Our concentrated attention to physical technology with quick profitable pay-offs has worked—up to a point—but we are now appreciating that our relative neglect of systems in nature, especially the vastly more complicated systems of biology and culture, have given us the urgent critical problems of our time.

IBP is really the world's first organized effort to face up to this class of vital problems that deal with the limits of natural productivity in various ecological systems. The possibilities of human management of such systems extends before us new frontiers that can be reached if we develop and apply ecological knowledge.

We are not, of course, starting from scratch. Late in 1965, Dr. Hornig, Director of the Office of Science and Technology, asked the National Science Foundation to establish a Federal Council for the IBP and urged Federal agencies to participate as actively as possible in the developing program.

At that time, most of the agencies looked over their on-going programs and identified what they were doing that was pertinent to the objectives of the IBP. The Department of the Interior, for example, formed its own IBP committee and its bureaus listed and described briefly a large number of projects. This was done about mid-1966 when the U.S. National Committee actually did not have the scope and details of the program as well worked out as at present. Nevertheless, we identified over 300 projects as possible candidates. Some three dozen of these on-going projects have been approved by the National Committee as appropriate to IBP.

What this means in Interior, and it is true of other Federal Departments, is that ecology is not a new area of concern and study. Environmental scientists have long known that nature is an endless web of interactions, and to a significant degree they have been working with conditions and processes and inter-relationships among them.

Those agencies with responsibilities for the management of living natural resources provide good examples, such as the farm, livestock and forest activities of the Department of Agriculture and those of the Fish and Wildlife Service of Interior. In a different frame of reference the National Institutes of Health have been working on human ecological problems, as have the National Aeronautics and Space Agency and the Department of Defense. Some of the most advanced and comprehensive studies of natural ecological systems are being made by the Atomic Energy Commission, and the Office of Naval Research and other units of the Department of Defense have supported both broad and narrowly focused studies of nature.

While all of this is true and there is an impressive amount of current work that is related to or might be considered a part of the U.S. involvement in the International Biological Program, it is also true that agencies of government have a history of restrictions on ecological study that are imposed by the limits of their

specific missions. They are, in general, under a pressing imperative to solve immediate practical problems.

One consequence of this situation is that Federal agencies can seldom devote to fundamental studies of entire biological systems and their inseparable environments either the scientific and technological manpower or the funds that they could justify on a long-range basis. Thus, while I have not found opposition in our bureaus to expanding the kind of work that fits into the IBP program, I do find—at least in Interior—that program formation processes are not at present a very good vehicle for enhancement of IBP-related work. Also, unfortunately, in government, as in universities, specialties and projects are often compartmentalized and out of communication. It is difficult to establish broad multi-disciplinary task forces and assign them to long-term fundamental environmental research.

How changes of emphasis may come about, however, under suitable circumstances, is illustrated in the marine field by the enthusiasm and fine effort of the new Marine Resources and Engineering Development Council ably chaired by Vice President Humphrey. This Council and its small staff, under the active direction of Dr. Wenk have elicited intensive government-wide activity along dozens of lines vital to finding out how to make effective use of the sea.

A similar marshalling of Federal strength in fresh water and terrestrial areas, especially to coordinate and enhance efforts, is feasible, practical and urgently needed. No better mechanism has been thought out than the IBP for interdisciplinary and multidisciplinary research on biological productivity and human welfare.

I do not wish to overemphasize the Federal role in IBP. It is an important part, but only a part, of what IBP is about. It will be advanced by the work of individual scientists and the programs of single laboratories. It is possible for government also to develop larger multidisciplinary programs, within certain bureaus and cooperatively among bureaus. It is also possible to extend the present level of international cooperation. But such efforts seem likely to develop more freely and work better outside of government.

In this connection the Committee has already heard testimony about the developing non-governmental part of IBP. I would like to elaborate under three headings: projects, action programs, and training.

During the first five months of 1967, the U.S. National Committee has received 89 research project proposals from individuals or small groups, usually from a single laboratory or institution. As proposed, the sum asked for was approximately \$160 million over the planned 5-year period. Judgment as to the appropriateness of the project proposals to the defined IBP program, made by the U.S. National Committee and its nine subject-matter subcommittees, ran about 50% rejection and 50% acceptance. The successful proposals then go to the granting agencies, where again they have experienced about the same 50% rejection and acceptance rate based on comparative scientific merit. This successive halving of proposals leaves us with possibly \$40 million of research projects that are meritorious, appropriate to the International Biological Program, and worthy of financing.

I turn now to our experience so far with programs designed by the U.S. National Committee itself. I believe that Dr. Revelle called these "major action programs" when he mentioned them during an earlier hearing. Five of them have received approximate price-tags for 5-year efforts:

Study of circumpolar peoples, \$500,000

Studies of migrant peoples, \$10 million

Hawaiian program, \$2 million

Studies of aerobiology, \$16 million

Systematic studies of drainage basins as ecosystems, \$15 million if two basins are undertaken; and \$45 million if six are studied (to permit comparative studies of all the major landscape types from which human sustenance is derived).

This totals \$43.5 or \$73.5 million for major action programs.

It has been apparent to all who have worked on the organization and planning of IBP, internationally and in the U.S., that there is a concurrent need for education and training, especially at the graduate level, both at institutions and in the field where projects are being carried out. The best present estimate of our institutional capacity for such training leads to a 5-year figure of \$21 million.

These elements of the Program can be summarized as follows:

	Millions
Research project proposals-----	\$40
Major action programs (\$43,500,000) or-----	73.5
Training-----	21
Total-----	134.5
U.S. National Committee overhead-----	1.5
Total-----	136.0

What I have called U.S. National Committee overhead has been obtained so far from the National Science Foundation and later by contributions from Federal agencies. I do not believe that the agencies on the IBP Council should continue to be asked to absorb this expense for planning and administration which, after all, seems to be little more than one percent of what is already sought for research.

It seems equally clear to me that not all of the funds being sought by American scientists to support IBP research can be squeezed out of existing appropriations for the agencies. That kind of latitude just does not exist. Furthermore, as important as we feel that this program is for national goals, it should not be entirely funded at the expense of existing worthy programs.

Now, if the American taxpayer is to finance a sizeable research program in ecology and related fields, and support the U.S. participation in an international biological program, he deserves to be told why it is a good investment.

First, I think, we can safely predict that deepening and widening our knowledge of the dynamics of a wide variety of plant and animal communities will better enable us to avoid ecological blunders. History is full of costly mistakes and some fiascos that would not have occurred had there been adequate ecological information or even a feeling for ecological relationships in nature. Here are a few examples that are well known:

—In the early post-war years Britain undertook an extensive groundnut program in East Africa and lost some millions of pounds because the inter-relationships of savanna vegetation, savanna climate, and savanna soils were not understood. The planners with a proper feeling for the nutritional need for more oil and protein among native peoples had ground prepared, peanuts planted, and hearts broken when the dry-season soils became like concrete.

—Again in the early post-war years UNESCO started a Hylean Amazonian program that was to develop a new breadbasket for the world's hungry people. There were international political troubles, but a central cause of failure was an inadequate understanding of tropical rain forest ecology, including lateritic soils. Something similar happened to Ford's rubber plantations and other efforts at monoculture in an area of the world's most complex natural vegetation.

—Much of the devastation and deterioration of soils in East Africa is due to dependence on cattle. Dr. Darling and other have pointed out that more useful animal flesh suitable for human consumption is laid down on the several species of native ungulates, and this is done without loss of range productivity. Something similar can be about much of our treatment of the arid and semiarid Western United States where over-grazing by cattle and sheep has seriously depleted range productivity. We, too, have participated, even continue to do so, in what has been called the Saharaization of the land.

—A pesticide program in Nova Scotia was embarked upon to control spruce budworm. The budworm was inadequately controlled, other insects had damaging exploded populations, valuable insect-eating birds were killed, and salmon almost disappeared from the streams.

—Around the turn of the century after we had pretty well cut over Eastern and Great Lakes timber and inroads were being made in the Gulf States and the West, there was a surge of afforestation programs in fear that we would run out of timber. Many of these plantations were complete failures because of the use of ill-adapted species or strains from climates quite different from the new plantings. Even as we developed more sense about this, it took a few more decades to realize that most conifers grow better with intermingled plantings of broadleaf species because of their effect in maintaining mineral balance and structure in the soil.

—In Pakistan, in the U.S. Southwest, and many other arid and semi-arid places faulty irrigation practices have ruined the soil for crop production by accumulation of salts at the surface and the development of shallow hardpans.

—In parts of South Florida the combination of over-drainage and over-draft of

aquifers for irrigated agriculture has destroyed the productivity of the soil, in some cases in a few years time.

It is not entirely fair to judge the acts of history in the light of subsequent knowledge, but it is fair to say that the checks and balances of nature never seem to have caused enough concern. The attractions of immediate profit and the stultifying influence of traditional practices cause many persons to think only about immediate conditions, forgetting or ignoring possible longer-term consequences of their actions.

We are still pursuing too many essentially single-purpose programs, running the risk of failure—not necessarily failure to accomplish the narrow objective, but failure to anticipate the consequences of our actions. These consequences, often unexplored side-effects, are ignored until they bear down upon us with intolerable changes in the environment. In some cases the cost of these changes may far exceed the direct benefits of a project.

—Most of our pollution problems of air, water and soil have arisen because we have come late to consider the effects of emitted gases, solid wastes, organic trash on the environment on which our very lives depend. Clever engineering went into everything but exhaust trash of internal combustion engines. Clever engineering goes into paper making and a thousand other manufactured products—and the poisonous effluents pour into streams, lakes and the seas. Only now, after decades of unconcern are we mounting an intensive program to handle human wastes by other means than pollution dilution. We are just now beginning to be given cost estimates for the correction of our oversights, and the tag is in the billions of dollars. In these terribly important matters environmental science is still in its infancy.

—We have spent millions on research and control of lamprey in the Great Lakes and are now spending more millions to re-establish lake trout and whitefish and control alewife. The lamprey and alewife came into the Great Lakes from the sea, using canals which were constructed with no thought for biological consequences.

—Today we are financing a Commission that is studying a sea-level canal to be built somewhere in Panama or Northern South America. There are two aspects of thinking about this, at least up to now, that suggest an almost complete ecological ignorance. One is the biological consequences of joining the Caribbean Sea and the Pacific Ocean that have been separated for 25 million years. The other is the possible, I would say probable, consequences of excavating the canal by nuclear explosions—consequences on man and nature. Any problem of radio-active isotopes is by no means a local one. Such materials pass along the food chains. Atlanta tuna have recently been found to cross from one side of the Atlantic to the other. Some migratory birds move between the area and the Arctic. Crustacea accumulate these substances, as they do pesticides and pass them along the food chains.

—High dams have been built without consideration of the warming of downstream waters and its effect on valuable fish resources, and sometimes without provision for passage of anadromous species.

—As more and more nuclear power plants are being constructed we are only belatedly, even in these times of scientific sophistication, beginning to design them for avoidance of heat pollution.

—The physical destruction of estuaries by dredging, excavating and filling has been continuing with little thought for the ecology of these complicated natural areas and their value for commercial and sport fisheries, wildlife, natural beauty and recreation.

—We are now well embarked upon a program of research and pilot experimentation with weather modification. It is an attractive and challenging idea, and one that promises to provide supplementary water in places where it is needed. But up to now there has been no significant attention paid to consequences of weather modification if it should become operationally successful. It is my guess that the necessary studies on the biological, economic and legal consequences of weather modification would be as expensive as the research to accomplish it, which now is running about \$5 million a year. And monitoring the modification could be a continuing cost as great as seeding the clouds. If precipitation were to be augmented 10%, perhaps 15% or 20%, ecological systems could be shifted hundreds of miles under certain circumstances, and the economy of entire regions could be disoriented.

The record is not all black. Ecological knowledge has been put to work in many cases with great success, and the pay-off has been important. Better fundamental knowledge of biological systems to serve as the basis on which applied findings may be derived is a second source of return from the IBP investment.

—Knowledge of freshwater systems is being put to work in fish pond developments, especially in our Middle West and South, which produce as much as a ton per acre of fish. And in rice-growing areas fish are raised with paddy, or rotated soybeans and rice.

—Aquiculture is being extended to salt and brackish waters in the cultivation, the farming, of oysters and shrimp.

—Reservoirs and lakes are being stocked with carefully selected organisms to fill out a productive food chain.

—Forest and range units are becoming more productive of a wide variety of goods and services because their management is firmly based on knowledge of plant and animal ecology.

—Systematic, long-term research into the central Pacific as a complete environment system has led to the discovery of such things as the equatorial undercurrent, factors explaining distribution and abundance of tuna, and prediction of skipjack catch on the basis of seasonal warming and location of water masses.

—The program of our Soil Conservation Service, designed in the 1930's to help correct the mistakes of our past soil and water mismanagement has matured into a land capability system that helps allocate each acre to its highest sustainable use.

—Comparative agrobiological and agroclimatology are guiding land-use developments around the world, or at least have the capacity to do so.

—Identification and study of planktonic eggs and larvae in the California ocean current system have led to discovery and development of Pacific hake resources to support an expanding fishing enterprise off the State of Washington.

—Many massive health programs are founded on detailed ecological knowledge of vector roles and their niches in the biological-environmental systems of nature. Disease problems in every part of the world, from rain forest to desert to tundra and high mountains, have caused medical researchers to look closely at the natural and agricultural communities in which people live.

I would like very much to be able to indicate how each thousand or million dollars invested in the International Biological Program would return two, ten, or fifty times the investment in forestalling ecological catastrophes or in making lucrative resources development applications possible. I cannot do this, however; and in a larger, more important sense, I do not believe it is realistic to attempt to.

Why? In the first place, we are talking about the kinds of investment—in research and understanding of the world—in which we have great confidence because we have had beneficial experience in the past. We know, for example, that agricultural research has paid off handsomely over the past century and especially in recent decades. Yet we cannot, with confidence, equate the cost of a particular project with its dollar benefits. Some preliminary and still very crude evaluations that we are doing in our Department on fishery research and development efforts, by the way, do suggest surprisingly high returns. In any event, we look mainly at the net cost and net benefits of the total effort. We seldom convert our investment in medicine and health into dollars resulting from one man's working days saved.

We expect pay-offs from our space developments, spin-offs, but we do not keep our books on a double-entry system any more than we apply such accounting to national security. We do not even say that to clean our air and water of pollution will yield benefits of certain amounts in relation to expenditures.

In the second place, the IBP does not deal exclusively with marketable products. We are talking about life and health, and survival. We are talking about an environment worthy of man and one in which he is comfortable and in which he can take pleasure.

At no time has pre-existing biological and environmental knowledge been adequate. We simply do not know our world well enough. It seems that we know more about the sub-atomic particles than we do micro-organisms. We have more knowledge about the functioning and forces among heavenly bodies than we do about the elements of a tropical rain forest. Despite the myriad species of plants and animals that have been discovered, described in Latin and catalogued, we

depend largely on a relative few kinds brought into cultivation and domestication before the advent of history.

Simple people close to nature have established cultural patterns—social and agricultural—with stability for centuries. This we cannot return to. But we need not do to our land what Rome did to North Africa, once its granary, and earlier civilizations did to the crescent of Eastern Mediterranean lands. But if we are to last for long, we must do two things: we must bring our explosive human population under control and we must restore and maintain the quality of our environment. Both imperatives require new dimensions in knowledge.

What was once based on the slow accumulation of experience and adopted as inexorable restraints on society, what was based on acute but unsophisticated natural history, we must now base on the most refined of our scientific technology and understandings. Such an environmental science is being created and put to work, but to only a limited extent. It is vital that we make other beginnings in a massive effort along these lines. We must face the challenge of understanding the world around us and the place of mankind in it. The International Biological Program, I believe, is a vital step along the way.

Mr. DADDARIO. Dr. Ketchum, can you summarize your paper, please?

Dr. KETCHUM. I will be glad to.

Mr. DADDARIO. We will accept the full statement as if read.

(Prepared statement of Dr. Bostwick H. Ketchum, referred to above, is as follows:)

PREPARED STATEMENT BY DR. BOSTWICK H. KETCHUM, ASSOCIATE DIRECTOR OF THE WOODS HOLE OCEANOGRAPHIC INSTITUTE AND CHAIRMAN OF THE MARINE SECTIONAL COMMITTEE OF THE U.S. NATIONAL COMMITTEE FOR THE INTERNATIONAL BIOLOGICAL PROGRAM

The Marine Program, which is developing as a U.S. contribution to the IBP, is concentrating on basic biological studies which are essential to provide the evaluation of the effects of man's modification of the marine environment in estuaries and coastal waters. About half of our population in this country lives adjacent to these coastal waters or to the shores of the Great Lakes. As a result of this concentration of population, man has become the most important living organism who uses and depends upon these waters in many ways.

Traditionally the maintenance of adequate navigation and the disposal of the wastes of our civilization into the waterway have been two of the major demands made by man on estuarine waters. These activities of man have been detrimental to the quality of the environment in estuaries, and this is, in part, because we do not understand the biological effects of man's activities. We do not understand well enough the ways in which the ecosystem works to predict the results of new or proposed engineering developments. By hindsight we can evaluate what man has done in the past. The problem is well stated in the report of this subcommittee on "Environmental Pollution" and I quote:

"Since man is very much a part of the biosphere, the living environment, he has always been changing and using the natural resources for his own benefit. Mistakes have been made and consequences have not always been foreseen, but civilization has advanced by taking risks which were largely overshadowed by obvious benefits."

For decades it has been obvious that pollution of rivers and estuaries has had a profound effect upon the food resources which are dependent upon them; that shad and salmon no longer run in many of our eastern Atlantic streams and that areas suitable for growing and harvesting of shellfish are constantly becoming more drastically curtailed.

Over 90% of the total harvest of seafood taken by American fishermen comes from the continental shelf and about two-thirds of that harvest consist of species whose existence depends on the estuarine zone or which must pass through the estuary enroute to spawning grounds. In recent years the increasing demand for recreational facilities has lead to the development of a multimillion dollar boating and sport fishing industry which requires that the waters in our rivers and estuaries be maintained at as high a quality as is practical.

The following quotation from the Introduction to the Program Statement of the U.S. Subcommittee on Productivity of Marine Communities describes the problem and the approach proposed in the IBP.

"The Marine Program of the International Biological Program has among its objectives the support of studies which will improve our understanding of the ecosystem as a whole, and provide an evaluation of its responses to modifications of the environment. Because of man's increasing interaction with his environment, it is urgent that marine biologists undertake, on a global scale, studies which will produce a clear definition of existing conditions, and an understanding of the mechanisms and processes which control the distribution, survival and development of natural populations.

"The rapidly increasing human population and the great technological developments of this century have modified the natural environment in many ways. One of the most injurious of man's activities is the introduction of pollutants into the air and water. In the marine environment man's influence has been greatest in the estuaries and the near-shore coastal waters. Even far from land, however, such pollutants as fallout radioisotopes, lead, and the organic residues of pesticides and detergents have been detected. Increasing pollution is a byproduct of industrial and municipal progress and is perhaps unavoidable in our affluent society. It is necessary to dispose of wastes; but as practiced today, waste disposal has depleted many natural populations and destroyed the recreational potential of waterways which a few years ago were among the most beautiful in the world. Unfortunately, the marine biologist is unable to predict the specific effects of proposed modifications or developments. There is urgent need to develop this ability.

"Another important reason for seeking a better understanding of the marine environment is the growing need to exploit more effectively the protein resources of the sea. There are many questions to be answered: Can local stocks of food organisms be maintained while industrial and commercial activities in inshore and shallow waters are developed? Can fishing pressures be increased? Can edible but presently not marketed forms take up the slack? Can we turn from being merely a predator to becoming a knowledgeable steward of the riches of our waters?

"At least half the world's people lack sufficient animal protein in their diet. The production of carbohydrates on the land has been greatly increased and can be further increased to provide sufficient calories for human needs. The importance of the sea is in providing animal protein. The sea is already supplying a major share of the protein eaten by many people in the world and is an obvious source for additional supplies. Fundamental knowledge of the ocean and its biota is necessary in order to use our marine resources wisely."

The sea is very important as a producer of animal protein which is needed to feed the expanding population of the world. For the United States and most of the European countries fish generally makes up a relatively small part of the animal protein eaten, only 5% in the United States for example. In other parts of the world the story is quite different. The countries of the Far East satisfy more than half of their animal protein needs with fish. It has been estimated that more than 1,500 million people depend upon marine resources for at least half of their animal protein needs. In terms of closing the gap between starvation and subsistence fish protein occupies a very important place in the nutrition of world populations.

While the world harvest of marine fisheries products has been expanding rapidly the U.S. contribution, while not insignificant, has remained relatively constant over the last decade. Between 1954 and 1964 the use of fish and fish products in the United States has increased from about 7,500,000 lbs to 12,000,000 lbs but during the same interval the proportion of this supplied by domestic fishermen has decreased from 63% to 38% of the total.

It is clear that the problems associated both with harvesting the marine resources and with the effects of pollution are complex problems. They include not only an inadequate understanding of the biology but also problems of sociology and economy. For example, the decreasing proportion of domestic production of fish products during the last decade cannot be attributed to a lack of fish in our coastal waters since other nations have been increasing their catch in our waters while our harvest has remained constant. A better understanding of the biology, however, offers a promise of ultimately reversing this trend.

Our existing scientific knowledge of the biological processes in the sea has not yet been utilized to increase the production of marine products which are useful to man. American technology excels in this type of application as is well demonstrated by our industrial and agricultural developments. We now have some

preliminary notions as to how marine productivity might be increased, but the biologists still cannot predict with an adequate degree of confidence what the effects of fertilization of the sea would be even though we know that marine production is limited by the lack of fertilizing elements in the water.

Thus, man is faced with two important problems which are certain to get more critical with the passage of time. Even as he pollutes his natural waters to excess and creates undesirable and obnoxious conditions in our rivers and estuaries he is faced with a growing need for supplies of food for the rapidly expanding populations of the world. Considered as separate problems these each seem almost hopeless of solution. However, the characteristics of population which create problems are the same as those which are needed to increase the productivity of the sea and to augment the food supply so greatly needed by mankind. This would require a drastic change in viewpoint, and we must begin to consider the wastes of our affluent society as a source of materials which might be used to benefit mankind.

A much more fundamental understanding of the effects of fertilization of the sea on the natural populations is needed before we know how to increase the production of useful species rather than of obnoxious ones by judicious and intelligent fertilization of the sea. It is this type of understanding which the Marine Program of IBP hopes to achieve. Even when this understanding is available, of course, there will be engineering problems to determine the feasibility and to evaluate whether the economic returns would justify the effort and expense.

The IBP makes no pretense that it can solve the sociological, economic, and engineering problems, but it seems clear that a better understanding of the biology and ecology of marine organisms is essential before one can even consider the application of technology to the solution of these fundamental human problems. The biologist must provide the engineer with the basic information which will permit evaluation of the effects of man's activities on marine populations. This increased understanding can, we hope, be supplied by investigations developed and supported in the IBP.

A great deal of marine ecological research in coastal and estuarine waters is already being supported in this country, and a substantial part of this ongoing research will contribute to the objectives of the IBP. For example, the budget in FY 1966 of the U.S. Department of the Interior for studies related to the development and maintenance of aquatic living resources was 26.1 million dollars. Additional research support is already available from such agencies as NSF, NIH, AEC, and ONR. An analysis is now being made of the research which is currently being supported which is pertinent to the aims of the IBP.

The increase in marine ecological research which develops under the aegis of IBP will probably be small compared to the current activities, perhaps 10-20%. It can be an important augmentation, however, if it succeeds in developing a realization, on the part of ecologists, of the importance of these basic studies to the solution of problems of immediate and critical importance to mankind.

The Marine Program is developing along three main lines of investigation, namely:

1. Interdisciplinary ecosystem analysis.
2. The effects of planned modification of the environment.
3. Distribution and abundance of marine organisms.

These are described in the program statement issued by the Subcommittee in January of this year. Each of these is a complex investigation which is beyond the resources of any single individual scientist or even of most of the groups of scientists in our universities or research laboratories. Cooperation is essential and it is hoped that IBP can provide the mechanism for the necessary cooperation on both the National and International levels.

Four regional meetings have been held in order to discuss the ways in which the research of individual scientists could benefit by such cooperation and could contribute to the objectives of the IBP. In these conferences, which involved over 100 scientists, about 80 pertinent programs have been defined. Some of these are already in progress and are now supported at a level of \$3.3 million annually. It was estimated that the cost of initiating new programs and the desirable modification and expansion of existing programs to achieve the objectives of the IBP would be about \$3.7 million annually, giving a total estimated cost of seven million annually for the Marine Program of the IBP. This estimate of the increased support needed for the marine activities in IBP may be an overesti-

mate since the programs have not been judged for quality, a judgment, which is in the province of the granting agencies. Past records suggest that only one-third to one-half of the proposals submitted are of sufficiently high quality to warrant support. On the other hand, as additional scientists learn of the aims and objectives of the IBP we may well expect to receive programs of high quality which are not included in our preliminary lists. At any rate, the additional cost of about \$3.7 million annually for the Marine Program of the IBP is the best estimate possible today.

I would like to summarize briefly a couple of the programs as examples of the types of investigation which are contemplated.

Several investigators are interested in the effects of temperature modification of the marine environment. Nuclear reactors are planned and being built along our seacoast and the seawater used for cooling purposes in these reactors will be warmed by as much as twenty-five degrees above the normal temperatures of the area. This heat can be considered a waste product, and the engineers discuss the problems of heat pollution. There are many ways that one can conceive of using this so-called waste product for the benefit of man. Could we not use this to increase the biological productivity of our cold New England waters by confining it and introducing species adapted to the higher temperatures? Could we not re-introduce this warm and lighter seawater so as to bring nutrient rich deep water to the surface to increase plant production? Could we not deliberately produce warmer water along our West Coast beaches and increase man's enjoyment of their recreational potential? If we did plan any of these benefits, what would be the effect on the natural populations in the area? We need to develop the basic biological understanding of the marine ecosystem before we can evaluate these questions.

The interdisciplinary ecosystem analysis is necessary because nature is too complex for a piecemeal attack. Many of the problems confronting biologists deal not with individual organisms or with single species populations but with the entire ecological system. For example, enriching the surface waters with fertilizing elements is certain to increase the production of marine plants, but when done to excess, as in polluted areas, the community structure is changed with fewer species able to survive in the changed environment. Those species which do survive are frequently those which are least desirable as food for the higher trophic levels. The upper limit of the amount of seafood which can be produced in an area is set by the amount of organic matter produced by the plants in the water. Excessive plant production, or plants of the wrong kind can, however, decrease rather than increase the production of seafood, just as weeds decrease the harvest of the garden. These effects are subtle at first, but dramatic when fully developed. The marine ecologists must provide the basic understanding of the ecosystem which is needed to permit us to modify the environment in ways which will increase the yield of useful species and reduce the growth of undesirable ones.

All of these problems require evaluation of the responses of organisms to the characteristics of their environment. Different species of organisms occupy different parts of the marine environment and all are spottily distributed in space and time. Thus, nature has provided us with various distribution patterns, and if we could understand why a given organism can live here but not there we would know more about how much of a change in environment the organism can stand. Many scientists are interested in these problems of geographical distribution, but much remains to be done. Even when we know where and when certain species are to be found, which is the exception rather than the rule, we know very little about the causes of the distribution. Temperature, food and salinity are certainly important in some cases, but in others the variations in these properties of the environment seem so slight that one questions their significance. Studies of the distribution and abundance of marine organisms and of the environmental causes for observed differences is therefore an important part of the IBP, and may help marine ecologists to solve some of the more practical problems of more general importance to mankind.

This summary of the U.S. Marine Program for the IBP has attempted to outline the reasons why such a program is urgent and desperately needed at this time. Some of man's most crucial problems, especially pollution and supplying protein for our expanding population, need the basic, fundamental informa-

tion which can be supplied by an intensified attack by marine ecologists. The problems are complex, and final answers will certainly not be achieved within the limits of time set by IBP. Some progress can certainly be achieved, however, and if IBP succeeds in developing a general awareness of the importance of man's part in the ecosystem and of the need to consider all aspects of the ecosystem in planning our future developments of civilization, the program will have been a great success.

STATEMENT OF DR. BOSTWICK H. KETCHUM, ASSOCIATE DIRECTOR, WOODS HOLE OCEANOGRAPHIC INSTITUTION

Dr. KETCHUM. The marine program of the International Biological Program is concentrating its activity on the estuaries and the inshore coastal waters largely because the effect of man's modification of its environment are most obvious in these inshore waters. But also partly because there are several international agencies supporting oceanography, for example, which is largely concerned with problems in the open sea. I would like to quote from one of the reports of your committee because I think this states as well as can be said the orientation that the Marine Committee has adopted independently of this statement since we discovered you made this only after our program was pretty well formed. In your report on environmental pollution it is said:

Since man is very much a part of the biosphere, the living environment, he has always been changing the natural resources for his own benefit. Mistakes have been made and consequences have not always been foreseen, but civilization has advanced by taking risks which were largely overshadowed by obvious benefits.

The risks are taken, in our viewpoint, largely because the biological effects of man's modification of the environment are unknown and unpredictable.

We feel strongly that our marine colleagues should develop the basic fundamental understandings which make predictions of the effects of changes in the environment possible.

This is not possible with what we know today. This statement, that has been handed to you, emphasizes two problems facing mankind—both of which are important and will increase in importance in the future.

One of these concerns the effects of pollution on marine environmental conditions and the other concerns the need to provide food for our expanding populations.

Now, I am not one of the ones who will argue that the oceans can feed the world, but the oceans have a very important part to play in this insofar as they already are an important source of animal protein. It is animal protein nutrition which is deficient in many parts of the world.

It has been estimated, as is stated here, that about 1,500 million people in the world—largely in the Far East—depend on seafood for at least 50 percent of their annual animal protein supply. This is a very important source and one which cannot sensibly be substituted for by land production even though the land is more efficient in raising grains and other crops which are essential to provide the calories for life. But the animal protein is an important part of our diet and the sea is important in providing this.

There is a great deal going on in the United States and in other parts of the world on the basic ecology of inshore waters. I believe it is correct that the fiscal year 1966 Department of the Interior budget for studies which are related to the development and maintenance of aquatic living resources was \$26.1 million.

A substantial part of this is closely related to the aims and objectives of the IBP and our National Committee is now making a survey to identify the parts which should be incorporated, which should be recognized as contributing to the IBP.

I do not, at this moment, know what fraction of this \$26 million this will be, but I think it will be a substantial part and maybe Dr. Cain can expand on this. There is additional research support and most of the independent research which Dr. Cain has mentioned is supported by such agencies as the National Science Foundation, the National Institutes of Health, the AEC, the Office of Naval Research, and so on.

All of these activities are in support of the general objectives of the IBP. As I have said, our committee is now making a study to try to identify what fraction of the research which is being supported is IBP-related. As a result of this, our committee does not envisage a large change in the support of marine research, but probably a small change relative to current activities—perhaps something of the order of 10- to 20-percent increase.

This, however, could be an important augmentation of the research. If it succeeds in developing a realization on the part of ecologists of the importance of these basic studies to the solution of problems of critical importance to mankind; namely, the two that I have mentioned as being part of our background; and if it succeeds in establishing the reasons for thinking that this program is crucial and urgent, then the problems of pollution of estuaries and coastal waters and the problems of supply of food may become less critical.

There are three main lines of investigation which the marine committee is supporting as central programs. There are the interdisciplinary system analysis that Dr. Cain has already mentioned, the effects of planned modification of the environment, and the studies of distribution and abundance of marine organisms.

Our approach to this problem has been to hold regional meetings and four of these have already been held which involved the participation of over 100 marine scientists in the discussion. As a result of these meetings, we have identified about 80 pertinent programs as fitting into these three categories of the main interest of the marine sectional program. Some of these are already in progress and are now being supported at a level of about 3.3 million annually, but it was estimated that the cost of initiating new programs or of modifying existing ongoing research so that it would fit more closely to the objectives of the IBP would increase the budget by about 3.7 million annually, giving a total estimated cost of about \$7 million for the marine program of the IBP.

I qualify this a bit insofar as Dr. Cain has already said some of the identified programs will be eliminated in the normal process of review. But, on the other hand, additional programs will be received from

scientists since the information and knowledge about the IBP is being disseminated; and the interest is growing in the scientific community.

I would like to summarize briefly the orientation that some of these programs have developed as an indication of the directions we are planning to take.

For example, several investigators are interested in the effects of temperature modification on the environment. Nuclear reactors are being planned and built along our seacoast and the sea water that is used for cooling will be warmed by as much as 25° above the normal temperature of the area.

One can talk about the problems of heat pollution, but I think the approach should be the opposite. In what ways could we use this excess heat in order to modify the environment in ways that are beneficial to mankind?

Couldn't we use it to increase the biological productivity of our cold New England waters, for example, by confining it and introducing species adapted to these higher temperatures?

Couldn't we introduce this warm, lighter sea water so as to bring the nutrient-rich bottom sea water to the surface?

Couldn't we deliberately produce warmer water along our west coast beaches and increase man's enjoyment of their recreational potential?

If we did plan any of these benefits, what would be the effect on the natural population of the area?

It is these questions that the scientist cannot answer. We need to develop an understanding of the marine ecosystem before we can evaluate them.

Nature is too complex for a piecemeal attack. Many problems confronting biologists deal not with the individual organisms, but with the entire ecological system.

The problems of pollution are largely created by the fact that fertilizing elements are added to the sea water; and this is now done in ways so that obnoxious and objectionable species arise and the valuable species are eliminated in the process of eutrophication.

It is conceivable that by a change in our viewpoint, and considering these not as wastes, but as materials which, if used judiciously and wisely could increase the natural productivity of the sea, we could change our concepts of these being wastes into concepts of general benefits to mankind. This is not only a biological problem. In addition to the fundamental biological problems, there are, obviously, sociological, economic, and engineering problems. IBP will not solve this aspect, but it may be able to achieve the basic biological understanding which is necessary so that the related problems may be solved.

The reasons for the inclusion of distribution and abundance of marine organisms in our fundamental program is that nature has provided us with a grand experimental program already produced and, if we could only understand why some organisms are found here and not in another place in the marine environment, we would know much more about marine biology than we now know.

We know that temperature and food supply and salinity are important in some of these distributions, but there are other distributions in which the changes in these characteristics of the sea

water are so subtle that one questions their significance. But we don't know the real reason for these various distributions. Consequently, it is considered that this aspect of our study will feed in the basic information to all other parts of the study, and a better understanding of the environmental conditions which determine where and how organisms can grow in a marine environment will be acquired.

This is a program that will not be completed in the 5 years that is envisaged for the IBP. I doubt that it will be completed in my lifetime.

I think we can make a fundamental start. I think if we can increase the awareness of the marine ecologist in the fundamental importance of these problems and of these investigations in the solutions of the practical problems which face mankind, I think that the International Biological Program will have made a contribution, not only to science but also to man's welfare.

Thank you.

Mr. DADDARIO. Thank you, Dr. Ketchum.

Dr. Cain, if we were to take Dr. Ketchum's example of the nuclear reactors and the warming of the sea, we could use it to our advantage. That would fit in with the need to have clever engineering go all the way as you have spelled it out and, if now we are to do something about it, we are actually paying now for something that we should have included in our overall planning in the first instance.

Dr. CAIN. Yes. I would agree that our traditional approach to engineering problems takes a short-time view and seeks an immediate goal of whatever the construction is. Very seldom do engineering considerations go on to the next steps as a consequence of the construction, in this case the atomic powerplant.

You haven't asked the engineers to do it. They are capable of solving many of these problems if this is part of the task assigned to them. I believe it is a public deficiency in not having thought far enough about these problems. Now we are correcting past mistakes and it is very expensive.

Mr. DADDARIO. We may not be able to. It may not work.

Dr. Bennett, you have a statement, and I know you intend to put it in the record.

PREPARED STATEMENT OF DR. IVAN L. BENNETT, JR., DEPUTY DIRECTOR, OFFICE OF SCIENCE AND TECHNOLOGY, EXECUTIVE OFFICE OF THE PRESIDENT

Mr. Chairman and members of the subcommittee, I welcome this opportunity to continue our discussion of the International Biological Program. It is my understanding that the Subcommittee is now giving serious consideration to writing a thoughtful and penetrating report on the IBP for the Congress, for the people of the U.S., and for interested laymen and scientists all over the world.

This submission to the Subcommittee consists of:

- I. Primary Statement.
- II. Summary of Benefits.
- III. Description of Action Programs.
- IV. Details of Major Programs Prepared by Drs. Smith, Benninghoff, Bliss, and Shimkin.

I. PRIMARY STATEMENT

At the outset, I think that it is significant to note that the increasing interest of this Subcommittee in the IBP parallels almost precisely the experience of others who have moral or assigned responsibilities for planning, promoting, and carrying out IBP research. Among biologists, our experience has been that initially

many rejected or only passively accepted the idea of the IBP. As plans for the program have evolved, the attitudes of most of these individuals have changed from an initial reaction of mild interest and involvement, to a more active acceptance of the challenge and enthusiastic recognition of the opportunities and responsibilities that this program places squarely on their shoulders.

It is worthwhile to repeat the theme of the IBP—"The Biological Basis of Productivity and Human Welfare" and to point out what a challenge it is to define, promote and accomplish research that will contribute to this theme. To this end, biological scientists who have accepted appointments from the President of the National Academy of Sciences to serve on the US/IBP committee and its subcommittees have themselves given further definition to the research that should be promoted. Their definitions, which you have reviewed in the form of program statements from the 9 US/IBP subcommittees, are the results of much hard work. The synthesis of these statements into National Committee report describing U.S. participation in the IBP, and the parallel publication of all these reports have had dramatic effects throughout the community of biologists both in the U.S. and internationally, particularly among South American nations. The interests among the latter have previously been described to you by Dr. W. Frank Blair on 6 June during another portion of this hearing.

Others here today will have something to say concerning the estimated costs of U.S. participation in the IBP. I would like to confine my remarks to—

- (1) our organization for selecting and accomplishing IBP research,
- (2) a discussion of the possible methods of funding that research, and
- (3) a discussion of the benefits that we can expect to accrue.

Organizationally, our U.S. effort is evolving in a fashion that I find most encouraging. Dr. Revelle alluded to this during his discussion on 9 May when he described what he termed, the "emerging" IBP programs. You will recall that the IBP is divided into two phases; the planning phase and the research phase. The U.S. Committee structure with its 9 subcommittees and 2 panels was tailored primarily for planning. Participants in this phase have defined the component programs and continuing from these, working groups have further defined specific programs and frequently have defined the elements of proposed research projects. These latter elements, which we now term "action programs," are being institutionally developed, usually in universities, and submitted for support to granting agencies. Dr. Revelle mentioned three of these programs to you. The metamorphosis involved here is best described by quoting from one of Roger's letters on this subject:

"Dear _____:

"At the last meeting of the U.S. National Committee for the International Biological Program the joint recommendations of the Subcommittees on Terrestrial Productivity and on Productivity of Freshwater Communities for a Program for Analysis of Drainage Basins and Landscapes were presented. The National Committee enthusiastically accepted and endorsed these recommendations.

"During the Paris meeting of the Special Committee for the International Biological Program (SCIBP), 28 March to 1 April, I presented a description of the Program for Analysis of Drainage Basins and Landscapes. My description was based entirely on the proposed program on this subject as developed by the two subcommittees and their invited co-workers. The program was wholeheartedly received and endorsed by members of SCIBP. It fired the imaginations of many on the international group. I think that the consensus was that the most significant impact of new work contributing to the goals of the IBP will become apparent from programs of this type. I do not mean to lessen interest in or detract from individual research projects that are or will be included in the IBP. However, coordination of a group of well-conceived, well-planned related projects will assure maximum achievement within the framework of the IBP. I think that we must recognize that this type of coordinated approach voluntarily worked out by many scientists and their supporters will become the backbone of contributions toward advancing the biological basis of human welfare and productivity and toward advancing knowledge for those purposes.

"I am grateful to you for your participation in this program. The IBP organization nationally and internationally is looking forward to further developments. I am sure that the research to be proposed and accomplished will reward all for their careful considerations and planning.

"Please accept the thanks of all who are concerned with the IBP.

"Sincerely,

"Roger Revelle, Chairman, USN/IBP."

At this time, I am told, 6 such action programs have been recognized by the U.S. National Committee and some 6 additional are in formative stages and show promise of comparable development.

It is also noteworthy that since publication of the U.S. National Committee report and the subcommittee program statements, approximately 100 individual research proposals have been submitted for inclusion in the IBP, good evidence that these publications are bearing fruit.

I think that a smooth and responsive transition to the research phase on this program is actively under way. The IBP committee-subcommittee structure, I understand, will be retained so the advisory roles that the members fulfill may be preserved and so that continuous broad program development is assured. At the same time with the emergence of the "action programs" and with an appointed director of each, the intensive coordination involved will be provided. For the record, a list of the "action programs" is attached to this statement.

Some of the difficulties of funding the IBP were discussed during the initial part of this hearing on 9 May. I think that Roger Revelle very accurately described these to you in his prepared statement, and the apparent conclusion from his statement suggests that an IBP "line item" be established somewhere in the Federal budget. During discussion of that suggestion it was brought out that during these times there would be a fiscal hazard in following this suggestion since such a line item might be stricken during the budgetary competition for funds and that an agency might then be loath to allocate any funds to the IBP as such. We concluded that the present method of funding is perhaps safer. Involved in this discussion was the question—just how much will the IBP cost, and a "guesstimate" of \$50 to \$75 million over a five-year period was given. This figure included \$60 million for recognized IBP "action programs" and projects and \$15 million for those still undergoing definition. During the 6 June portion of this hearing a rough estimate of \$100 million was provided. I have been furnished with a more recent estimate that brackets the cost between \$105 and \$135 million. This latter estimate involves \$21 million for training—an estimate that was not included in the previous figures. All estimates however are carefully qualified and point out that as additional IBP "action programs" are identified, the 5 year estimate will probably increase. The significance of this apparent escalation in my opinion is related not so much the Committee's inability to estimate as it does to the rapid increase in interest in the IBP which is spreading through our community of biologists.

In the course of these hearings, several references have been made to comparisons between the International Geophysical Year and the IBP.

The budgets of several agencies contained a "line item" for the IGY; these items were defended by all and were supported by the Congress. With the words and thoughts that established the IGY and with its existence, a focus of purpose was established. As I understand the Subcommittee's intent, it is desired to establish a focus of purpose for the IBP and, in so doing, to seek some assurance that it can be defended without endangering the overall support of the IBP research effort.

In essence, the problem boils down to an exploration of the ways in which—

1. Additional funds can be secured to support IBP activities.
2. The specific purpose of the IBP can be highlighted in agency budgets.
3. The budgetary identification of IBP funds can best be safeguarded during the authorization and appropriation process.

Each of these three questions, of course, consists of two parts—one referring to the formulation of the President's budget for submission to Congress—the other to the Congressional actions on that budget.

As to the second part, that of likely Congressional actions, I am in no position to speculate upon this question. Certainly, the members of this Subcommittee are in a far better position to deal with this matter than am I. I would hope that a Subcommittee Report on the IBP would go far toward assuring sympathetic acceptance of this important program. I would also think that the judgments of the several involved agencies concerning the exact method used for highlighting the IBP in their budgets should be given due weight.

I have sought the advice of the Bureau of the Budget concerning the problem of highlighting the IBP in the President's budget.

I was told that, in principle, there is no objection to the identification of funds for a program such as the IBP. The fact that the IGY was circumscribed and involved a single year commitment whereas the IBP is intended to extend over a

longer period may pose certain difficulties. In general, however, rather than a single "line item" which would be highly vulnerable to paring, the inclusion of a series of specific programs in the budget which could be identified as IBP activities in an overall analysis was suggested as a feasible approach. Indeed, it was pointed out that such a procedure might very well be backed up by including a special analysis of IBP activities in the overall administrative budget, thus highlighting the program without making funds specifically vulnerable.

I neither sought nor received assurance that additional funds might be forthcoming for the IBP. This is a matter that will be up to the agencies involved as will be the presentation of arguments for according the IBP this type of identification.

Without going into further detail, I would say that report from this Subcommittee could be a major factor in assuring that the IBP would be accorded the attention it deserves.

How can the IBP budget be defended? On two broad premises and on one specific principle. The first premise is equated to the IBP theme. On this premise the usual question—can we afford to do it?—is immediately inverted and becomes—can we afford not to do it? In my first statement on this subject before this Subcommittee on 9 May I hope that I established the basis on which OST supports this program. But, more specifically and recasting more bluntly some of Roger Revelle's thoughts—if we believe that worthwhile effort advancing knowledge of the biological basis of productivity and human welfare can be expended; if we believe that it is possible to bring man in closer harmony with nature; if we recognize the worldwide threat that pollution in all its aspects has on the balance of nature and believe that we may beneficially influence or reverse these threats; if the world's population and its food supply are to be brought into better balance; and if we believe that science and technology are better prepared and poised to address these problems than ever before, then we thoughtfully support the IBP. The IBP promises significant progress in solving these problems and more.

My second premise is concerned with the international aspects of this program. Sixty countries are participating. I think that this premise is established with another simple question: Can we fail to participate and in that participation, include all the energy, knowledge, and scientific expertise that we can muster? I am well aware of the "do good" nature of this program, and of the hazards that "do-gooders" face. Careful nurturing and intelligent guiding can overcome such hazards. I am encouraged both by your interest in the IBP and in the progress that we have made.

With these two premises and careful application of judgment as to scientific merit we can assure creditable U.S. participation in this program.

Returning to my third question—what are the benefits that we can expect to accrue? The broad answers to this question have been given—advanced knowledge, man's better harmony with nature, beneficial management of our renewable resources, maintenance of U.S. prestige thru our creditable participation, and many others.

I wish that we could assure you of hard measurable benefits that will result from this program. Even though I am confident that with the care exercised in the choice of IBP work such benefits will be realized, we can do no more than briefly describe some of the work now planned, and point to the probability of benefits. The action program directors have furnished brief statements for this purpose. These statements are summarized below. Some are expanded and are attached in full context to this statement.

II. SUMMARY OF EXPECTED BENEFITS

A. Program to study drainage basins and landscapes

The goal of this program is to study intensively 6 biomes as total ecosystem entities. The first to be selected will be a grasslands area. In these studies, investigators will attempt to identify all interacting components, plant and animal, and the environmental factors that influence their interactions; and will attempt to qualify the interdependent relationships involved. Complex mathematical modeling will be undertaken. Validity of the modeling will be progressively refined with repetitive testing by selectively varying one or more factors.

The large scale benefits that may be anticipated seem to be—

- (1) A more precise knowledge of the interrelationships involved;

(2) An evaluation of primary and lesser factors influencing the inter-relationships; and

(3) A greatly improved ability to predict the results of planned, imposed, or uncontrolled variations in the overall operation of the ecosystem.

The implications for better resource management seem to fully justify this effort.

B. Program on aerobiology

This program proposes to correlate ongoing work and to promote needed investigations into passively air-borne microorganisms, and to relate these investigations to similar work in other countries and continents. The promise involved seems to be the acquisition of greater knowledge of such pathogenic organisms as attack wheat, rice, and other crops, such pests as locusts and sucking insects; bacteria, viruses, air-borne allergens, and insect vectors of human diseases. The primary concern of this program is to advance knowledge of methods of lofting, air transport, and deposition of these organisms. Implications seem to lead to:

- (1) Better predictive techniques of the spread of crop diseases and pests.
- (2) Probable identity of more vulnerable periods in the life cycles of pests and microorganisms leading to improved methods of control.
- (3) Better predictive techniques on the spread of bacterial, viral and fungal diseases.

C. Program on phenology

This program proposes to more explicitly describe the seasonal development of organisms and from these descriptions to better understand, interpret and predict biological events. A. D. Hopkins from 1905 to 1920 was very successful in working out the planting dates for winter wheat so that the seedlings developed after the last date for egg laying by the hessian fly (*Phytophaga destructor*). The larvae attack the stems of wheat and barley and are one of the most destructive pests in the raising of wheat. Hopkins utilized about 40,000 records of winter wheat planting and harvest dates in working out his fly free dates (based upon the life cycle of the insect and growth rates of wheat) for various parts of the country. From these charts one could predict the average best date for wheat planting to within a 10-day range. Hopkins developed bioclimatic laws and maps to predict the seasonal development of organisms across the country. Unfortunately this work has not been continued.

The work of Hopkins using insect and plant phenology and climatic records, enabled the U. S. to increase wheat production during World War I by a wiser selection of fall planting dates. The same principle has had other uses:

- (1) In predicting dates and intensity in outbreaks of wheat stem rust (*Puccinia graminis*) from Mexico to Canada.
- (2) Aiding in the determination of forest fire hazard based upon the dryness of the herbaceous layer and moisture content of tree leaves.
- (3) Determining the carrying capacity of range lands and the length of time stock can be permitted to graze based upon plant development, and
- (4) Studies on codling moth and chinch bug development in relation to temperature.

The determination of egg hatching and adult emergence in relation to climatic conditions permits better estimates of when the insects will be more troublesome.

It is hoped that more intensive studies, conducted in a variety of locations in this country and others will better enable man to understand and predict seasonal activity of key organisms. Some of the kinds of phenologic data that would be most useful include:

- (1) Predicting the time and intensity of fall bird migration, especially geese and ducks in relation to hunting season. The time of migration as well as the species that overwinter in a given location is largely controlled by fall weather patterns and intensity.
- (2) Some zoonotic diseases are carried north in the spring by birds migrating from the tropics. An accurate network to predict spring migration would enable man to better control diseases carried by these birds.
- (3) Some insects develop in the south and are then carried north and east by summer winds. A phenologic network which studied the rate of insect development in the south and the rate of insect spread from such areas would have great value.

(4) The developing program on aerobiology will be greatly aided by phenological studies on the a) species that produce pollen of an allergenic nature, b) plant pathogens, and c) harmful insects.

(5) Predict fruit and seed yields of native species (commercially important tree species to weeds) as well as crop yields based upon phenologic data throughout the range of key species.

(6) The development of phenologic maps that would predict, a) planting and harvest dates for crop and ornamental plants, b) outbreaks of plant and animal borne diseases, c) migration of birds, d) spawning of fish, e) emergence of aquatic insects. Such maps might in time, evolve into a phenologic Atlas that would be of value to farmers, agronomists, foresters, and many other groups of people. The Europeans have developed similar maps for planting and harvesting crops. This resulted from a cooperative phenologic network.

This desired information can only result from intensive studies on a few key species at selected locations (the intensive program) and from a North American Phenological Garden Network and a Survey Network. Such a program requires a great deal of cooperation and coordination. In turn the international exchange of information will be essential to the development of an efficient and accurate predicting system.

D. Program on environmental physiology

The program in Environmental Physiology is broadly concerned with environment-organism interaction at all levels of biological organization—from population dynamics to cellular reactions to stresses. In order to understand why particular organisms live in certain environments it is necessary to know the nature of their physiological adaptations. Studies of physiological adaptations of plants and animals can give clues as to how these organisms came, in the course of evolution, to be distributed as they are now; also such studies have predictive value as to how organisms may adapt to environments as they are changed by human influences.

Anticipated achievements

(1) Knowledge is needed of the physiological basis for geographic and ecologic distribution of closely related species and identification of genetic difference between local populations. For example, a comparison of estuarine and shallow-water marine animals will be made over a wide latitudinal range. Such latitudinal studies cannot be made by single investigators but rather require cooperative efforts among many laboratories and several countries. Studies of physiological variation in diverse populations are necessary to provide a rational basis for improved productivity of different habitats.

(2) Two Inter-American studies are proposed. One deals with species of expanding ranges. This is vital for both Americans as man continues to alter his habitats and many plants and animals are becoming "weeds" by expanding their ranges. Examples of such organisms are: mesquite, Acacia, house mouse, cockroaches, certain thorny leguminous shrubs. Knowledge of the physiological basis for expansion of range of such species, usually undesirable ones, is important in terms of productivity of different habitats. A related Inter-American project deals with the physiological basis for divergent and convergent speciation. Questions will be asked concerning the degree of occupancy of the same niches by related species, the similarity between ecological communities in similar environments on the two continents, the origin and relationships of similar genera and species in the Americas. Such knowledge is of evolutionary importance in providing baselines for planning man's future relations to his ecosystems.

(3) A program of great practical importance deals with chemical interactions between insects and plants and between plants. A new method of insect control may emerge from studies of terpenes and other organic products of plants which prevent development of pest insects by serving as hormonal analogues. For example, in the course of evolution some conifers have come to produce substances which protect the trees from insect attack. The fir balsam contains an extremely active analogue of the juvenile hormone of one family of bugs and has thereby achieved full and complete protection from a number of related insects. The protective compounds of a native tree on one continent against particular insects appear to be different from those of related trees on another continent. Chemical identification of the active substances for various plants against poten-

tial predators may be of great economic importance, especially for plants introduced from one continent to another.

A related problem concerns chemical interactions between plants as a means of regulation of growth of weeds and crops. For example, the exudate into air or soil of one species of plant, such as *St. Johnswort*, may prevent seedlings of that species from appearing in the vicinity of the parent. Also the exudates from one plant species may be toxic for plants of other species. The implications of such studies in plant chemistry for crop production and weed control are extensive. The evolution of such natural controls of both insects and plants has occurred within certain temporal and geographic limits and an international attack on these biological problems is necessary.

(4) It is of little use to increase food productivity if the world population increases at a faster rate. The EP program anticipates collaborative studies in different cultures of the physiology of human reproduction from the view points of differential fertility, fecundity, age span of reproduction, placental properties and the like.

Another aspect of proposed population studies relates to population dynamics. There is a tendency to extrapolate from studies on the growth and regulation of animal populations (in both laboratory and field) to human populations. The validity of such extrapolations has important implications for population planning on a world-wide scale.

E. The Hawaiian terrestrial biology program

The participants in this program will study intensively the endemic and invading species of the Hawaiian Islands. Already, more Hawaiian plants and animals have become extinct than the total lost for the entire North American continent. However, most of the species still remain, and many of them can be saved with scientific understanding of the interactions between the plants and animals and their changing environments, especially those environments which are influenced by man. A great many others will inevitably disappear in the course of the next few decades, and the native communities which they compose will undergo drastic reduction and change. The whole of the rich Hawaiian flora and fauna evolved from about 700 immigrant ancestral species, under conditions of extreme isolation, in the course of a few million years; what happened might be called an evolutionary explosion, and Hawaii constitutes an unsurpassed natural laboratory for the study of evolutionary and ecological phenomena. Because of the rapidity with which native species and communities are disappearing, it is urgent that an intensive study of the Hawaiian biota be carried out while there is still time. The primary purpose of the Hawaiian Terrestrial Biology Program is to increase our knowledge of how evolution works and how biotic communities are organized and function.

The specific areas of research are as follows: Genealogy and demography; genetic markers; anthropometry; epidemiology; nutrition; physiology; behavior, attitudes, and mental illness; ecology and microclimatology (impact of ecosystems on man); and population history.

III. DESCRIPTION OF ACTION PROGRAMS

A. Approved by the U.S. national committee

1. Aerobiology program

A large segment of the population is allergic to pollen grains, fungus spores, algae, and other airborne allergens. Public health agencies need information on the patterns of distribution of all allergens as affected by meteorological conditions. Because these allergens are so widespread, attempts to reduce their number by chemical measures have met with little success.

Insects are pests of man, animals, and plants, and many species of insects serve as vectors of virus and other diseases. Insects are scattered widely by wind currents.

Gene flow in populations often occurs as a result of atmospheric dispersal of pollen, insects, plant pathogens, and other biological materials to new areas.

Although there has been some research on dispersal patterns of pollen grains, spores, plant pathogens, and insects, relatively little is known about their distribution patterns as related to air movements (including movements of upper air masses). Information is needed on sources of production of particu-

lates, the processes and factors affecting lofting, the trajectories while airborne, the effects of concentration gradients and diffusion, the factors affecting deposition and viability, and phenology or time relations.

It is essential to conduct research on a worldwide basis to learn about the spore loads—their kinds, their viability, their pathogenicity, and their distribution patterns.

The International Biological Program provides means for standardizing observations and for establishing communication between investigators, nationally and internationally. The IBP also furnishes a framework for interdisciplinary cooperation (meteorology, oceanography, ecology, etc.). These advantages will greatly enhance prospects of success in solving problems of gene flow resulting from atmospheric dispersal of biologically significant materials.

In the U.S. an effort will be made to get scientists with existing projects to standardize observations and reporting and to initiate needed projects, so that a sound and comprehensive national program can be established. The program will be coordinated with its counterparts in Europe, Australia, Japan, and elsewhere.

IBP unit involved.—Aerobiology Panel of the U.S. National Committee, IBP.

Funding.—To be developed.

Director.—William S. Benninghoff, Botany Department, University of Michigan, Ann Arbor, Michigan.

Coordinator.—To be selected.

2. Analysis of Ecosystems (*Drainage Basins and Landscapes*)

The Terrestrial and Freshwater Productivity Subcommittees have jointly proposed the study of large ecosystems, such as whole drainage basins and landscapes. The program consists of (1) six major studies for each of six biomes and (2) an unspecified number of contributory studies. The goal is to clarify the operation of ecosystems. Phases studied will include the dynamics of interchange among all components, the mechanisms of homeostasis, the capacity to change, and the levels of productivity sustained under a variety of natural and disturbed conditions. The major studies will permit the synthesis of information at the system level; the contributory studies will provide comparative data and broaden the scope of application of results.

Success depends upon the coordinated efforts of investigators from many disciplines and upon meaningful collaboration among them. Hence the subcommittees propose a task force including a program director, a program coordinator, a secretary, and the members of the subcommittees. This group will work intensively on the design and development of system-level research, and will promote and coordinate collaborative projects. It will try to accelerate ecosystem analysis, which is proceeding in several research centers, but too slowly to provide the understanding of ecosystems that is needed today. At least 2 years will be needed before an effective evaluation of this approach can be made.

The bulk of support for this program will be sought separately by agencies sponsoring sites and by investigators wishing to participate. The major costs of administration will be included in such requests.

IBP units involved.—Subcommittee on Terrestrial Productivity and Subcommittee on Freshwater Productivity, U.S. National Committee, IBP.

Funding.—Pilot research supported by grant to University of Michigan, Ann Arbor, Michigan.

Director.—Frederick E. Smith, School of Natural Resources, University of Michigan, Ann Arbor, Michigan 48104.

Coordinator.—To be selected.

3. Study of Eskimo Populations

Because Eskimo populations are distributed in a series of communities all stemming from the same origin, but are increasingly isolated from one another, these populations are particularly appropriate for studies of human adaptability.

The aim of this program is to carry out intensive, multidisciplinary studies on at least three Eskimo centers of population and to coordinate the studies so that:

Each study within a given discipline will have scientific interest and merit for that discipline.

Data on interactions within a community, and on interactions between the community and its environment, will provide improved understanding of the productivity of the community and of the way in which it utilizes its environment.

Comparisons of communities will bring out differences resulting from different environments as well as similarities that exist despite the different environments.

The three major populations to be studied are: Alaskan Eskimos at Wainwright, a Canadian Arctic Eskimo community, and the Upernavik Eskimos of Greenland.

The specific areas of research are as follows: Genealogy and demography; Genetic markers; Anthropometry; Epidemiology; Nutrition; Physiology; Behavior, attitudes, and mental illness; Ecology and microclimatology (impact of ecosystems on man); Population history.

Details concerning seven of these areas follow:

Genealogy and Demography.—Because the populations are small, a detailed description of genealogies will be made to provide the basis for identifying and coding individuals. Later the genealogies will be verified by genetic markers.

Nutrition.—Aspects to be included are: Clinical examinations for signs of specific deficiencies, laboratory examination for signs of specific deficiencies (hemoglobin, serum iron, serum proteins, blood levels of vitamin C, folic acid, vitamin B₁₂, and serum lipids), estimation of caloric intake by major food types and season variations (dietary histories, diet analysis, 24-hour urinary nitrogen excretion, etc.), use and management of local food sources, distribution of food in the community, and type and quantity of food brought into the community.

Physiology.—Aspects to be included are: Cardiovascular function, respiratory function, adaptations to heat and cold, work capacity, thyroid and other endocrine studies, and carbohydrate and protein digestion, absorption, and metabolism.

Behavior, Attitudes, and Mental Illness.—Aspects to be included are: Observation of people during their daily activities, use of diaries and activity logs, use of heartbeat, totalizers and other devices, systematic study of cultural values, testing attitudes and aptitudes, and interviewing by psychologists or psychiatrists.

Genetic Markers.—These consist of data that should be collected on the total population. In one group are blood group antigens, serum enzymes, serum haptogens and hemoglobinopathies, salivary substances, urinary substances, and hair characteristics. In another group are dermatoglyphics, taste testing, chromosome characteristics, special dental features, and special eye features.

Anthropometry.—Aspects to be included are: Physical anthropometry (head, face, body, limbs, and skinfolds), photographs, X-rays (head, teeth, joints, and spine), and bone density measurements.

Epidemiology.—Aspects to be included are: Nutritional diseases, infectious diseases (viral, bacterial, and parasitic), metabolic and degenerative disorders, neoplasms, injury and accident, environmental factors and pollution, mental and behavioral disorders, and principal causes of morbidity and mortality.

The study of Eskimo populations is a joint United States-Canadian program. It will be coordinated with similar programs conducted by Scandinavian scientists in Greenland and Lapland, by Japanese scientists in -----, and by scientists of the USSR in the Siberian Arctic.

The following scientists have planned the U.S.-Canadian program:

J. A. Hildes, member of the Subcommittee on Human Adaptability, Canadian National Committee for the IBP (Chairman);

Frederick Sargent, II, Chairman of the Subcommittee on Human Adaptability, U.S. National Committee for the IBP;

J. S. Hart, Chairman of the Subcommittee on Human Adaptability, Canadian National Committee for the IBP;

William S. Laughlin, member of the Subcommittee on Human Adaptability, U.S. National Committee for the IBP;

L. Irving, Institute of Arctic Biology, University of Alaska; and

Frederick A. Milan, Aeromedical Laboratory, Fairbanks, Alaska.

This group has held two meetings and plans to hold a working conference in November 1967.

IBP units involved.—Subcommittee on Human Adaptability, Canadian National Committee for the IBP, and Subcommittee on Human Adaptability, U.S. National Committee for the IBP.

Funding.—To be developed.

Director.—To be selected.

Coordinator.—Frederick A. Milan, Aeromedical Laboratory, Fairbanks, Alaska.

4. *Hawaiian terrestrial biology program*

The Hawaiian Terrestrial Biology Program will be a detailed, long-term, comprehensive investigation of the endemic and invading biotas of the Hawaiian Islands. These biotas are among the most critical in the world, because of their exceptionally high endemism and the rapidity with which they are disappearing before the onslaught of man and introduced species of plants and animals. The islands are outstanding as a natural laboratory for the study of evolutionary processes.

A planning committee for this program met with local biologists at the Bernice P. Bishop Museum in Honolulu, March 20 to 23, 1967. Members are Sherwin Carlquist, Richard S. Cowan, F. R. Fosberg, Hermann T. Spieth, Robert S. Usinger, T. H. Hubbell, and Warren H. Wagner, Jr. Dr. Wagner has been named by the U.S. National Committee to be director of the program.

The following summarizes the decisions made at the meeting in Honolulu:

Scope of the Program.—It was decided that the program should be limited to the biology of the land areas, including the leeward islands, and that the "cut-off point" should be the shoreline. The principal basis for this decision was simply that the terrestrial biotas are the ones most severely threatened and most likely soon to be destroyed. Furthermore, work on the marine biology of Hawaii is already reasonably well supported.

Determining Priorities.—It was concluded that the following considerations should be weighed together in determining the priorities of studies to be made:

To what extent is destruction in the near future likely? This may be judged by considering (1) rarity, or narrow geographical extent, and (2) narrowness of ecological specialization.

To what extent does the group of organisms, the community, or the ecosystem have special biological interest and significance? Is the taxon, community, or ecosystem unusually distinctive? Has the taxon undergone unusually vigorous evolution in which important biological principles may be involved? Does the taxon, community, or ecosystem show unusual biological adaptation or specialization?

Among the insects, the longhorned beetles (Cerambycidae) of Hawaii call for detailed investigation. They constitute an outstanding example of evolutionary development in insular isolation. They include 110 or more species in 6 endemic genera, all of which apparently evolved from a single immigrant. Very little is known of their biology, ecology, or immature stages, and a concentrated study would produce results of great interest.

Among the mollusks, the Hawaiian members of the genus *Achatinella* have long been cited as classic examples of evolution. In recent times the American Institute of Biological Sciences has requested color slides of *Achatinella* for teaching purposes, and there has been a resurgence of interest in these snails. Detailed biological investigations should be made now, for we are witnessing the rapid decline and disappearance of these organisms as a result of disease, dilution of the native forests, rats, and other factors. perhaps ways and means should be formulated to save the most adaptable species for prosperity.

Among the birds, the Hawaiian Honeycreepers (Drepaniidae) provides the most striking examples of adaptive radiation. We know virtually nothing about the ecology and breeding biology of any species of this strictly endemic family. Some of the species are already very rare, and a few may be extinct.

The following genera are among plant groups that should have high-priority study: *Gouldia* (Rubiaceae), *Euphorbia* (Euphorbiaceae), *Cyanea* (Campanulaceae, subfamily lobelioidae), *Adenophorus* (Filicaceae, Grammitidaceae), and *Metrosideros* (Myrtaceae), the endemic genus *Gouldia*, although considered in the most recent monograph as comprising only three species, has generated an almost unbelievable number of varieties and forms, all of which seem to hybridize wherever their ranges overlap. *Gouldia* provides ideal material for studying the evolution of plant groups on volcanic islands, but many of its forms have already disappeared or are in danger of extinction.

Effect of Invasive Species.—Over the past century the spread of introduced species of plants and animals has had spectacular effects on the terrestrial biology of Hawaii. The introduced species have invaded environments formerly occupied by native organisms, and have changed or eliminated native communities and ecosystems. Among the exotic invaders are mites, ants, axis deer, mongoose, pigs, goats, blackberry, lantana, pamekane, Christmasberry, and gorse. Two

species of guava have established themselves and taken over great areas of formerly native forest.

Native Communities and Ecosystems.—Special attention should be given to communities and ecosystems in which the effects of modern destruction are minimal these include rainforest, bog, dry forest, and sand dunes.

The dry forests that remain in the most primitive condition are on the island of Hawaii. The Waikamoi Forest area of East Maui stands out among the rainforests because of its relatively undamaged condition, its altitudinal range, and its great size. The planning committee felt that if it proves desirable to set aside a single area for intensive ecosystem investigations by several subcommittees of the U.S. National Committee for the IBP, the Waikamoi Forest would be especially suitable.

Development of Lists, Manuals, and Bibliographies.—The planning committee recognized that progress on the Hawaiian program will require proper source materials. A considerable amount of material has already been developed; what is now needed is to collate and publish it. Various authorities have made lists of particular groups for their own use, which are not known or available to others; it is necessary now to bring these together and revise them. Manuals are greatly needed.

Development of an annotated, cross-referenced bibliography on the terrestrial biology of the islands is proposed. Out of this work of correlation it should be possible to prepare lists of threatened taxa as well as lists of threatened habitats and communities. Such lists might lead to recommendations of sites to be designated as natural area preserves.

IBP units involved.—Subcommittee on Systematics and Biogeography and Subcommittee on Conservation of Ecosystems, U.S. National Committee, IBP

Funding.—To be developed

Director.—Warren H. Wagner, Jr., Department of Botany, University of Michigan, Ann Arbor, Michigan 48104

Coordinator.—To be selected

5. Phenology program

Man has always been interested in the natural events in his immediate surroundings, and this interest gives him knowledge of direct use; for example, he can predict insect outbreaks, manage ranges and pastures, and recognize periods when forest fires are likely to occur. Phenological studies may serve to give modern man a deeper understanding of biological phenomena.

A broad-scale study of phenology is planned as a major phase of the IBP activities in which the Terrestrial and Freshwater Productivity Subcommittees are to be engaged. Phenologic investigations are particularly appropriate under the IBP. Investigations can be carried out at several levels of sophistication, and many people with different training and divergent interests can participate.

The program will include intensive and extensive studies on plant and animal species both aquatic and terrestrial. The intensive studies, at a select number of sites throughout the country, will encompass detailed measurements of environmental parameters and an investigation of population structure, breeding behavior, and physiology of organisms in addition to the detailed phenologic observations. The species studied should provide a basis of explanation for the geographic and temporal pattern of phenological events revealed by the extensive program.

The extensive studies will be conducted with the aid of a large network of stations and observers. Seasonal and annual observations require well defined scoring systems and specific individuals marked for observations. Species selected should be easily identified and widely distributed.

Some of the objectives of this program are (1) preparing phenological maps, which will summarize phenological events for species or groups of species (leaf enlargement, fruit and seed production, nest building, fish spawning); (2) providing phenological clues to the physiologic limitations that determine the climatic limits of species; (3) conducting studies that will aid in explaining the biological basis for phenological timing; (4) testing the hypothesis that phenological studies will aid in understanding evolutionary mechanisms; and (5) contribution to understanding the role of phenology in community structure and productivity in ecosystems.

Selection of species for both extensive and intensive phenologic studies is the first step toward standardized methodology in this program. Tentative selections have been made as follows:

Species suggested for intensive study

Birds:

Passer domesticus—house sparrow
 Progne subis—purple martin
 Zenaidura macroura—mourning dove
 Turdus migratorius—robin

Mammals:

Microtus spp.—meadow voles
 Peromyscus spp.—white-footed mice

Herps:

Amblystoma tigrinum—tiger salamander
 Thamnophis spp.—garter snakes

Fish: Carpio carpio—carp

Plants:

Asplenium pinnatifidum—spleenwort
 Oxalis pes-caprae—Bermuda buttercup
 Phragmites communis—common reed
 Populus alba—silver poplar
 P. nigra italica—Lombardy poplar
 Pteridium aquilinum—bracken

Species suggested for extensive study

Birds:

Agelaius phoeniceus—redwing blackbird
 Turdus migratorius—robin
 Zenaidura macroura—mourning dove
 Richmondia cardinalis—cardinal

Mammals: Odocoileus virginianus—whitetail deer

Insects:

Pieris rapae—white cabbage butterfly
 Papilio glaucus—tiger swallowtail
 Papilio marcellus—zebra swallowtail
 Leptocoris trivittatus—boxelder bug
 Lygaeus kalmii or Oncopeltus fasciatus—milkweed bug

Plants:

Acer platanoides—Norway maple
 Aesculus hippocastanum—horse chestnut
 Ailanthus altissima—tree-of-heaven
 Berberis thunbergii—Japanese barberry
 Betula alba pendula—weeping birch
 Cichorium intybus—common chicory
 Cirsium arvense—Canada thistle
 Dactylis glomerata—barnyard grass
 Daphne mezereum—Mezereum
 Eleagnus angustifolium—Russian olive
 Forsythia suspensa—Forsythia
 Ginkgo biloba—Ginkgo
 Hedera helix—English ivy
 Hermercallis fulva—orange day-lily
 Lonicera japonica—Japanese honeysuckle
 L. tartarica—Tartarian honeysuckle
 Melia azedarach—China-tree
 Metasequoia glyptostroboides—dawn tree
 Nerium oleander—oleander
 Nuphar variegatum—bullhead lily
 Oxalis pes-caprae—Bermuda buttercup
 Philadelphus lewisii—mock-orange
 Phragmites communis—common reed
 Plantago lanceolata—ribgrass or English plantain
 P. major—common plantain
 Populus alba—silver poplar
 P. nigra italica—Lombardy poplar
 Prunus tomentosa—Nanking Cherry
 Spiraea vanhouttei—Spiraea
 Syringa vulgaris—common lilac
 Verbascum blattaria—moth-mullein
 V. thapsus—common mullein
 Vinca minor—lesser periwinkle

Fungi:

Polyporus sulfurea—sulfur mushroom

Coprinus ornatus—shaggy-mane mushroom

IBP units involved.—Subcommittee on Terrestrial Productivity and Subcommittee on Freshwater Productivity, U.S. National Committee, IBP

Funding.—To be developed

Director.—To be selected

Coordinator.—To be selected

6. Ecology of Migrant Populations

The rapid technological, socioeconomic and demographic changes of the recent past have profoundly altered the nature of physical and psychological stresses operating upon many human populations. Some stresses, such as bacterial diseases, have diminished in importance; others, such as noise, mechanical injury and air pollution, have intensified. Medical intervention is saving and repairing an increasing number of persons with serious congenital defects. Dietary changes and decreases in physical activity have introduced new stresses; industrialization and mass communications have caused unparalleled demands upon symbolic learning, memory and fine psycho-motor coordination. The tolerances of man to these changes and the degrees of hazard, on one hand, and of potential, on the other, that they may present to development, physiology, behavior, longevity and genetics in man are still poorly understood. Their evaluation consequently represents a key task in understanding human adaptability.

The changes described are largely encompassed in the process of urbanization, and particularly affect migrants from farm to inner-city localities. For this reason, a series of comprehensive bio-social assessments of selected migrant populations is planned as part of the United States contribution to the International Biological Program. Bio-social assessments constitute quantitative ecological descriptions of a group's size and composition, biological (including genetic) and sociopsychological characteristics, habitat, and major behavior patterns so designed as to (a) identify and measure that group's constituent biological populations, and (b) the levels of fitness, adaptive mechanisms and selective pressures characterizing each population. These assessments would be applied to (a) migrant (especially rural) source communities that are relatively homogeneous in race and socio-economic status, of statistically adequate size and subject to significant out-migration to urban areas; and (b) the migrating components of these communities in receiving areas especially urban. Specific sites are to be located in rural Mississippi among Negroes and among whites, and in receiving areas, especially Chicago. Other studies are planned upon the base and migratory components of Spanish-American and American Indian populations.

A study of migrants and their relatives in source areas for the city of Fairbanks, Alaska also appears desirable, since it would aid in understanding similarities and differences in White, Eskimo and Indian adaptations to the special environment of a sub-arctic city. It is also hoped to correlate these studies with research under the IBP on migration to Israel, and perhaps in Canada (Ottawa-Hull and Yellowknife areas).

Description of Studies.—The researches contemplated are to be prospective studies of at least three years' duration. They would necessitate a large variety of observations and measurements. To estimate fitness in genetically defined populations, both cross-sectional and longitudinal indications would have to be marshalled. The former include age-and-sex specific data on height, weight, nutritional status, physical maturation and aging, physical work capacity, heat and cold tolerance, glucose tolerance, allergic sensitivity, vision, hearing, dexterity, cognitive capacity, etc. The latter are age-and-sex specific actuarial measures of fecundity, morbidity, accidents, behavioral disturbances, and mortality.

Fitness in given populations is always relative to specific types and intensities of selected pressures and a particular inventory of adaptive processes. Selective pressures may be conceived as imbalances between an individual's conditions and capacities, on one hand; and on the other, his psychological needs and socio-psychological anticipations. They can be inferred, on the basis of biological and socio-psychological theories, from evidence on an individual's (a) locus and associated micro-environmental exposures to temperatures, light, dust, pathogens and allergens, etc.; (b) behavioral; rhythms and activity patterns; (c) group and intimate community status; (d) nutrition; (e) explicit goals, and (f) projections and fantasies. It must be stressed that even in families selective

pressures vary greatly because of role differences; age; sex and other genetic factors; and variations of locus and activity patterns.

To identify adaptive mechanisms, i.e., the behavioral, physiological and ultimately morphological responses stimulated by the pursuit of goals or by attempts to manage stresses, there are needed observations and analyses of (a) manifestations of basic drives (hunger, curiosity, aggression, sexual satisfaction, play, etc.); (b) stressful events (illness, pregnancy, birth, death, conflicts, etc.); and (c) adaptive breakdowns, both physical and behavioral. Adaptation involves analysis at the levels of inter-acting groups and generational changes, as well as that of the individual, since behavioral adaptation is normally a group response and many responses (mating patterns, differential fertility) have genetic expressions.

The complexity of the work contemplated calls for a deliberately phased undertaking. Its basis will be a series of comparable studies executed by field and visiting specialist teams under individual institutions, and coordinated by a Program Director, aided by senior Working Groups (on-site selections, methods, data processing, etc.), and by a Central Office providing a data bank, etc.). To the maximum degree, use would be made of methodological data from SCIBP and of environmental information from other IBP studies. However, a good deal of new methodological and instrumental work will undoubtedly be needed, e.g., to gain effective assessments of urban micro-environments. Finally, while the basic program would be observational, a number of experiments, e.g., increments in child care resources, or nutritional improvements could well be integrated into studies once base-line data had been established.

Potential Value of the Studies.—It is hoped that the proposed research on the Ecology of Migrant Populations will—

(a) Identify and test extensively, a variety of observational techniques and measures of fitness, stress and adaptations in man.

(b) Develop a large body of factorial information on the bio-social characteristics of American populations and environments. Such data would have distinct value for many medical and socio-economic applications, as well as for fundamental knowledge.

(c) Aggregate the data into systems and to some measure, predictive models placing hitherto isolated human ecological phenomena in more meaningful contexts. This would improve both basic understanding and possibilities for effective management, especially city and regional planning, including that of health and education.

(d) Identify some specific areas—in which intervention for human welfare may be desirable or conversely, highly undesirable, e.g., the estimation of maximum tolerable levels for noise, or in mobility for given ages and other population components.

(e) Serve as the foundation for associated research undertakings on specialized theoretical or practical problems, e.g., an operational understanding of "poverty."

In sum, the program envisaged and initiated is both difficult and potentially rewarding, theoretically and practically. It is urged that biological and behavioral scientists interested in contributing to this effort through the affiliation of existing projects, the design of new ones, or in other ways, communicate with the Program Director: Dr. Adrian Ostefeld, Head, Department of Preventive Medicine and Community Health, University of Illinois College of Medicine, Chicago, Illinois.

B. Emerging programs still being developed

1. Adaptation of Peoples at High Altitudes

Effort sponsored by the Human Adaptability Subcommittee under Dr. P. T. Baker, Pennsylvania State University, as the working group convenor. It seems probable that the working group to be convened in November 1967, will develop the basis for a group of related coordinated and collaborative proposals involving both South and North American countries.

2. Latitudinal and Longitudinal Variations in Marine Species

A working group convened by Dr. F. John Vernberg of Duke Marine Laboratory in March has resulted in three individual, but related proposals. The idea of further probing for additional proposals on a coordinated basis is being assessed. Both the Environmental Physiology Subcommittee and the Productivity of Marine Communities Subcommittee are providing guidance.

3. *Interactions Between Plant and Insects and Between Plant Species*

A preplanning group convened by Dr. E. F. Knipling of the U.S. Department of Agriculture, Beltsville and Dr. Cornelius Muller of the University of California, Santa Barbara, will hold a 5-day working conference at Santa Barbara in the spring of 1968. It seems probable that a least one action program will develop from this conference since widespread interest is increasing.

4. *Adaptive Processes of Hybrid Populations*

A study or group of studies of U.S.-Japanese population groups in both Japan and the United States sponsored by the Human Adaptability Subcommittee. The HA Subcommittee predicts that the program will develop.

5. *Adaptive Processes of Primitive Populations*

Sponsored by the Human Adaptability Subcommittee. Status is similar to Hybrid Populations described above.

6. *Convergent and Divergent Evolution and Physiology of Colonizing Species*

Working group conferences on these two subjects will be held in Caracas, Venezuela, 22-24 November 1967. The first will be chaired by Dr. W. Frank Blair, the second by Dr. Calvin McMillan. Participants will be from the U.S., South American countries, and from other countries. Preplanning indicates that "action programs" will develop in both areas.

IV. A. PROGRAM TO STUDY DRAINAGE BASINS AND LANDSCAPES, BY FREDERICK E. SMITH, UNIVERSITY OF MICHIGAN

Scientific goal

The central goal is to understand how ecological systems operate. To date, we have analyzed a number of aspects of different systems, such as the kinds of organisms present, their abundances, their arrangement in space, the food they eat, the flow of energy through the system, and the cycling of nutrients. Although we can describe *what* happens, we cannot explain *why* it happens: why so many different species live together, why so much plant production is not eaten by animals, why mortality rates are so high among most herbivores, or why systems persist year after year although their populations fluctuate greatly. The answers to such questions lie in the structure and function of the systems as wholes. They cannot be answered by analysis alone, but require also a synthesis of information into working models of whole systems.

This work must be done in whole, functional systems. If portions are isolated, or brought into the laboratory where better analysis can be done, each isolate becomes a whole, and functions as if it were a complete system. Attempts to synthesize models of large systems using information from isolated subsystems can be extremely misleading. For example, the behavior of a laboratory system of predator and prey populations may be utterly unlike the behavior of those same two components in a much larger system.

To achieve our goal, many ecologists must collaborate. Furthermore, since traditional scientific training emphasizes analysis, not synthesis, an intense intellectual effort is needed. If we are successful, a new subject area will be established: principles of ecological systems. The scientific value of this goal is demonstrated in the broad support this program receives from leading ecologists. Already, enough talent is available to ensure a high probability that the program will be successful.

Beyond the satisfaction of intellectual curiosity, a number of other benefits will derive from this program. They are presented under three general headings: biological production, resource management, and environmental quality.

Biological production

A major theme of the IBP is biological production. A major set of data in this program is the production achieved by each species within each ecological system. Productivity is one criterion by which systems as different as grasslands, forests, and lakes will be compared. The program as a whole will provide extensive information on natural production. In collaboration with other countries we will be able to assess natural production on a worldwide basis. Production measurements will include not only calories, but also protein content, and any other component that is deemed significant. Similarly, production by mammals,

fish, and insects will be estimated as well as production by plants. All of the kinds of production that are significant in man's economy are also significant in the analysis of ecological systems.

Of greater significance will be the knowledge gained on interactions between species within systems. We know already that green plants, herbivores, predators, and decomposers are all interconnected. Production at one level may be influenced greatly by activities in other parts of the system. As yet we know very little about such processes. Studies with mathematical models of systems suggest that interrelations may be much more pervasive than we appreciate. In models, for example, the presence or absence of predators has a profound effect upon the economy of not only the herbivores, but also of the plants, and even of the decomposers. Such repercussions undoubtedly exist in natural systems. They must also have their counterparts in agricultural systems, and they have strong implications for the field of biological control.

The proposed research includes extensive manipulation in ecological systems, since their responses to treatments comprise a powerful method of analysis. Treatments affecting production are among the most critical manipulations. For example, in the proposed grassland study, the performance of the whole system will be studied under various levels of grazing, fertilization, and irrigation. The responses of insects, rodents, soil fungi, etc. will be studied as well as those of grass and cattle. Thus a classical experiment in range management is incorporated in the design. Since irrigation adds a dimension not often used, these experiments will contribute directly to our knowledge of range management. In addition, we will learn where those nutrients go that fail to show up in beef, and will gain a much more complete picture of the entire grassland system. Knowledge of production in all components of the system will surely improve our understanding of production in particular components, even though the research required is several times more complex than that of the best studies to date on range management.

Manipulations of this kind will be repeated in the other ecological systems. They will yield information on methods for increasing production, as a by-product of each program. Thus, in addition to our assessment of the present level of worldwide production, we will learn a great deal about its potential for improvement in the future.

Resource management

All management programs for living resources contain, somewhere in them, ecological concepts that attempt to describe the response of the managed population to its management. At present, the ecological theory that serves this function relates to individuals or to populations, not to systems as wholes. This has been reasonably satisfactory so long as the goals of management have been centered on single species or populations. In many cases, such programs may be satisfactory for years to come. Our contribution to this approach rests in the manipulation planned in ecological systems, and the production data that will result. The relation of such experiments to programs of resource management are obvious.

In addition, we hope to make a contribution of another kind. The multiple use of natural resources is increasing steadily, often combining timber production, hunting, fishing, recreation, etc. Management programs are in use that attempt to optimize or maximize these several uses simultaneously.

These programs are complex enough to involve systems analysis, and have developed very well in the direction of including the human socioeconomic systems in the program. The ecological base, however, remains largely a collection of principles at the population level, representing an aggregate of unrelated populations rather than a system.

The principles of ecological systems that we will produce will serve as a much more appropriate theoretical base on which to design complex management programs. They will be much more amenable to problems of multiple use, much more reliable for predictive purposes, and much more adaptable to changes in use that may occur with time.

Management practice is, in fact, a manipulation of an entire ecological system, whether it is conceived as such or not. Thus, the only level of ecological theory that is completely compatible with management is at the system level. Even simple problems involving a single use may be solved more reliably if they could be framed against the operation of the entire system.

It is probable, therefore, that this program will revolutionize resource management. Once we are familiar with the principles of whole ecological systems, we may find that management has become both simpler and less expensive than it is at present. If it is also more effective, the practical gains from this program will be enormous.

Environmental quality

A second major theme of the IBP is human welfare. One aspect of this is its relation to biological production and resource management, already discussed. Another is the growing concern with the quality of the environment.

A rapid deterioration of the environment is often due to obvious causes that can be corrected if man decides to do so. The solution of these problems seldom requires more than the most elementary level of ecology, and will not be discussed further here.

Of equal concern are the less visible, more chronic, insidious changes that take place over decades. Many phenomena of varying importance to man can be listed: the thinning of the soil, the increasing need to spray crops, the increasing abundance of weeds, the dense algal blooms in lakes, the decreasing abundance of sport fish, and the changes in species of birds in our towns. These and many others combine to produce profound changes over long periods of time. Often they are difficult to document, and equally often it is difficult to obtain action to prevent them.

Many of these problems are deeply rooted in ecological systems. They are system responses in which the visible effect appears in one part of the system while the cause lies in another. The relation between cause and effect, and recommendations for improvement, would be much easier to determine if the operations of ecological systems were better understood.

As a simple example, in crude models of aquatic systems, the addition of nutrients inevitably leads to increased algal blooms, no matter what species of algae are included. With continued nutrient enrichment, the oxygen balance in the water invariably deteriorates, making the system less hospitable to many fish. Simulated treatment with herbicides, or cropping algae mechanically, produce surprisingly small changes for large inputs of effort. Such treatments are attempts to fight the system. If some species of algae are reduced, others take their place. These are symptomatic treatments that do not attack the source of the problem. On the other hand, if the nutrients are reduced, the system itself recovers its former state.

While natural systems may behave differently from the model, if we knew how they operated we could determine ahead of time whether a treatment would be effective, and more easily focus on the source of the problem. Some ponds and lakes have responded exactly as the above model predicts.

Systems analysis is already being employed in major attempts to improve the quality of the environment. A variety of social, economic, industrial, and hydrological systems may be combined. Such programs would be much improved if biological systems were included. At present the ecology in these programs, like its counterpart in management programs, is represented by collections of population concepts, not system concepts. In some programs no ecology at all is included.

An understanding of ecological systems will provide the basis for designing more effective methods of restoring beauty to the landscape. To the extent that environmental restoration has become an urgent problem in some localities, this research program becomes equally urgent.

Dollar value

We believe that, if this program did not exist, a major part of the proposed research would be completed in fifteen to twenty years rather than five. Thus, one effect of the program is a saving of ten to fifteen years. A second effect will be a better national organization of ecological research facilities than would otherwise develop.

The cost of this program is estimated at 45 million dollars for the five year period. The dollar value of this investment, in terms of savings in other programs, can be estimated in many ways. Two examples are offered here.

Federal agencies alone spend half a billion dollars a year in the management of such resources as fish, game, forests, and soil. A better knowledge of ecological systems should make at least a 20% improvement in these programs. That is equivalent to 100 million a year, which becomes one billion for the ten years of

time that have been gained. Even if this program produced only a 1% improvement in management programs, it would pay for itself.

The cost of cleaning up our waterways has been estimated at 80 billion dollars. Most of this will go into urban and industrial problems. If only 10% of the cost involves ecological systems, then each 1% improvement in this portion of the program is worth 80 million dollars.

Similar values will accrue from programs to restore the quality of the terrestrial environment and of the atmosphere, from the state programs on natural resources, and from programs concerned with biological production. Beyond this, the international value of the program should be considered. An improvement in the effectiveness or reliability of development programs in other countries will be of immense value to the peoples of the world.

Note: The "pay-off" statement claims that the federal government spends half a billion yearly in the management of resources such as fish, game, forests, and soil. This figure is approximately the sum of the line items from the various agencies for resource management, including only those that seem to fall into the category involved. Various items can be left in or out, but half a billion looks like a reasonable figure.

B. THE AEROBIOLOGY PROGRAM, BY DR. WM. S. BENNINGHOFF, UNIVERSITY OF MICHIGAN

It is well known that a wide variety of various small organisms, microbes, spores, pollen and tiny insects, are carried by currents of air, sometimes in very large numbers and for long distances. Most people are aware of the significance of such biological dispersal in relation to the spread of many infectious diseases of plants and animals, but only workers in the field of aerobiology appreciate how little grasp we have of the phenomenon of atmospheric dispersal and the urgency of our need for this knowledge in order to take measures to control the problems. Aerobiology is the study of passively air-borne microorganisms; it comprehends and identifies, lofting, movements in the atmosphere, viability and landing. The atmospheric conditions that influence the lofting and transport are also studied. The ultimate questions are those of the practical consequences of the atmospheric dispersal for the organisms transported and for the other organisms they may influence by distributing new genes, or by parasitizing them, or by entering into some other ecosystem relationship with them. Crops, natural vegetation, man and other animals are all affected to some degree by these microorganisms transported in the atmosphere.

Some of the most costly of crop plant diseases are spread by air-borne spores, extensive pure cultures, such as fields of cereal grains, lead to massive local infections that yield fantastically large spore loads into the atmosphere. Wheat rust spores are produced each year first in Mexico, then they infect areas successively farther north through the grasslands of the U.S., and ultimately through the prairie provinces of Canada. The losses from plant disease are difficult to calculate, but they run into hundreds of millions of dollars annually in spite of enormous expenditures on control efforts. New chemical fungicides, bactericides, and insecticides seem to promise solutions, but in many instances when they are administered in adequate dosages in crop lands, serious damage occurs to other organisms (and potentially to man himself), thus forcing re-examination of the control systems.

The influence of air temperature, humidity and wind speed and launching and transport of disease spores and similar particles are acknowledged but still very little understood so cannot yet be used in control measure planning. Weather history effects on maturation of fungus fruiting bodies and release of spores are only now being subjected to study. This knowledge of the influence of atmospheric conditions on aerobiological phenomenon is essential for successful applications of control measures for both the useful and the harmful occurrence.

Some of the most troublesome plant diseases are caused by viruses transmitted by sucking insects, such as aphids. In recent years instance of the atmospheric transport of large population of these insect vectors have been recorded over as much as several hundred miles and small numbers of insects have been collected from upper tropospheric levels over mid-Pacific. Over local areas herbivorous insects, such as locusts and sucking insects, such as aphids, leaf hoppers, and spittle bugs, are regularly moved on to new areas by wind; the larger the insect, the stronger the wind needed to transport it and the more uncommon the occurrence, in most areas. Atmospheric dispersal of these insects is appropriate in a comprehensive program of aerobiology.

Animal diseases carried by bacteria and viruses are in numerous instances dispersed through the atmosphere, to the point where some are regarded by public health scientists as virtually ubiquitous. Many kinds of bacteria from highly resistant spores that survive long periods in the atmosphere, even at stratospheric levels, and their small size and light weight cause them to settle out at a very slow rate. Viruses are probably always carried in other organic matter, but that matter could be dead and dried, recognizable only as some kind of "humus." When the virus makes contact with fluid bathing living cells of the proper host the virus begins its viable self duplication, and so becomes infectious. Thus, innocent appearing organic "dust" may be a carrier for parasitic diseases. Spores of yeasts, molds, and other fungi are small and airborne in tremendous numbers, yet very few are truly ubiquitous, germination occurring on a special site or stratus.

One of the insistant problem areas of aerobiology is that of allergic pollen and spores that cause millions of people to suffer reactions that range from mild "hay fever" to very dangerous asthma. Intensive effort is put into air sampling in order to establish coincidence between reactions of sufferers and pollen and spore loads in the air and also for the purpose of giving some warning of outdoor air conditions to those susceptible to given aero-allergens. There are fungus diseases of man, some very serious, such as coccidioidomycosis and histoplasmosis, that are definitely known to be air borne, but they have been confined to local areas and low incidence thus far. Disease microbes launched in droplets from nose and mouth become air-borne and the virus and bacterial infections of the human upper respiratory tract are among these. Survival conditions for the different forms in air are almost completely unknown. Insect vectors of human diseases such as malaria and yellow fever are also subjects for aerobiological investigations in certain warm ranges.

Aerobiology is being advanced in the U.S. perhaps as rapidly as in any country, but the total effort is far short of that required to meet the urgent needs for information. Many federal and state agencies and groups of university research workers are recording observations of air-borne microbes, spores, pollen and radioactive particles. A major effort is being applied to practical measures against various kinds of dust and gases in the atmosphere under pollution control programs, but no organization so far exists for communication between these various investigating groups, and as a communication link with the international coordinating group connected with programs in other countries. Here is a special opportunity provided by the IBP and our national program within it.

The aerobiological program of the U.S. national program of the IBP is being designed to coordinate and facilitate aerobiology investigation within the U.S. and in particular those investigations having relevance to problems shared with other nations. The mode of operation will be to invite participation by scientists with projects in progress where mutual benefits might be expected, to endeavor to inform all interested persons of the state of knowledge within the field including current work in other nations, to develop improved records and communication systems in aerobiology, and to maintain a clearing house for information on urgent problems, qualified personnel, and usable records and facilities.

C. PHENOLOGY PROGRAM, BY LAWRENCE C. BLISS, UNIVERSITY OF ILLINOIS

Man has always been alert to the natural events taking place in his immediate surroundings. In past ages, knowledge of these events was often essential to his survival and even today such knowledge is of direct use (in prediction of insect outbreaks, allocating grazing lands, or forecast of forest fire hazard). Phenological studies now being developed may serve to give modern urban man a deeper interest in and understanding of biological phenomena. Phenological studies might include observations such as: leaf flush, corolla opening, leaf fall, nest building and egg laying of birds, nuptial flights of insects, and fish spawning.

A broad scale study of phenology is planned as part of the United States contribution to the International Biological Program (IBP), an elaborate program in which biologists of many countries are participating. The phenology program will include intensive and extensive studies of plant and animal species both aquatic and terrestrial. Under the IBP, these studies can be conducted simultaneously over widely separated parts of the world, and at several levels of sophistication. Phenological aspects of pollen and spore discharge, transport, and sedimentation are central to the developing program on Atmospheric Dis-

persal of Biologically Significant Materials. This program is also being developed under the auspices of the IBP.

Description of studies

Intensive studies at selected sites throughout the country, will encompass detailed measurements of the environment and investigations of population structure, breeding behavior, and physiology of the organisms in addition to detailed phenological observations.

Knowledge of the genetic variability within a species is fundamental to the selection of appropriate species for study. The program should clarify reasons for the geographic and temporal pattern of phenological events revealed by the extensive program and species studied should be chosen with this end in mind.

Individuals may wish to study intensively the phenology of a select group of organisms in one or more sites for a period of time as a part of or separate from the intensive site studies.

The extensive program will require a large number of stations and observers organized into state or regional networks. Seasonal and annual observations will require well defined scoring systems and specific individual organisms marked (in the case of plants) for observation. Species selected should be easily identified, widely distributed, readily observable with relatively simple and definable seasonal events for recording. Two kinds of networks are envisaged: (1) A North American phenological garden network which will use a combination of genetically known clonal stock and native species. Garden sites might include arboreta, nurseries, agricultural experiment stations, etc. (2) A survey network in which a greater variety of organisms would be studied. This network might be organized by state academies, teacher associations, garden clubs, arboreta, Audubon Society, state conservation departments, and federal agencies such as the Forest Service and Park Service.

Examples of phenological studies now underway include: (1) Intensive studies of selected shrubs and herbs planted at agricultural stations with detailed meteorological records. Dr. Bryon Blair of Purdue University is operating a network of 10 such stations distributed throughout Indiana. (2) Extensive studies involving a variety of events observed on native species in sites as well as ornamental and cultivated plants. The Wisconsin Phenological Society (731 University Avenue, Madison, Wisconsin) operates a network of over 300 observers. (3) Detailed regional studies involving topographic and microclimatic variation. The studies in Neotoma Valley directed by Dr. Gareth Gilbert of Ohio State University have been gathering data spanning a period of over 10 years.

The objectives of this program include: 1) preparation of *maps* which will summarize phenological events (leaf enlargement, fruit and seed production, and nest building) for species or groups of species; 2) discovery of phenological clues to the physiologic limitations which determine the *climatic limits of species*; 3) clarification, through intensive studies, of the *biological basis of phenological timing*; 4) investigation of the hypothesis that phenological studies will aid an *understanding of evolutionary mechanisms and strategies* in the taxa involved; 5) elucidation of the *role of phenology in development of community structure and productivity in ecosystems*.

Species useful for recording phenological events

As a first step in the development of the phenology program we need to select a list of species for both the intensive and extensive studies. Lists of suggested species prepared by an *ad hoc* committee at Madison, Wisconsin, April 14-16 are attached. Species were chosen on the basis of wide geographic distribution, yet rather narrow range of genetic diversity. We would appreciate your evaluation of these species and suggestions of other species which are suitable for either intensive or extensive study.

Species suggested for intensive study

Birds:

Passer domesticus—house sparrow
 Progne subis—purple martin
 Zenaidura macroura—mourning dove
 Turdus migratorius—robin

Mammals:

Microtus spp.—meadow voles
 Peromyscus spp.—white-footed mice

Herps:

- Amblystoma tigrinum—tiger salamander
- Thamnophis spp.—garter snakes

Fish:

- Carpio carpio—carp
- Lepomis macrochirus—Bluegill

Plants:

- Asplenium pinnatifidum—spleenwort
- Oxalis pes-caprae—Bermuda buttercup
- Phragmites communis—common reed
- Populus alba—silver poplar
- P. nigra italica—Lombardy poplar
- Pteridium aquilinum—bracken
- Picea glauca—white spruce
- Pinus strobus—white pine
- Pinus taeda—loblolly pine

Species suggested for extensive study.

Birds:

- Agelaius phoeniceus—redwing blackbird
- Turdus migratorius—robin
- Zenaidura macroura—mourning dove
- Richmondia cardinalis—cardinal

Mammals: Odocoileus virginianus—whitetail deer

Insects:

- Pieris rapae—white cabbage butterfly
- Papilio glaucus—tiger swallowtail
- Papilio marcellus—zebra swallowtail
- Leptocoris trivittatus—boxelder bug
- Lygaeus kalmii or Oncopeltus fasciatus—milkweed bug

Plants:

- Acer platanoides—Norway maple
- Aesculus hippocastanum—horse chestnut
- Ailanthus altissima—tree-of-heaven
- Berberis thunbergii—Japanese barberry
- Betula alba pendula—weeping birch
- Cichorium intybus—common chicory
- Cirsium arvense—Canada thistle
- Dactylis glomerata—barnyard grass
- Paphne mezereum—Mezereum
- Elaeagnus angustifolium—Russian olive
- Forsythia suspensa—Forsythia
- Ginkgo biloba—Ginkgo
- Hedera helix—English ivy
- Hemerocallis fulva—orange day-lily
- Lonicera japonica—Japanese honeysuckle
- L. tartarica—Tartarian honeysuckle
- Melia azedarach—China-tree
- Metasequoia glyptostroboides—dawn tree
- Nerium oleander—oleander
- Nuphar variegatum—bullhead lily
- Oxalis pes-caprae—Bermuda buttercup
- Philadelphus lewisii—mock-orange
- Phragmites communis—common reed
- Plantago lanceolata—ribgrass or English plantain
- P. major—common plantain
- Populus alba—silver poplar
- P. nigra italica—Lombardy poplar
- Prunus tomentosa—Nanking Cherry
- Spiraea vanhouttei—Spiraea
- Syringa vulgaris—common lilac
- Verbascum blattaria—moth-mullein
- V. thapsus—common mullein
- Vinca minor—lesser periwinkle

Fungi:

Polyporus sulfurea-sulfur mushroom

Coprinus ornatus-shaggy-mane mushroom

Scientists who participated in the Working Conference and the preparation of this document include the following:

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QUESTIONNAIRE

Should you be interested in undertaking phenological research in connection with: (1) intensive environmental and physiological studies, (2) aiding in the organization of extensive studies, or (3) in long term observations on a variety of species in a given locality please write to: Dr. Lawrence C. Bliss, Department of Botany, University of Illinois, Urbana, Illinois 61801.

The Phenology Program will be assisted materially if you would answer some or all of the following questions.

1. Please suggest additions or deletions to the list of organisms proposed. Your comments on the lists would be most helpful.

2. Are you now engaged in phenological studies or have you been recently? If so, please describe your program briefly.

3. Would you be interested in establishing an intensive station?

4. Would you be interested in cooperating in a garden network by gathering data on specific species in one or several localities?

5. Would you be interested in organizing a state or regional network?

6. Please suggest names of those individuals or groups who you believe would consider assisting in organizing and/or operating an intensive program, an extensive program (garden network), (state or regional network).

D. HUMAN ADAPTABILITY, PREPARED BY DR. D. B. SHIMKIN

At the present time, the scope of the research program on Human Adaptability includes five major areas of effort:

1. The most extensive development is concerned with the *ecology of migrant populations* (A. Ostfeld, Director). This program is undertaking comprehensive, longitudinal, bio-social assessments of selected, poverty-stricken, rural populations in the United States (Negro, Spanish-American, Indian, Eskimo, and white), and of their close relatives who have migrated to urban, especially inner-city, environments. The work is attempting to develop basic knowledge, which will be of special relevance to medicine and education, on the characteristics of these seriously deprived elements of the U.S. people, and of the effects upon them of migration and urbanization. The program is also planned to be a vehicle for associated studies, *e.g.*, on lifeways (O. Lewis); in addition, a number of experimental studies, *e.g.*, the measurement of improvements in physical and mental development attainable by given changes in maternal and infant regimes, are contemplated. Attention is being directed to problems of sub-Arctic urbanization, the study of which will probably be sited at Fairbanks, Alaska, since the sub-Arctic environment vastly intensifies the effects of air and water pollution, housing inadequacies, and other special urban problems.

Cooperation is being initiated with emerging foreign projects on migrants, namely, the comparative study of French migrant communities in Quebec and the Antilles (J. Benoist), and perhaps sub-Arctic urbanization in such remote areas as Canada's Northwest Territories; adaptability of immigrants from many different nations and environments in Israel; and the physical development and mental health urbanizing peasants, in Yugoslavia.

2. A second research area consists of intensive studies on *human genetics and adaption among surviving primitive peoples*, especially in the humid tropics. J. V. Neel leads this effort, which also includes cooperation with the World Health Organization and the Pan American Health Organization. A component project is Harvard's anthropological and medical survey of the Solomon Islands

(A. Damon). In general, these investigations will help define those patterns of reproduction, stress, physiological and social adaptation, and resulting genetic evolution which probably characterized the entire human race prior to recent millennia.

3. A third field of concentration, now being developed by J. R. Audy, is that of *nutritional statuses, human development, and deficiency diseases*, especially among contrasting populations and cultures in Malaya, and, ultimately, other areas of Southeast Asia. A component project, which is a joint commitment of Mt. Sinai Hospital, New York, and the University of Malaya, is V. D. Herbert's study on the *incidence and etiology of nutritional anemias in Southeast Asia*.

4. Research on the *effects of altitude* ("Ecology of People in High Places") centering especially in Latin America, is being organized by P. T. Baker. While much work has been done on the altitudinal stresses encountered by mountain climbers, the new studies promise to add greatly to knowledge of mountain dwellers, particularly upon foetal wastages and stresses upon children, the ill, and the aged, within related, genetically defined populations, with comparative research being planned on selected groups at various altitudinal levels—sea level, plateau level, and high mountain levels. Aligned with this particular area of effort is that of the British Sub-Committee on Human Adaptability, particularly in studies being conducted by E. J. Clegg in Ethiopia, and M. P. Ward in Bhutan.

5. Finally, research on *native circumpolar peoples* (F. Milan, Coordinator), will study, among the Eskimo and Aleut populations, the effects of varying intensities of cold, hunger, and disease stresses; and of losses of variability through genetic draft in small isolated groups, upon a single racial unit. Much attention will be paid to careful measurements of human microenvironments and stresses met in hunting and fishing. The work is to center on the intense, internationally cooperative, investigations of communities in Alaska, Canada, Greenland, and, it is hoped, the Soviet Union. Coordination is to be established with the joint Scandinavian effort on the human ecology of nomadic Lapps isolated maritime groups, and other populations adapting to severe stresses in Fennoscandia.

The Sub-Committee on Human Adaptability also plans, in cooperation with the other relevant Sub-Committees, to initiate conferences and subsequent research undertakings in human fertility, in the physiology of adaption, in the assessment of nutritional statuses and nutritional deprivations, and in the measurement of human (especially urban) micro-environments.

Research in Human Adaptability will if successfully conducted, bring significant benefits both nationally and internationally through substantive findings; methodological and technical improvements; institutional building and personnel training; secondary services; and international cooperation.

Data developed by this research will aid in the formulation of more comprehensive and reliable standards for human physical and mental development; help indicate safety limits for tolerable stresses and exposures appropriate to varying ages, physical states, and, in part, genotypes; and provide bases for improved estimates of qualitative and quantitative needs in public health (both physical and mental), general and physical education, housing, and recreation. It will provide sources of public information on the nature and management of human fertility, and other adaptive behavioral patterns. Specifically, the proposed program will augment existing research by providing a more diversified population base (genetically, environmentally, and culturally), including data on groups presently under severe stresses generated by malnutrition, disease, cold, heat, altitude, and social repression. The program will seek to accomplish coordinated and longitudinal studies on carefully defined groups, so that the interactions of biological and social factors may be adequately and accurately observed and analyzed. And it will emphasize the observation of natural processes such as migration and hybridization.

Rigorous bio-social research on man is still new and little standardized. Consequently, in order to produce comparable and cumulative results, researches on human adaptability in the United States and internationally must accelerate the selection or development and subsequent standardization, of statistical designs, mathematical programs, observational techniques, instrumentation, and data-processing systems.

Extensive cooperation with professional bodies, specialized laboratories, and manufacturers will be necessary. These efforts to produce, more economically, increasingly reliable data and better analyses will be transferable to other programs. In particular, they should aid in improving cost-effectiveness evaluations

and related decision-making in public health, education and other activities contributing to the national well-being.

In order to conduct the program, and set up capacities for further pure and applied researches on human adaptability, the extensive development of new facilities and the continuous training of new scientists, technicians, and auxiliary workers will be imperative. This is true partly because of existing deficiencies and unequal distributions in resources for bio-social research. Likewise important is the fact that studies on man in the breadth, duration, and intimate depth envisioned can be done only by establishing strong bases at research sites, and by working in partnership with the populations being studied. The present program, therefore, undertakes both the elicitation of cooperation from established institutions and scientists, and the vigorous search for, and aid to, promising potentials in new academic and non-academic sources. It should be noted that significant capabilities for the program can be generated on less-developed campuses and in health and educational service facilities, such as the Columbia Point (Boston) and Mile-Square (Chicago) Health Centers, and the Milton Olive Memorial Program for Children (Holmes County, Mississippi).

Conversely, one of the lasting benefits of a well-executed program in Human Adaptability would be the birth of new scientific centers and the formation of new scientific staffs in the United States and abroad. Indeed, such resources would be useful in the development of general educational excellence in today's less favored areas.

While the performance of medical and social services is not a direct goal of the International Biological Program, work in Human Adaptability will necessarily generate modest levels of service. Serious and remediable conditions discovered in the conduct of research will necessitate some interventions. In fact, the very conduct of prolonged research on a population will inevitably serve to modify it. And for that research to be acceptable to the population, some sense of services received must prevail.

At present, research on Human Adaptability is limited, at the level of sophisticated, multi-disciplinary investigations, to relatively few countries. This situation exists, notwithstanding the magnitude and complexity of the human problems of the developing nations, because few of these countries as yet have the requisite facilities and trained personnel. Consequently, if research on Human Adaptability is to attain maximum scope and usefulness, a sizeable international training and facility-development program is indispensable. With a general paucity of people and facilities in bio-social research and training present today, a careful allocation of resources will be needed to expand overseas capabilities without destroying the United States program.

But this challenge must indeed be met, and at an accelerating rate.

STATEMENT OF DR. IVAN L. BENNETT, JR., DEPUTY DIRECTOR, OFFICE OF SCIENCE AND TECHNOLOGY

DR. BENNETT. Mr. Chairman, I would like to identify the statement and indicate that it is only the first few pages that represent what I myself have to say. It is my understanding that the subcommittee is now contemplating the preparation of a report on the International Biological Program, and since among the questions that have been posed are those of benefits, there is attached to this statement of mine a summary of the specific benefits from the six action programs that touches on these somewhat more specifically. This is from pages 10 to 20 of my prepared statement.

In my own opinion the best statement is on pages 11-14 of my prepared statement on the benefits that would accrue from the action program in phenology, which is the science that treats with the effect of climate and weather on recurrent physiological cycles such as budding, and so on. It is an excellent and specific statement of what could be expected from this program.

The opportunity to discuss again the entire International Biological Program with the committee I think, in a way, demonstrates the increasing interest of the subcommittee in this program and, in a sense, parallels the increasing interest on the part of the scientific community as this program has developed.

In our previous discussion on May 9, reference was made by Dr. Revelle, you will recall, to the desirability of what you referred to as a "line item" somewhere in the Federal budget that could be identified as funding for the International Biological Program, and at that time, if you will recall, Mr. Chairman, you portrayed vividly the dangers of a single "line item" in the budget for any purpose.

Also at that time there was considerable discussion of the similarity between the International Geophysical Year and the International Biological Program, and I think the fact was brought out that it was possible for agencies to enter a line item for the International Geophysical Year and to defend that line item quite successfully.

The problems of funding and of highlighting the funding of the program which are really what I would like to address myself to. Since this has been a recurrent subject of discussion before the subcommittee, I took the liberty of consulting the Bureau of the Budget on this matter, and I can report, to begin with, that there really are two aspects of this. The first has to do with the Administration budget, and the second has to do with what Congress does with the administration budget.

I would only say that in terms of how Congress would deal with an administration budget, I defer to the opinion of the subcommittee, but I would also state my own opinion, that a strongly-worded, penetrating report from this subcommittee would go far toward making it possible to defend whatever budgetary structure is suggested by the various agencies for this program.

Mr. DADDARIO. You understand that when we had that discussion at the earlier meeting, it was just a normal process of looking into every point being raised. It was not any fixed position that I was stating.

I was then and still am trying to figure out how it would be best to file this whole program. In fact, the major reason we wanted to have this meeting this morning, as you have rightfully stated, is to discuss that aspect of it.

It seemed to me at that moment to try to get line item support in each of the agencies would run into a great deal of difficulty.

At this stage of the game it is even more complicated, but as it gets to be more complex, the arguments in favor of having a worthwhile program for the IBP over this period of time becomes more and more formidable.

You state your case very well on the one hand, and you show the needs for funds, on the other. The more funding you put in, to my mind and that of the committee, the more difficult you make the problem.

Dr. BENNETT. I would like to restate what you said. It seems to me that once the administration budget is formed and has to be defended before the various committee of Congress, then the best defense will be a strong statement of the benefits to accrue from this program,

which I think certainly are being portrayed increasingly before this subcommittee.

Mr. DADDARIO. That is one of the reasons, too, Dr. Bennett, why we have felt that we ought to have more testimony, so that such a case might be made.

Dr. BENNETT. Well, if I may report further on my conference with the Bureau of the Budget, there certainly——

Mr. DADDARIO. We are always interested.

Dr. BENNETT. There certainly is no objection, in principle, to highlighting any items as being related to the IBP. On the other hand, they pointed out that if you have a single "line item" and if someone is in the mood for paring, that the single line item may be removed. Therefore, their suggestion was as follows: Rather than as a single line item identifiable as this program, it might be possible for the various agencies to budget for those aspects of the program that would finally be included in the International Biological Program, but not as line items, so that one could pull out of the budget a program consisting of a series of activities that could be identified in that fashion.

Indeed, the Bureau of the Budget went so far as to say that if this particular procedure was followed, it was possible, when the administration budget was issued, to undertake a special analysis of the agency budgets with an identification of the program and a description of it so that this would be highlighted as a program. I did not seek or obtain any guarantee from the Bureau of the Budget that new money would be made available.

Mr. DADDARIO. You are not alone. I don't think anyone has gotten such a guarantee. How do you overcome the problem already raised in this regard? If you were to follow that suggestion, how do you distinguish or prove to people that you are not taking moneys away from present programs that other people think are important? Since they are not always satisfied with funding as presently existing and if it doesn't go up, they say it is because moneys have been deferred for this particular purpose.

Dr. BENNETT. I can only say it is a problem and it is one which I pointed out to the Bureau of the Budget. The success of this program will be dependent upon new money so there should not be an "apparent" program with simply the shifting of existing funds. While I didn't obtain a guarantee from them that such money would be available, they are well aware of the fact that a successful program will involve new moneys.

Mr. DADDARIO. Then they are certainly in support of the program?

Dr. BENNETT. In general they are in support but the components of it will be dependent upon the presentations that the various agencies make to the Bureau of the Budget. They did indicate a willingness, if this program does develop, to include in the special analyses, an analysis of the funding for the IBP which will highlight this program and give it the visibility which has been of concern to the subcommittee and to the planning committee for the International Biological Program. I would simply conclude by reiterating the fact that it seems to me that the best defense of funds that might be proposed for this will rest in a description of the program and the very real benefits that will accrue not only to mankind, but to the United States from

the successful carrying out of the various projects that have been proposed.

Mr. DADDARIO. Are you gentlemen all in agreement with the figures which have been estimated insofar as the cost of the IBP over the course of the years as being somewhere in the vicinity of a low of \$50 million and a high of \$100 million? Does this fit everybody's idea?

Dr. BENNETT. The last information I had, the estimate was closer to \$135 million, which included some \$21 million for training.

Mr. DADDARIO. We have a new high now.

Dr. BENNETT. I am simply mentioning what I was told. I haven't seen the figures in Dr. Cain's testimony, but it is something in that neighborhood, I believe.

Dr. KETCHUM. In terms of new funds, these are reasonable funds. But there are ongoing programs which are already supported which will be important in the IBP.

Mr. DADDARIO. Yes, that is in Dr. Cain's testimony.

Dr. BENNETT. It is on page 11 in Dr. Cain's testimony. It has a summary.

Mr. DADDARIO. In any event, the large amounts of money that this program would cost would make it extremely difficult to work it out in such a haphazard way as the Bureau of the Budget indicates. At the moment, there is \$312,000 which the agencies have been able to scrape together. We are asking them to multiply that a hundredfold.

Dr. BENNETT. I was not suggesting that this be done in a haphazard way.

Mr. DADDARIO. No, I am not arguing, I am trying to get an idea of how difficult it might be within any of your agencies to be able to get support to release those funds for the purposes of the IBP.

Dr. BENNETT. We used the National Science Foundation as an example only. Most of these projects are multidisciplinary. They suggested that instead of a line item, that the additional budgeting be done on the basis of the usual categorization of disciplines and then it would be possible to pull out from this what the International Biological Program would be without having to risk a single item that might be struck.

Mr. DADDARIO. I understand that, but in the final analysis it boils down to a line item because as it comes up for appropriation and as these figures are swollen beyond what they have been, that amount is in fact going to be isolated and then acted on separately.

Well, let us get into some of the other aspects of this.

What is presently going on in areas similar to this? For example, how does AID, or the Peace Corps, or UNESCO or the Private Foundations Fund come into this? How can they be expected to be helpful if at all?

Dr. Cain, will you think about that?

Dr. CAIN. Well, I am obviously not in a position to speak with any authority about other agencies; however, I have had a little experience. You mentioned the Peace Corps, for example, and a few years ago I was returning from an experience in Argentina with university students including graduate students in fieldwork—it was a semiarid region in the Andes. In interacting with the local people there seemed to me a magnificent Peace Corps opportunity. I suggested the plan

that we develop a multidisciplinary team of young people with some technical competence who would work jointly.

For example, they would be paired. Let us say that there is a soil scientist from the United States working in the Peace Corps with an Argentina counterpart. You have ecologists interested in the structure and composition of vegetation and its indication for the capability of land for certain kinds of uses and the two would represent a pair.

Another would be an agronomist who had knowledge about crop and rain possibilities and so on through an array of maybe six or eight U.S. Peace Corps people and six or eight Argentine counterparts, and with proper supervision we could make extremely good use of some of our more mature Peace Corps people, let us say at the graduate student level, having had some experience.

I hate to report it but this struck no responsive chord with the Peace Corps. This was 3 or 4 years ago when we explored this.

If I may comment again—and you will have to understand this as a personal and limited experience—I believe that the AID program under its various labels through the past three decades has come extremely slow to recognize such an obvious thing that the fundamental effort must be put on agriculture and the use of primary natural resources if a country is to develop.

I am very happy to report in the last AID budget that over 50 percent of their money is for this sort of thing. But the early emphasis was on magnificent construction development without all the underpinning that is necessary to make irrigation work.

The AID program might at this moment have evolved enough so that it would be susceptible to the support of some fundamental science along these lines. My optimism isn't conditioned on anything I have seen happen. I think the possibility is there now if we talk to the AID planners. Here is a possibility of IBP. The AID programs are built out of what the foreign nationals wish to do, not that there can't be some influence on them.

But if the IBP program produces in the first year the types of thing that are useful—our special interest is defined as being Latin American, although not exclusively that—I can see developing out of Brazil or Argentina or Colombia, an interest in asking AID for support for this kind of effort. If we worked along this line, I think we will find that there is money for this sort of thing but the concept isn't there yet. I hope I am wrong but this is the way I feel about it.

Dr. KETCHUM. May I supply some information with regard to support from UNESCO which you also mentioned. I am also the convener of the International Marine Committee and UNESCO is supporting that Committee financially in three different ways.

It is supporting the salary of a part-time scientific coordinator for the International Committee, and in this regard I would like to emphasize the fact that the scientists on these various committees are contributing their time and services with the left hand while the right hand is busy with other things. The scientific coordinator of our International Committee has been a tremendous asset in being available for a specified part of his time and getting things actually done. This is supported entirely with UNESCO funds through the budget of the Intergovernmental Oceanographic Commission. We also have support

from them in terms of technical meetings, in terms of supporting the expenses of participants in technical meetings, and in terms of printing and issuing the handbooks that result from such technical meetings. The third way in which we have financial support from them is in terms of fellowships.

We have had half a dozen fellows from the underdeveloped countries go and work with progressive laboratories primarily in European countries because it costs so much more to send someone from Africa to the United States and UNESCO is a bit tight with its money. It doesn't have a great excess to spend in any case, so they are generally distributed throughout the European laboratories. We have had some support in this latter regard from the FAO. On the international scale both UNESCO and FAO are supporting actively the development of this program.

Mr. DADDARIO. What shape are we in at the present time financially to get our own part of this program working? What are our present problems, Dr. Cain?

Dr. CAIN. If I may start by commenting on Dr. Ketchum's recent statement by the international agencies, I would like to point out that these are funds on the international scale that are comparable to about a third of the million that you referred to a while ago for the U.S. National Committee. They have to do with planning and programing, organization and administration. These are not research funds and so far we have been without these research funds.

Now, the ongoing overhead for the U.S. National Committee, and let us say it has been running about a third of a million, has been raised at least in the last year by transfer from Federal agencies into one pocket. There is some resistance to this. There are even budgetary policy statements against this way of running an operation. I myself would like to see the administrative part recognized and carried without assessment on the agencies because they are going to have enough trouble raising money anyway. With respect to the financing, again I have to draw a distinction between the financing of in-house programs and the financing of extramural programs by nongovernmental scientists. Here is where the most difficult problem lies. I would like to illustrate how it is working in Interior and at the same time the limitations under which the Interior Department finds itself. I suggested to Secretary Udall and the late Mr. Larsen, who was Interior's budget director, that each of the appropriate bureaus in Interior identify what it is doing that is part of the IBP program or closely IBP related. I have the information for the planning of the fiscal 1969 budget from the Bureau of Commercial Fisheries which I think is a good illustration if I can very briefly state it to you.

Mr. DADDARIO. If you would.

Dr. CAIN. The scheme that we settled on is very much like what you have just discussed with Dr. Bennett, a feeling of the Bureau of the Budget. Instead of asking for a Department of the Interior line item for its IBP work, we decided that each agency would prepare its budget in a normal way including increases where warranted for more activity along certain lines. Then we could attach to the budget as an appendix for each bureau where it is appropriate a statement about that part of the budget which is IBP. The Department budget trans-

mittal letter would in turn point out by a paragraph or two of emphasis what Interior is doing that is IBP.

In internal planning, as an appendix to the regular budget they have identified what they have proposed that is IBP. Under a category entitled production activity of fresh-water communities they identified 2.2 million.

Under a category production activity of marine communities, they identify 11.6 million.

Under a category, use and management of biological resources, they identify 2.3 million.

And under systematics and biogeography, a quarter of a million, this totals 263.5 million in their proposed 1969 budget of the Bureau of Commercial Fisheries. It is pulled out and discussed. It is not identified as IBP in the budget.

In other words, several months ago when we first started thinking the fiscal 1969 budget, the Department of the Interior decided on its own that this was a good way to approach the problem.

The Commercial Fisheries Bureau is a good one to illustrate this operation because it is primarily a research and engineering bureau. It is not entirely, but almost entirely, related to the marine researchers program so that this 26.3 million proposed for 1969 is about 40 percent of its total budget that they have identified as IBP-related work. This is grand from my point of view. I think we have made the case and made it clearly and not necessarily confused the issue, although, as you have said, anybody looking closely will recognize that it is IBP just as truly as if it were standing as a line item. It does show that it is an integral part of what the Bureau is about anyway and that is an advantage.

Now, we come to the disadvantage. There is very little capacity in this to support anything but the Bureau's in-house program. There is some possibility for training because there is some space in the Bureau's 14 or 15 laboratories, some desk space and so on, there is some opportunity for training with respect to the research vessels of which I think there are about a dozen and a half in operation. But the capacity to assist training in the university laboratories is almost nil.

As to grants and contracts, this is possible, but all the history is that these are on very short-term, very immediate, very practical and very closely identified problems. If they want to do some fundamental research—fortunately this Bureau has been able to do it through history.

If I may take another couple of minutes I would like to tell you one story that illustrates payoff of research. The Bureau started back in 1949 in cooperation with California, what is called the California cooperative oceanic fisheries investigation. They started to study the California current. On a regular sampling basis they recovered the fish eggs and larvae, identified and got statistics on all kinds of species, not worrying about which were economic or which might be. This started as a fundamental biological ecological research program on the California current. Several things happened: An important sardine resource collapsed and a fishery disappeared. This vacuum, as it were, this biological niche got occupied by anchovies so one industry has the potentiality of substituting for another.

The thing that makes this kind of case extremely interesting was that they discovered an unexplored, actually unknown, hake fishery, and this is the basis for the fish protein concentrate plant that will be built in the Northwest. If it hadn't been for something that looked extremely remote, we wouldn't have known about the hake industry or about the anchovy potentiality as quickly to substitute for the disappearing sardine industry and we wouldn't be in a position to locate the FPC plant properly. You can't start out with a fundamental ecological program and say it is going to pay off this way, but if you have faith you can tell from history that there will be payoff.

I thought these two things together would round out some of the discussion we have had both on the problem of financing and the sort of cost accounting that seldom can be even as precise as this one is.

Mr. DADDARIO. What restriction through funding of this kind will be imposed on you who are going to have the responsibility to run the IBP? How do you feel you will work out funding and how do you concentrate the activity?

Dr. CAIN. The U.S. National Committee is a planning and coordinating body. We have made that one point clear out of which our IBP program is developing, the concept of it. We are not in the granting business as a committee. Now, ever since the initial meeting of the ad hoc committee 4½ years ago, there never has been a meeting of the national committee that hasn't been attended by representatives of the National Science Foundation, the National Institutes of Health, the Atomic Energy Commission and so on. The whole community of Federal agencies including those which have normal responsibilities have from the beginning followed every step of this plan. I believe that the mechanism exists to determine what it is that we want to do, to identify and pass judgment as to whether it fits into the restrictive program. This is one way we keep the program from sprawling everywhere but leaving the determination and the approval and funding of specific recommendations to the agencies which have been in the business.

This, then, as I understand from the point of view of nongovernmental research that is carried on from grants and contracts, this falls back where it has always been.

The bureaus and the agencies in their in-house work have their own machinery, an example of which I have given you from the Interior Department. The mechanism is adequate for keeping this controlled and operational and sound, and on the track. The same thing exists internationally, although obviously a little looser internationally. Two-thirds of the nations are interested.

Mr. DADDARIO. The committee would not be one that would be completely in charge of the program?

Dr. CAIN. No. Not in charge in any sense except to transmit to funding agencies the judgment that somebody's proposal is or is not a close fit in the goals sought by IBP. This is where the national committee functions. They don't determine who gets money. That is determined on scientific merit together with the appropriateness to IBP by the granting agencies within their own limitations and their own machinery and their own review mechanism for passing judgment.

Mr. DADDARIO. You see no problem through the committee being actually a coordinating force rather than one of direction?

Dr. CAIN. I don't.

Mr. DADDARIO. Dr. Bennett.

Dr. BENNETT. I think that the fact that the review mechanisms for the extramural projects will consist of those presently in existence is a strong point to defend this as a program of quality. The mechanisms that are presently utilized in the agencies to pass judgment on the scientific merits of grants and contracts will be used in connection with this program. The point should be made, in terms of guaranteeing that the quality of the work done under this program, its scientific quality, that it will have to pass the same judgment that presently exists in the various agencies.

Mr. DADDARIO. That takes into consideration Dr. Cain's concern about getting support for scientists outside of Government?

Dr. BENNETT. Yes.

Mr. DADDARIO. Then being able to work such support through already established criteria, already developed within these agencies for which they would have the basic responsibility.

Dr. BENNETT. The judgment on the scientific merit of the suggested projects, yes.

Dr. CAIN. I believe what we are loosely calling the granting agencies, the National Science Foundation, the National Institutes of Health and to some extent several others are in the habit of talking to one another about giving projects and sort of dividing up into pieces so that you get a joint funding. Many of these things that IBP wants to do are disciplinary but yet under human adaptability.

The Department of Health, Education, and Welfare may very well want to carry part of it. At the same time they may not want to carry ecological studies of the environment in which the people who are doing the studies may live. There is an experience in putting these things together. I am not worried about the capacity of the existing organization to function if they have the money.

Mr. DADDARIO. What do we need the committee for then, Dr. Cain?

Dr. CAIN. Well, let me say this, that this concept was not dreamed up by any Federal agency. This was dreamed up by an independent scientist.

Mr. DADDARIO. I understand that.

There is no question in my mind that the agencies can work together. This committee has had experience with them. We have seen some important improvements over the course of time but obviously what you are trying to do is to place emphasis on a very important need, both nationally and internationally. I wonder if it is sufficient and if you have the force and drive to accomplish these objectives if, in fact, all you are doing is coordinating what is a presently existing capability.

Dr. KETCHUM. I would like to comment on the need for a national committee. Scientists are unique people and the best scientists don't submit to being told what to do. But on the other hand they have a tendency to work in isolation and sometimes to see a rather narrow part of the program. Something like the National Committee on the International Biological Program forces a few scientists at least to

look at the broader problems that face us in terms of the application of the specific parts of science to practical problems in human welfare.

I think the development of the statements that have been made of the national problems have themselves been a valuable contribution on the part of the National Committee. I think that they have already oriented the thinking and broadened the thinking of some of the scientists who are anxious to participate. As my mother used to say about me specifically, I could be led but not driven and this is typical of scientists. You can sometimes tell them or intrigue them about a direction in which to go but you cannot sit there and direct them to go. So in terms of the national committee being in command or charge of this, this is not true, as Dr. Cain has said. But I think the statements that have developed and the orientation of the program that they have provided have been useful contributions.

Mr. DADDARIO. Is it in command or in charge or not? Is the function of this committee to act in a coordinating way?

Dr. KETCHUM. Yes.

Mr. DADDARIO. Dr. Cain has said you have meetings where all these agencies are present on each occasion and that they have shown a great interest. You would impose upon them through the process of discussion your thinking and you would be developing those programs which they bring back and sell to their own agencies. These would then, after being funded, come back to you.

It would appear to me that even though you are not in direct control, you do have an extremely important function, both to initiate the impetus and to generate support within the agencies and through OST so that you could get some help.

Dr. KETCHUM. "In charge of" in the sense of insuring communication in a way that has not been possible previously, between the individual scientists interested in similar things.

Mr. DADDARIO. And the other countries participating—

Dr. KETCHUM. That is right.

Mr. DADDARIO. In the IBP they would have to have somebody to look to which has been established on an international basis rather than have contact with the agencies themselves which would complicate things further. I think it is important that we spell out the need in this particular area. I can't conceive that you would be in any way superior to the agencies, but that the agencies would be working with you and after they were done, the work would be earmarked for your attention. Wouldn't you conceive it that way?

Dr. BENNETT. Yes. Dr. Cain made a point. The existing committee will pass judgment whether certain proposals will fit in with the overall program; there will be a continuing judgment before the proposals are referred to the program. This will prevent the sprawl of the program. In such a broad program it is possible to tack things on around the edges unless you monitor your objectives and see where the pieces fit together. It seems to me that now the planning stage is drawing to a close, this coordination and prevention of sprawl would be a very important component that could be only filled by a committee of this type and not by any single agency.

Mr. DADDARIO. And you would be able to identify it pretty well, it seems to me. Dr. Bennett spells out how following this formula

the programs could be supported. You seem to have no great disapproval with that, Dr. Cain. All the agencies could then be put together. In that way you would have a responsibility to see that it doesn't sprawl out. As I think about it, it would seem to me that it would be perfectly manageable in that way.

Dr. CAIN. Another need is for holding the U.S. National Committee together and in existence and we are now talking about—

Mr. DADDARIO. I am not trying to get rid of it.

Dr. CAIN. I know. Perhaps 80 people are involved through the parent committee and the nine sections officially and directly involved. It has been this mechanism that has been able to finance and to sponsor and to help organize the various meetings out of which the sectional programs have matured. It has brought the people together to interact, to think together, to rub ideas off on one another and come out with something to do which can be done and is worth doing. Part of the job isn't the doing itself. The first part goes to the concepts of what it is about. The second part goes to how are we going to do it; what methodology we are going to settle on if our data is not standardized. This applies to our own scientists of various disciplines as well as it does between nations. There is this continuing need for these conferences and symposiums as data comes in; you never do it exactly the best way at the start. You need the national committee for purposes of helping the evolution of the program as we go down the road and begin to find out the meaning of the scientific data we are gathering, maybe the gaps that were overlooked and some deficiencies in the use.

I would envision that the international committee needs to continue throughout the life of the IBP. It is not big money in terms of the whole amount, it is 1 percent for administration.

Mr. DADDARIO. You are talking about the life of the IBP. How do you see this funding grow from this point on? How long do you believe that it will be effective? How do you step out of the picture?

Dr. CAIN. I think there are two comments on this. The national committee planning for the second phase, which is the active research phase, is in terms of 5 years. We are not looking down the road any further than that.

The other point is that the IGY was called the International Geophysical Year. Some of the consequences are still going. They validated themselves. The Congress and the people recognized this was worth further support. It is my personal feeling that the IBP banner will be irrelevant after the first 5 years. We won't need this special identification.

Mr. DADDARIO. Which is an additional reason for supporting this kind of funding.

Dr. CAIN. It is a good deal of money to call seed money but in effect it is this sort of thing. If it proves its worth, it will continue to flourish.

Mr. DADDARIO. You touched earlier on the criticism there was through administering this program by bringing moneys in from the various agencies and there was some budgetary argument against doing it. What is your suggestion to meet that argument?

Dr. CAIN. You mean an identifying pool for IBP?

Mr. DADDARIO. Yes, as I understand some of your remarks—

Mr. BROWN. Is there an alternative suggestion as to a single source, where the overhead can be appropriated from NSF or someplace?

Dr. CAIN. It is offered to get the overhead money by assessment on participating agencies. Some have more difficulty than others scaring up a few thousand. It is awkward. If we recognize this program, the administrative cost is a part of what we have been doing and should be funded directly.

Mr. BROWN. Through what agency?

Dr. CAIN. Our first money came from NSF. As it got bigger, NSF put up resistance. Their main job is to support research, not administration. Although they carry in all their grants to university and overhead cost. You ought to ask the people from NSF their opinion on this. I am uneasy about a single identifying pool because of the hazard of its being cut to the bone or even left out.

Mr. DADDARIO. How much money are we talking about when we are talking about administration?

Dr. CAIN. I thought about it when I prepared this testimony and I decided to leave it out.

Mr. DADDARIO. I hate to bring it up.

Dr. CAIN. I don't mind bringing it out.

Mr. BROWN. Don't we have a figure of a million and a half?

Dr. CAIN. About a third of a million a year, for 5 years.

Mr. BROWN. Thank you.

Dr. CAIN. It is misleading to talk about a total figure for 5 years because the figures which have been put before you are estimates on a brief experience now when we haven't really got very active into the research program. Another point is, and this is very important and what I thought I would do and then didn't is obviously we can't spend a fifth in fiscal 1969 or fiscal 1970 or whenever this starts. We don't have the manpower; we are not tooled up. I believe if this program goes forward in the first appropriation year, whatever it may be, about \$15 million could be spent in new money. The second year we would have to spend a little more. I was thinking personally in terms of an escalating program in terms of funds which is a perfectly normal procedure, it seems to me, something in the order of \$15 million could be used in fiscal 1969.

Mr. DADDARIO. You don't know where it ought to come from. Have you any idea on that, Dr. Bennett? Did the Bureau of the Budget give you any help?

Dr. BENNETT. It would have to come from an agency that would be willing to ask for it. I would think NSF would be in a position to ask for it but I would say that is a matter for NSF to decide.

Mr. DADDARIO. You would agree it ought to be specifically identified for that particular purpose and then this would, in fact, be a fund that the national committee would be responsible for?

Dr. CAIN. If it were an item in the NSF budget, they would be responsible for the dispensing of it. All the committee would do as proposals are referred to it by any agencies is the judgment that this is an integral part of IBP as described but that is not management of the money.

Mr. DADDARIO. Are you saying then that the agency—and let us say it is NSF—would have the responsibility and if they have the respon-

sibility would they, in fact, be directing the program? Is this what you want them to do?

Dr. CAIN. We have no problem. Our relationships with NSF are excellent, not only in understanding one another but they have the experience in cooperating in funding with other agencies of the Government of which this committee is not one. Isn't this correct, if the IBP new money is identified in one budget, it would still be spent through several budgets because there are several granting agencies that have an important stake in the IBP program?

Mr. DADDARIO. Let us talk about this for a minute.

Dr. BENNETT. In terms of the overhead and the expenses of running the committee, if this came to \$300,000 a year and if under the hypothetical circumstances that this were money in the NSF budget, I am quite sure they would turn it over to the National Academy of Sciences in terms of the overhead. They would make a grant to the National Academy of Sciences. I am speaking only of overhead moneys.

Mr. DADDARIO. Following procedures presently in effect of other programs that have worked out properly through such filing arrangements.

Dr. BENNETT. Yes, indeed. It is quite commonly done. You would not then have the National Science Foundation doing anything but obtaining the funds and the responsibility would then in fact be the committee's.

Mr. DADDARIO. The committee would then have the responsibility of accounting for those funds. Is there some wisdom in doing it that way?

Dr. BENNETT. I would say on the basis of past experience that it is a relatively common procedure for granting agencies to turn over funds of this type to the national academy when the national academy takes over this type of responsibility which it does with great frequency.

Mr. YEAGER. It wasn't clear to me when you mentioned the \$15 million figure because the bell was ringing, but you are talking about the money for the operation and overhead of the committee?

Dr. BENNETT. That is correct.

Mr. YEAGER. It is not phase 2 money?

Dr. BENNETT. No; it would be utilized during phase 2 to support committee activities.

Mr. YEAGER. It is not research money.

Dr. BENNETT. No; if it was in the NAS budget, it would be dispensed in accordance with the procedures set up for it.

Mr. DADDARIO. Even though these funds are administrative type funds, it does put the responsibility on the committee to coordinate all of these activities and it would be a point to which we could look for explanation as to accountability and what have you?

Dr. BENNETT. Yes, indeed.

Mr. DADDARIO. I think that is an important phase of this.

Dr. CAIN. I would like to make one other point on the relationship between the national committee and the NSF or any other agency. I doubt that if the national committee said about a proposal that it did not fit the IBP, I doubt that they would fund it unless it fits something

else in which the foundation would be interested. On the other hand, our experience so far is that they do not fund all of the things that fit because they have to make comparative judgments among the many proposals they have. The final determination as to who gets money lies with the granting agency and not with the committee. It is a two-step process.

Mr. DADDARIO. I understand this. Once these are identified and you work out some arrangement, I would expect, so that we would know which of these were in fact directed toward the IBP, then if we had some question about it, we would be able to look to the committee and the committee would then explain it to us and I presume go to whatever agency necessary to supply us with the information requested.

Dr. CAIN. I think I finally now understand the question. In the National Academy of Sciences where the national committee for IBP is seated, there is a staff, there are now two gentlemen who devote their time to keep track of all of the aspects and this will be continuing so there is a center for any information about any aspect of IBP as it is developing and as it will develop.

Mr. DADDARIO. This is an important source of responsibility that your present committee would have. You are presently structured to carry it out and you would be able to develop it further to take care of responsibilities of this kind.

Dr. CAIN. I believe so; yes, sir.

Mr. DADDARIO. Mr. Brown.

Mr. BROWN. I am interested in this line of questioning, Mr. Chairman. The amount of money that we are talking about for the overhead effect of this \$300,000 year or a million and a half over 5 years is really a very small amount of money. It is the type of funds which the National Science Foundation could easily find for the purpose of administering an international program of this sort and it seems to me that we should have this recognition, because I think that is what it amounts to; it is the recognition that we are interested in this whole biological program, and I think we should have that recognition centered in the National Science Foundation and as a mechanism they could probably enter into a contract for 4 or 5 years for a million and a half dollars to administer this; that would be with the Academy or whatever is appropriate.

The other thing that caused me some confusion is the question of the total amount of money that we are talking about, the \$136 million for example on page 11 or the figure of \$100 to \$150 million, trying to determine whether this is new money, whether it is to come from the agencies such as, for example, the Bureau of Fisheries which was mentioned, which you have earmarked or identified as \$23 million which is related to the program. Is this \$23 million a part of what we are talking about or is this all new money or just how? Is it half and half or just what exactly is it that we are talking about in terms of new funds. Now, along with this, looking at the general program of NSF, for example, they have a biology program, they are making grants for training, for example, I am sure they are making grants to graduate students, probably there are some in ecology. Are we asking for \$21 million more of this or are we looking at this in some other respect, for example? I am trying to get this straightened out

in my mind. Is there anyone who can answer this definitively or is there an answer?

Dr. CAIN. I can comment and then I think that somebody else should supplement it.

These figures which are on page 11 of my statement are derived independently of any ongoing programs in Federal agencies, the \$20-some million that we identified in the Bureau of Commercial Fisheries is not included within this figure that you see on that page.

Mr. BROWN. Those are ongoing programs?

Dr. CAIN. Yes.

Mr. BROWN. How do those compare, say, for the last 3 years? Is this a normal extrapolation of what you have been spending?

Dr. CAIN. No, there is in the Bureau of Commercial Fisheries—and this is true of fiscal 1969 planning—there is an acceleration for everything pertaining to food from the sea. This is in response to the new National Committee of which Vice President Humphrey is Chairman. This is in line with governmental policy to emphasize the importance of food from the sea.

Mr. BROWN. But it does overlap to some extent what you are talking about for the biological program?

Dr. CAIN. Yes.

The other part of the question—I have no basis for suggesting how much they would be able to finance through normal budget through the Panel on Environmental Biology or otherwise. I can't say. There is money there. Our experience in proposals coming in directed to the IBP program has this kind of price tag.

I would be sure part of this could be carried today without any money, but I don't know how much.

Mr. BROWN. We have been talking about the normal increment in our need which is a healthy thing for our internal R. & D. program. This includes biology although the emphasis has been largely in the physical sciences, an increase in the order of possibly 15 percent per year.

If we are talking about this kind of an increase in R. & D. budgets, including biology, and I am using R. & D. in a rough sense here, it seems to me that NSF should have some funds which they could program with this kind of an emphasis that the international biological program has in mind here and what we are talking about is not really \$136 million in new money, but the directing of certain funds which would normally expect to channel into R. & D.—a specific type of program.

And for budgetary purposes, I am just wondering whether there is really going to be any substantial increase or not because you are talking about a substantial increase already in this one area of fisheries resulting from a demand from another source.

There are other areas which are likewise being increased because of other demands.

I have only seen this administrative fund of a million and a half dollars over 5 years. That ought to be separated out. We ought to be able to define and channel the program more, probably.

Mr. DADDARIO. Dr. Keck?

**STATEMENT OF DR. DAVID KECK, DIVISION OF THE BIOLOGICAL
AND MEDICAL SCIENCES, NATIONAL SCIENCE FOUNDATION**

Dr. KECK. I would like to answer Mr. Brown's very good question.

The history of the International Biological Program has been very interesting in the way that we have met the financial questions involved here. I think in the hearings that you have had in the last 3 months, you have had an escalation of prices on this which this morning topped what you heard last month about the cost of this program. And so it has been difficult for us in the funding agencies to visualize with very much leadtime exactly where we were going, but I remember at a meeting 5 years ago in Paris when the IBP was first launched the Chairman thought the entire IBP might come to \$25 million.

Many others, including myself, thought this was a very high figure at that time. That was for the world program. But I think as we reflected on it later we realized that figures such as we are hearing today would probably be coming to the surface because this is the kind of a program which covers such an important aspect of man's need for understanding his environment that the programs of the magnitude we are talking about should emerge from it. These have been slow in emerging. This has been to the good. There has been a great deal of discussion about what should be done.

In the United States, we have had a program evolving which has really come into focus within the last half year, so our Director of the National Science Foundation heard \$100 million mentioned for the first time the day before yesterday.

Our National Science Board has never heard this figure, so we can't speak with any confidence or assurance here as to what NSF is willing to stand on and stand for, but I can say certainly that the National Science Foundation is very interested in seeing this program flourish because of its importance to mankind and because this area of biological science must be pushed in order to take care of the needs that we have with us. We have had that brought out by every speaker, so I don't need to belabor the point.

I would like to answer Mr. Brown, also as to what is going on.

We have looked in the National Science Foundation at the kinds of projects that we are supporting that are "IBP-like" and in fiscal 1967 we supported about \$4 million of these projects. Most of those scientists will now shift emphasis and get full swing into IBP projects.

Those same people are going to need money under the IBP banner, but also the IBP program as we now see it crystalizing is definitely going to need new money beyond and above anything we can see in our National Science Foundation present or future budgets based upon the growth rate we have been experiencing. We feel in the Biological and Medical Sciences Division in the National Science Foundation that this demand, to fund IBP projects which are first rate and that should be funded through our Division, may be something of the order of \$20 to \$25 million for brand new money, using your term, Mr. Brown, that we do not have in hand now.

Somehow, we need to get this into the system if the IBP is going to flourish and move ahead at the rate it should.

Mr. BROWN. Could you comment whether your discussion includes the money that is being spent for fellowships, the training of graduate students, and so forth, in the biological sciences?

Dr. KECK. No. That will be beyond that. What that may be will depend a great deal on the requests that come to the Foundation for this.

I, personally, cannot say I see exactly what this is going to amount to. The projects supported with research funds will support a good many graduate students as all of our projects do normally.

Beyond that, we need moneys for the training of additional people. You may think that the amount of money I mentioned is inadequate when earlier you have heard \$134 million mentioned, but I think the manpower situation is going to have some limiting effects upon how far we are going to move. They are putting everybody to work who is ready.

It means that there is a finite amount of money that can be properly spent for the training of additional scientists. I would include a need, also, for having the international aspects well brought into focus. We need to let these Latin American scientists have the opportunity to obtain training which perhaps often should be in the United States; funds for this purpose are not at hand in any quantity at the moment, so far, as I am aware. We need that kind of help but I don't know what this figure might be.

Mr. BROWN. You are aware that the committee in general has been quite favorably disposed toward the type of program we are talking about. I know I am personally. We are in a period in which programs which require the identification of even small amounts of new money are going to have tough sledding and I think we would look as a possibility at the prospect for perhaps rechanneling some funds which, for example, are going into the physical sciences at the present time.

Maybe this is heresy, but I think some of these statements which have been presented this morning indicate that there is an imbalance as between the two areas and instead of looking at each new prospect as something on which we have to load new money, is there some possibility that some of the funds, some of the emphasis, can be changed as between the physical and the biological sciences? Some of these quite large amounts which are now going into a little more esoteric phases of physics and chemistry and engineering could be diverted into the biological sciences.

Dr. KECK. I think we need to have some help from the executive branch; perhaps Dr. Bennett could speak to something like this. I am not in a position to make these judgments for you as to whether we can pull back on something that is more esoteric appearing than these problems. Someone has to weigh this point that you have raised, obviously, and decide whether money spent in this direction is really money that should be spent ahead of something else which can be deferred; this cannot be deferred. This is a judgment that I am sure we all have independent opinions on and these are held with varying degrees of intensity.

The competition for funds is intensive, as we are all well aware. I am certainly not willing to say what could be held back so IBP could go forward.

Mr. BROWN. I am of the opinion that those persons in the executive branch who have been concerned with the biological and ecological programs have been deferring, for a score of years or longer, pressing for the type of funds necessary to carry on this kind of research. I had this very strongly impressed on me last week, when I spent a few days in Maine and observed the devastation of the forests of northern Maine for the purpose of producing pulpwood to supply an increasing and insatiable demand for paper without any regard to what is being done to the entire northern part of Maine and the southern part of Canada as a result of it.

This is a small example. For a hundred years this has been going on with very little thought as to its ecological effects. It is time we considered these things in terms of research.

Dr. KECK. That is true. The International Biological Program is bringing this sort of thing much more clearly into focus in everybody's minds. That is one of its attributes.

Mr. MILLER. Isn't that one of the troubles with biology?

You have never brought it into focus through the proper people. Each individual went out on his own. It was never united to do very much. It was only in the last few years that you have had an association to cover all parts of biology.

Dr. KECK. Yes. And the people who sent into the depths of the Amazon were among the most independent and couldn't come back and get everybody to do the same thing with them.

Mr. MILLER. I was told in Jerusalem that historically Bethlehem was beautiful, and still, along the dry hills, you found evidence of terraces which meant a green place. It is not much of a green place now. They also tell you—Spanish tradition tells us—you could drive from Madrid and Seville and never be out from under the shade of oak trees.

Dr. KECK. I haven't followed that route, but I am sure it is hard to imagine what is might have been when you see it today. There have been climatic changes in Palestine where there has been some drying which has worked against man.

One of the remarkable things is to see how the European countries have preserved so much when I consider how fast we in the New World destroy what we have. They have learned to live with their environment and keep it beautiful and we have, as a young nation, still had to learn that.

As you go farther south in Latin America, they are behind us and have to learn it from two steps down the ladder.

Mr. MILLER. Isn't it true in part, for instance, in Europe, Germany, and France, where they have taken excellent care of their forests and then cut it down. They let an equal amount of ground go back to forest and stay for a hundred years?

Isn't this the way they continue to produce and live in Europe?

Dr. KECK. Yes. In Norway they take baskets of dirt from the fjord and take it to the top of the hill and start it down the hill for 10 years. That is real conservation.

Mr. MILLER. In China, on the terraced hill, as men go up to work, they carry up dirt.

In my State of California, I can show you a beautiful valley that was brought under cultivation and now you go down and find a bay filled as a result of this and rocks exposed in the valley.

Mr. BROWN. I thought some were completely destroyed in 1800.

Mr. MILLER. I didn't think they completely destroyed it. They had to build levees on the Sacramento River.

Mr. BROWN. I notice the paragraph on phenology. I read about an Israeli biologist in an article in Time who solved the problem of a hybrid corn virus which destroyed the corn. He found out by planting somewhat later, this virus was brought under control.

I see we apparently had some research done here some 40 or 50 years ago with regard to Winter wheat, and I presume this biologist in Israel had some knowledge that earlier research had lain neglected with regard to this means for controlling the virus in the hybrid corn. This is a good example of the need for international dissemination for research in this area.

Dr. KECK. Yes.

Mr. DADDARIO. We have been trying to distinguish what would be the nominal interest of funding in some of these agencies and what programs would be funded separate and apart from those under the IBP.

The testimony this morning indicates that there are certain programs which are developing in all of the agencies which find themselves amenable to IBP involvement which could also be part of the normal growth.

Dr. CAIN. Yes.

Mr. DADDARIO. And we ought not to try to separate the one from the other, but it seems to me to support the case for the IBP that this is so, that there is this natural development that would take place in any event and we are now drawing attention to it by the international label of the IBP.

Dr. KECK. I would like to mention further that some of these new programs that were mentioned for the IBP such as this study of ecosystems in river valleys and so on are going to call for large sums of money in an amount such as we have not received in the individual project type of research. These are going to call for many people working together on big cooperative programs so that here will be a demand for large grants which would again require that the agency have something with which to respond.

The U.S. National Committee for the IBP has established a program which has been put together by the best scientists available for this, who have given a long time and a great deal of thought to formulating a program that is obviously going to have the scientific merit to deserve support. So, we don't have to worry about whether these major programs have real content in them worthy of support. Rather, the question is whether the agencies are going to be able to support with respect to dollars.

Mr. DADDARIO. Are all of the river valley projects domestic or are we talking about those that are international?

Dr. KECK. Two are on board that I think are domestic.

May I ask Dr. Inger?

Dr. INGER. So far, those that have been considered are domestic so far as the budget that Dr. Cain has in his testimony.

Mr. DADDARIO. These are projects that deserve support of themselves. They are very costly.

Dr. KECK. Yes. They are identified as IBP. This kind of project must be supported whether we have an IBP or not, some way or some time as soon as we can get at them and take care of them, because this kind of study is among the most necessary still to be done in biology.

Mr. DADDARIO. Dr. Cain?

Dr. CAIN. Dr. Ketchum stated, if I remember correctly, that this marine section program that they have planned for the U.S. National Committee will cost \$7 million. It would be \$35 million over 5 years, of which 40 percent is already supported. I wanted to put this non-governmental marine program, mostly inshore, but to a considerable extent related to the environment and consequently to food from the sea.

By comparing the planned 5 years from the Bureau of Commercial Fisheries in what they call the accelerated food from the sea development program, it starts at 1969 at 19 and a half million and it ends at 1973 at 57 and a half million for a 5-year total of \$182 million. This is built on a program that has been growing through the last decade of exactly the same kind but accelerated because of the present awareness of food from the sea.

So that in one agency—and there are others involved in this—so in one agency, there is the magnitude of what they feel and this is not optimum. This is their planned program in relationship to their non-governmental additional funds that the IPI-IBP Marine Section seeks over the next 5 years; it is about 15 million.

Mr. MILLER. What part do the States play?

Dr. CAIN. It varies quite a bit. Some States are without research. Others have a considerable investment. California spends a good deal of money. Washington does. Massachusetts does. But the ability to handle the large-scale programs always falls back to the Federal Government and often the State cooperates and does supplemental and complementary work.

Mr. MILLER. Thirty years ago, wasn't some of this in reverse?

Weren't the States spending more money in this field than the Federal Government?

Dr. CAIN. I don't know.

Mr. MILLER. I think, if you will check, you will find that it was just about this time that the Federal Government began to get into this field in California. The California Department of Fish and Game did a lot of pioneering before all this was organized.

Dr. CAIN. The joint program on fisheries is just two decades old.

That is when we started working on it with a large Federal income.

Mr. MILLER. The State was already working on it.

Mr. YEAGER. Do you or does the National Committee have an estimate of the minimum figure that they think will be necessary during the next fiscal year to keep the U.S. program on the track moving at a satisfactory pace?

Dr. CAIN. Not to my knowledge. I don't think we have asked ourselves that question. Mr. De Carlo from the staff, though—

Mr. YEAGER. You did not have an estimate at this time?

What was the \$15 million figure you gave?

Dr. CAIN. This was a horseback estimate figure that throughout the many activities in the IBP program—in the next fiscal year, \$15 mil-

lion could be officially spent. This is my estimate of the manpower and facility.

Mr. YEAGER. \$15 million could be profitably used?

Dr. CAIN. That is my judgment. We can analyze this and give you a developed opinion if you would like it for the record.

Mr. YEAGER. I think it would be useful if you could do that.

The second question I have that I direct to Dr. Keck and, perhaps, Dr. Bennett is this:

Does the Interagency Coordinating Committee believe that it can acquire this amount under the methods that have been proposed here today?

Dr. BENNETT. Well, the—

Dr. KECK. Thank you, Dr. Bennett. I don't believe I am really able to field that question because that is an interagency question that you are asking.

Mr. YEAGER. Do you think it would be possible to look into this and give us an estimate on that for the record?

Dr. KECK. We can try that.

Mr. YEAGER. I have several other questions which I would like to submit to be answered subsequently, if we may do that.

Mr. DADDARIO. There will be opportunity, Mr. Yeager, so that we may have additional questions submitted for the record and this committee does intend to hold at least one other hearing so that we can get more testimony before us in some of these matters.

I appreciate the attention all of you have paid to these problems and your statements are extremely helpful and the discussion here, I think, has helped to build up the case and to eliminate some of the questions that the committee has in mind.

We will be back here from time to time and if there are no further questions, gentlemen—

Mr. MILLER. Before I go, I want to apologize for not arriving here earlier, but I just returned from California.

Mr. DADDARIO. This committee stands adjourned, subject to the call of the Chair.

(Whereupon, at 12:25 p.m., Wednesday, July 12, 1967, the subcommittee adjourned, subject to the call of the Chair.)

HOUSE CONCURRENT RESOLUTION 273—CONCURRENT RESOLUTION EXPRESSING THE SUPPORT OF THE CONGRESS, AND URGING THE SUPPORT OF PERSONS AND ORGANIZATIONS, BOTH PUBLIC AND PRIVATE, FOR THE INTERNATIONAL BIOLOGICAL PROGRAM

THURSDAY, AUGUST 3, 1967

**HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND ASTRONAUTICS,
SUBCOMMITTEE ON SCIENCE, RESEARCH, AND DEVELOPMENT,
Washington, D.C.**

The Subcommittee on Science, Research, and Development met at 10 a.m., room 2325, Rayburn House Office Building, the Hon. Emilio Q. Daddario (chairman of the subcommittee) presiding.

Mr. DADDARIO. This meeting will come to order.

Dr. Galler, why don't you come forward and sit at the table?

Our first witness is Dr. David Gates, who is director of the Missouri Botanical Garden at St. Louis, Mo. We are pleased to have you here. We have been trying to accumulate some advice on this International Biological Program. We know of your interest in the subject and your relationship to the IGY. We appreciate your coming. We are anxious to hear from you.

STATEMENT OF DR. DAVID M. GATES, DIRECTOR, MISSOURI BOTANICAL GARDEN

Dr. GATES. Thank you, Mr. Chairman.

Indeed it is an honor to be here. Let me add that I am professor of botany at Washington University in St. Louis and say just a few words in terms of background here so that you will understand why I am making the remarks which I shall make in just a few minutes.

I have my degrees in physics, spent nearly 20 years in research and teaching atmospheric physics. My father, Dr. Frank C. Gates, was a distinguished plant ecologist and botanist with whom I worked very closely. I spent many years in the field collecting and doing research. I have been professor of natural history at the University of Colorado, professor of ecology at the University of Michigan Biological Stations, summers, lectured in ecology at many universities. Formerly consultant in atmospheric physics, National Bureau of Standards and Scientific Director of the London Branch of the Office of Naval Research.

I mention these things because it will bear upon some of the remarks I make later on. With the background in physics and geophysics, interest in biology, experience in the field and in research and ecology, it is this combination that perhaps gives a different viewpoint than people trained purely and strictly in biology or ecology.

The basic subject with which we are concerned this morning has to do with the natural history of the planet Earth and the complexity of ecosystems. I should define this term. An ecosystem is the sum total of the organisms and the physical environment within a given region. It may be the forest and the atmosphere around it and the soil beneath. It may be a lake, a valley or it could be a city, for that matter. Ecologists have described many ecosystems well. They have described many plant and animal communities thoroughly, but describing is far from understanding. It is not sufficient to understand a single organism; the ecologist must understand simultaneously the interaction of all organisms and the environment including the atmosphere and the soil.

In many respects this is the most difficult of all the sciences. It does not leave out a single branch of science; it involves physics and chemistry, mathematics as a tool, geology, even astronomy, meteorology, all of biology, all the aspects of physiology, morphology, anatomy, whatever you wish to mention; these all combine and integrate in the subject of ecology.

Our lack of understanding of natural habitats of ecosystems is enormous. We are literally in the stone age of ecology. Modern science is capable of greatly improved understanding, of vastly superior techniques, of methodology capable of assimilating knowledge from many branches of science and applying this knowledge to understanding plant and animal communities.

Mankind is living incredibly dangerously. We are living with and disturbing, disrupting and attempting to manage the earth's surface without understanding. We fight a war in the tropics little understanding the complexity of the tropical environment. We attempt to supply food and technology to underdeveloped nations without knowledge or understanding of their natural habitats. We dam canyons in the arid West; we divert whole watersheds and often we do not understand the impact of this on the ecology of the region. Droughts are accelerated by man and floods, and of course can be controlled also by man. Pesticides, herbicides, poison our fish and fowl, plants and people. We do not know the cycles for many of these chemicals as they go from soil to plants to animals to atmosphere, to rain and fallout back into the soil.

We change the natural, diverse, complex ecosystems, which have considerable plasticity, into highly vulnerable and simple productive ecosystems of agriculture. This is one of the great dramatic changes that has taken place clear across this country from coast to coast, taking out the grasslands of the Midwest and the forests of the North, and changing the arid desert and replacing them with the simple ecosystem of agriculture. You take the natural habitat, which has diversity of plants and animals which can take enormous shock and climatic change and put in its place a very simplified ecosystem, namely wheat or corn or oats—we must do this, of course, in many areas, but

we end up with a vulnerable system. Last summer when the heat storm hit in St. Louis and the Midwest (100° to 107° for a week) the damage was in the crops, not in the natural habitats, not in the forest, and not in the grasslands. These native plants had the ability to take this shock and to weather it.

Some plants, of course, suffered, but overall, because of the diversity, they could adjust and maintain the vegetation on the surface where the agricultural crops were badly damaged.

My next point is one of priorities in science.

We must arrange some priorities in science, or at least if not priorities let's work on some of the urgent matters for which passage of time means lost opportunity. If many ecosystems are not studied vigorously and soon, then it will be too late—they will be gone. I want to make it emphatically clear that I am not saying that we should not study the nucleus, the stars, and the galaxies—we must study those things, also. We also must and should build new nuclear accelerators. But the study of natural habitats is urgent and critical. No matter what else we do—no matter how many nuclear powerplants we build, how many trips to the Moon or to Mars or Venus we take, or how many rivers we dam and divert—we must still live with and within the natural history ecosystem of the surface of the Earth. We are in desperate need to understand this point now.

Why do we do those things first which are less urgent than others? When will we realize that soon it will be too late to study relatively undisturbed plant and animal communities? Is it more urgent to study the galaxies, the stars, the planetary systems, which will be here a thousand years hence, than it is to study the biota of the terrestrial habitats? The terrestrial ecosystems are the most susceptible of all to destruction.

The aquatic systems—the oceans, the lakes, the rivers—are next. They have a little longer time constant, particularly the oceans, the change is a little more sluggish, a little slower, but there is damage, there is change while the nucleus, the stars, the planets, the solar system, and the galaxies, they will be here indefinitely. Yet, what we are doing is to study these things with considerable emphasis, and to delay or to dabble with studying the terrestrial ecology. I do not wish to be misunderstood. I am not saying, let us not study the stars and the galaxy and the nucleus—we must. But if there has to be a choice, if there is a matter of urgency, the terrestrial system, the surface of the earth in which we are living, the thing that surrounds us on all sides, this is a matter of urgency. This is where the big changes are. This is where the impact of man is occurring.

Mr. DADDARIO. Dr. Gates, even though you are not saying that we shouldn't do some of these things, you are concerned about the priority and attach, as I understand it, some importance to the matter of urgency. Is it so important that we must do it or is it a matter of what ought to be done first?

Dr. GATES. I think there is very little disagreement among biologists, and by others as well, that there is a lack of understanding of natural habitats. I think it is absolutely urgent to study these now. Our generation has been denied the opportunity to study quite a few natural habitats of importance because they are changed and destroyed. We

will never know what they were like in detail. Others are slipping by very fast and in another generation it will be much more difficult to study many areas than it is today.

Mr. DADDARIO. This is one of your fundamental reasons I expect for supporting the International Biological Program?

Dr. GATES. Yes.

Mr. DADDARIO. The argument has been made that the opportunities presently available to us will perhaps disappear if we don't take advantage of it in quick order.

Dr. GATES. Yes.

Mr. DADDARIO. What is the danger of our not taking advantage of this at this time?

Dr. GATES. If we don't do it?

Mr. DADDARIO. Yes.

Dr. GATES. The problems will crop up on every hand, pollution, erosion, weather modification, drought, floods, the testimony is absolutely clear as to what happens when man operates within his environment without understanding what he is doing. The events are dramatic, which we have seen in the last decade. They are growing. I can't sit here and tell you—look into a crystal ball and tell you all of the potential disasters for the future except to say that to continue to live in a very expansive way without understanding is highly dangerous.

Mr. DADDARIO. Therefore, it is not just an academic matter or an opportunity through which the scientific and academic community can increase its own learning. It is a matter of great concern for civilization as a whole that we accumulate this information and apply it to this rapidly changing world within which we live?

Dr. GATES. It is of enormous concern, whether it is the estuaries on the sea coast, whether it is the arid Southwest or the grasslands of the Midwest or the tundra of the Rocky Mountains. Wherever it is, man's impact is increasing enormously and the problems come with it. Many of these problems are for lack of understanding, lack of forewarning, lack of proper management; the cost in engineering, the cost in technology to correct these problems is absolutely incredible.

Mr. DADDARIO. When you talk about lack of forewarning, is it your feeling that through a stronger International Biological Program, our involvement and study of these problems, we can develop a system of forewarning so that this can be fed into our technological planning to prevent disaster?

Dr. GATES. Yes. I would like to say a word about that in a few moments. I think the IBP is one of several programs that should be considered during the next decade. It is not the only one. It will not hold the answer to all our problems, but it is a very good start. It is an excellent way to get on with some of these things.

Shall I proceed?

Mr. DADDARIO. Yes, please.

Dr. GATES. I would like to make it clear that although there is a lot of biology being done it is not ecology. There are many thousands of biologists—if you take the full spectrum from molecular biology and biochemistry clear through to the whole organism and ecology—but this is not what I am talking about, because the anatomist or bio-

chemist or the geneticist will not understand the ecosystem. Basically he is not directly involved with understanding it. He is studying in detail, devoting all his efforts to some particular problem connected with an organism or with a cell or with a complex molecule, but he is not working with the problem of the synthesis of all these things in this ecosystem context of organisms and environment in the out of doors of which we are speaking here. This is an exceedingly important point because we could support biology very vigorously and very strongly and yet fall flat on our faces from an ecological standpoint and not deal with the external environment in the least.

The ecologist must be trained to synthesize the results of all these other disciplines. This is a very difficult task and demanding. The ecologists need to work as a team and to use all the techniques available, including mathematics and computers and the other sciences, as I mentioned a few moments ago. We need a new breed of ecologists, a new aspect to this discipline, and this is my next point. We need a branch of theoretical ecology. The same thing occurred in physics a half century ago when theoretical physics became very strong and physics advanced incredibly because of this. This put physics on a predictive basis where they could actually predict out of theory certain particles which could not be observed and they were found through experimentation. Ecology cannot do this today. It has relatively little predictive power because of lack of strong theoretical formulation, lack of good mathematical models to work with in order to carry through the complex order of things to the predictive stage.

We are being asked today the consequences of weather modification on the ecosystem or the impact of the highway or a bridge or dam on the surrounding ecosystem. We have difficulty giving answers. We can give some. We can say if you cause a deluge you are going to have a flood or plants are going to grow like crazy in some region, or if you cause rainfall here and drought over here we can say there is going to be a dying off of the plants and a desert, but it doesn't happen that way. It is much more subtle and it is these subtle changes that are slowly going on. Perhaps you have heard of the inadvertent weather modification which man is producing in terms of jet contrails in the atmosphere all over the world. Maybe out of this, we are not sure, we are getting a slow but definite cooling trend of the world's climate. What is the impact of this on our growing of crops? On our natural habitat, on the forests, and the grasslands, and everything else? We don't know; we don't know exactly what will happen. The systems are very complex.

Mr. DADDARIO. There are those that say it has no effect whatsoever.

Dr. GATES. I don't think it can be told at the present time.

Mr. DADDARIO. Your idea is that certainly something ought to be examined because we don't have the capability of developing any real forecasting about the effect of jet transportation. Jet transportation will be increasing at a remarkable rate in the years to come and it could start a chain of natural circumstances which would be dangerous to us all?

Dr. GATES. Yes. What I am speaking of here is not so much the question of the cause there, which is another subject in atmospheric physics, but the result, the consequence, of that event on the biota of your surface.

Mr. DADDARIO. I understand the way it translates itself now.

Dr. GATES. This is where we are in trouble. We simply don't know what is going to happen. What if the change was in the other direction and the climate was getting warmer, this would be more disastrous. Cooler, not so bad, things will grow and produce. Warmer. Very quickly many plant and animal communities get into disaster. They get overheated; they get too hot. That is a long story in itself.

Mr. DADDARIO. Your judgment would be that we ignore the effect of jet flight on our system?

Dr. GATES. Absolutely.

Mr. YEAGER. According to the CO₂ greenhouse theory, isn't there a possibility that there might be a warming trend?

Dr. GATES. Beginning about 1887 and during the first half of the century there was a warming trend in the mean temperature of the Northern Hemisphere. The speculation and considerable evidence through calculations and measurement showed that the increase in carbon dioxide from industrialization caused the greenhouse effect. The atmosphere is not all that neat so it is not very easy to sort out one thing and say this is it. This is part of the reason why we have to hedge a little bit on a definite answer.

But the striking thing is that this warming trend and the increase in carbon dioxide did occur, occurred over the same period of time. Since 1950 there has been an abrupt change from a warming trend to a cooling trend so that today we have a mean temperature approximating that of 1900. In other words, in a decade and a half we have gone clear back to what things were like half a century ago, and what occurred as a warming trend over nearly 50 years was reversed in about 16 or 17 years, and this is striking. Coincidental with that, 1950—

Mr. DADDARIO. Can we go back far enough, however, to come to any determination as to this being a natural cyclical trend during which there will be increasing warmth and then sharp temperature changes?

Dr. GATES. There are cyclical trends either way. This would appear to be beyond that. This is what makes it difficult to sort it out because of the fluctuations that occur and the cycles that occur. But coincidental with this cooling trend starting about 1950, this is very close to the date when jet flights became predominant. It looks like a cause and effect. We can't yet be absolutely certain.

My next point has to do with our dependence on plants and animals. Mankind is utterly and completely dependent upon plants every way he turns. Plants are the ultimate basis of all food production which refers back to photosynthesis, converting solar energy to chemical energy. Man needs plants for food, for fiber, for pharmaceuticals, for medicinals, for beauty, for color, for form, for mental therapy to escape from fellow man. We like to get away from one another once in awhile and we need plants, we need forests, we need grasslands and other places to hunt, to fish, to get away from one another and to camp.

Plants slow the winds which sweep and desiccate the earth. Plants absorb the punishing noises which man creates—the trucks on the highways and the jets overhead. If we didn't have these natural plant communities in little pockets and valleys and pieces and strips all

around us the punishment we would take would be incredibly greater. Life would be miserable here on earth.

Plants are responsible for the earth's atmosphere; they generate the oxygen for the atmosphere; they clean the air that man pollutes; they try to clean it.

The cycle of carbon dioxide, oxygen, water, these various gases, the plant takes them in through the pores, through the stomata in the plant surface and works them over chemically in the plant and then releases oxygen and water to the atmosphere. This takes in a lot of pollutants with it. The plants suffer severely.

Mr. DADDARIO. And many die?

Dr. GATES. And many die and there is much plant damage. We need the plants and the native plants, not only the flowers that are hybrids and not just the agricultural crops, we need the native trees and grasses and all these things that surround us on all sides in many ways. They cushion the good earth against the ravages of mankind. Yet, in terms of emphasis within our national scientific effort we sometimes look only at atoms, nuclei, stars, and galaxies which are of such significance to be prestige sciences. The ecosystem may appear less tangible than some of these inanimate things and because of this we are sometimes misled in our critique to realize that it is of overriding urgency to understand the ecosystem now.

We tend to take these things for granted; this is part of the trouble, the commonness of the communities around us; whereas, the galaxy is way off there; it is mysterious; it is appealing; it is esoteric. But the trees and plants right at hand we sometimes think, well, they are there, you know, we can get along without worrying about them.

My next point is concerned with national facilities for systematics and ecology. Systematics deals with the naming of plants and animals and their relationships one to another, how they are related, the evolution and the genetics. People working in this subject are called systematists. The field is systematics; plant and animal systematics. The precious few national systematics collections, collections of pressed plants, bones, bird specimens, animal skins, et cetera, are the only benchmarks of natural history which we possess. These collections are the only world reference system for the identification of plants and animals. These are our biological standards, without the specimens for which species are named, man would be adrift in a morass of organisms, millions of insects, hundreds of thousands of plants, incredible numbers of organisms without any reference basis for identification. This is terribly important. Pharmaceutical firms have been known to send out expeditions costing thousands of dollars in search of plants that didn't exist because no one had a specimen in hand to show what it looked like when a name was given. Quickly, we would lose all horizon, all reference, if we didn't have these type collections at Smithsonian, at New York Botanical Garden, at the Missouri Botanical Garden, at a few of the great universities of the country, the Field Museum in Chicago, and others. These precious few great collections are the only testimony of what this world was like before and during the time when man's impact was most severe.

In World War II, 4 million specimens in the Berlin herbarium were destroyed in World War II. This is a loss that can never in any way

be restored. At the Missouri Botanical Garden we have the only major collection of African plants in the Western Hemisphere. We derive many things from them in different ways, in the many ways that I mentioned earlier. It would be a national sin to ignore all the plants of Africa and not understand them and know about them in this country. We happen to have that single great collection of these plants in St. Louis.

The only way with which any of us can go back and ask what was Missouri like and what was California like and what was Washington, D.C., like before man's impact and during it, in the early stages 100 and 200 years ago is to look into these collections, because each of these States, every State in the Union is so disturbed, so changed by new plants brought in every day, some consciously by man, some inadvertently by man, that we could not sort out what the flora of our States were like 100 years ago or 200 years ago if it were not for these great collections. These collections, every way we turn, are the benchmark for our understanding of natural history.

The great systematic collections of the United States have been ignored and abused. These collections are the grapes of wrath of natural science. They are poverty stricken and nearly bankrupt. They have come precariously close to complete disaster.

We build elegant new facilities for many branches of science, new telescopes, oceanographic ships, nuclear accelerators, rockets, satellites, computers, a national center for atmospheric research, radio-telescopes and at the same time ignore one of the most precious and precarious of our national assets, the large systematic collections which are the only direct evidence of the earth's natural history. Must we deny all future generations of mankind the opportunity to ever understand the intricacies of the biota of this planet?

Is it important that we understand the natural history as well as the history of the earth? Are the lessons of the past any more significant than the life of the past? Can man afford to ignore and abuse the complexity of natural habitats which evolved over a period of a few million years in order to satisfy his whims of the moment for exploitation? We are the first generation to be faced critically with these decisions. Previous generations could get away with this. We no longer can. The pressures on every side are too great. Biological stations are inadequate. These major collections are very badly supported, the acquisition of land for research is nearly a desperate situation in some parts of the country. Two and three and four decades ago in many areas where we have biological stations we had free access to sand dunes, to bogs, to swamps, to estuaries, to alpine tundras—no longer is this true for many of these areas because of man's occupation, resorts, the release of millions of people into recreation to use these areas to disturb them, to use them in other ways. We don't have free access to so many of these areas we once had and for some parts of the country it is becoming a desperate situation to hold on to these natural habitats for study, for research, and for training.

But institutions do not have the money to purchase the essential areas of land needed for research and teaching.

My next point, number of ecologists: In my mind, there is no question but what there is a critical shortage of ecologists in this country

as well as throughout the world. And this includes systematists as well, ecologists and systematists. We have whole groups of animals about which we do not have a single expert in this country, there are many groups of insects (and if there would be an outbreak) we have no one in America who knows certain groups of insects in sufficient detail to advise how to cope with them.

The demand for ecologists will go up steeply as awareness and recognition develops. It will go up by university, by Government, by industry, by city planning, by highway construction, almost every way we turn—as the problem of the environment and the impact of man develops an awareness—suddenly there is going to be a demand for astute advice among agencies of many types.

Now, these things are sort of self-propagating. Once you produce ecologists and they are being fed into the system, then the awareness develops because of their rapport with the community and with the organization and the demand will increase as a result. Strong training programs are urgent. Ecologists must be trained as whole scientists. This I mentioned earlier, but I want to be very clear about one point. We also need the classical ecologist, the qualitative descriptive ecologist who has been the primary individual we have had in the past, but we also need this new breed of theoretical ecologist to go along with them.

Ecology as a discipline can be made very exciting, can be made glamorous if we wish to use that term. Personally, I know of no field that is any more appealing than is ecology. Ecology can become elegant, sophisticated, and erudite; ecology can be appealing, involve travel to interesting regions of the world in which an ecologist can work outdoors, in the laboratories, use computers and all of science; what more can one ask? It is a very, very exciting field.

MR. DADDARIO. You made it appealing without even the last pitch. I think it is exciting especially as you put it here this morning.

DR. GATES. Thank you.

Two more points and I will be through. I believe we need the establishment of a National Ecological Institute. Along with this I want to make another important point. Do not confuse ecology and environmental science. The word "environmental" is being taken up by almost anyone and everyone today and if you look at this, it would soon make one believe that everything was going great, everyone is jumping on the band wagon, but this is not ecology. Ecology is more basic, more subtle, much more specific. I only wish to make that point for the record so that it will be in front of us for any other discussion.

My last point: To deal with the natural history of the earth, the natural habitat of the earth's surface, several programs are needed in the next decade. The IBP is a tangible international program and will serve as an important vehicle for achieving some of these goals. It will only be effective if properly funded by a direct allocation from Congress. Just to recapitulate in two or three sentences what I said, we do not understand the dynamics of a forest, grassland, ocean, lake, pond, or river nor are we proceeding rapidly enough toward this understanding. The condition of our ecological and systematic facilities is a national disgrace. Our budgetary support within the National Science Foundation for biological facilities is shockingly small and

grossly inadequate. The lack of training programs for modern ecologists is or will be an international disaster. We will go down in history as an elegant technological society struck down by biological disintegration for lack of ecological understanding.

I would like, if I may, to put in the record a short article I wrote recently on conservation and understanding. It deals with this very topic. I would have had it with me today if I had come directly from my office but I came here from the field where I was doing some work with my students in the Rocky Mountains during the last 3 weeks.

That concludes my remarks.

Mr. DADDARIO. If you could provide the article for the record, we certainly would be pleased to have it.

(The article referred to may be found in appendix D of this hearing.)

Dr. GATES. Thank you.

Mr. DADDARIO. How do you see the IBP in more detail, Dr. Gates, as a vehicle to accomplish some of these objectives, taking into consideration, not only our domestic needs, but the international needs as well? I wonder if you have any feeling about where the emphasis of our international movement in this project ought to be directed? Should it be South America? Should we try to confine the activities in great part to this hemisphere so we can wring from it the greatest results? Do you have any feelings about this at all?

Dr. GATES. Indeed, because of the complexity of the topic and diversity of the topic that we should define rather specific goals. Some of this has been done. The Hawaiian project is an excellent example. Hawaii, an island community of plants and animals of what we call endemics which are characteristic, which evolved and changed there, are unique. Man coming in has brought in many things to disturb it. If we don't study the Hawaiian Islands now and quickly, it will be too late and we will never know what the islands were truly like in any detail. It is a very significant project. It is one of those earmarked within the IBP.

Another is for emphasis in Colombia, in the northern Andes. This, again, is a very important project because of the impact of man on that tropical area. The tropics are undergoing very rapid change and again it will be too late if we don't start studying them now.

As you know, it is easier for us to understand the animals of the world that have been wiped out, are extinct, but there also are many plants undergoing extinction or are near extinction. This is part of the matter of urgency, to deal with those regions which are undergoing the greatest immediate change.

I would hesitate to speak to the question of international cooperation generally. I do certainly agree that emphasis on South America is a wise one and also Central America. I am sure that there are projects in other parts of the world that we must undertake because, after all, the plant-animal populations interact over vast areas. I will be pleased to comment further on your question if you wish to elaborate any further.

Mr. DADDARIO. No, I was only interested, Dr. Gates, because you had raised the point somewhere in your remarks about the critical shortage of manpower here and abroad, how you see this in relation to the problem, as it presently exists, and the impetus which could come

from the International Biological Program if properly funded. You have made the point that the funding ought to be direct. This is a matter of great concern to the committee as to how the program can be supported as it is initiated, and how it may be funded over the next several years so it can accomplish what you think it should accomplish. Because the funding is important, what we can best do with the amount of funding available then becomes a matter for us to determine.

Dr. GATES. I think the IBP is an excellent way to get to some of these things. In laying the groundwork, I hope to point out that there are many other urgent concurrent matters that should go along with this. I don't think we should mislead ourselves with the fact that if the IBP is seriously undertaken and well funded this is going to be the salvation of all the programs I mentioned. There should be other programs that the Nation should consider looking into in the next decade.

Mr. DADDARIO. You raised a point that seems to constantly crop up. This should not be looked upon as the beginning and ending of all programs. At the same time, it certainly does seem to me that the IBP could serve the purpose of arousing interest, creating support and a better environment in which we could all work.

Dr. GATES. It certainly can. May I mention one other thing that I intended to mention in the record. We do not understand the food plants of South and Central America in any great detail, and the only major work on the cultivated food plants of the Americas is a Russian work by Vavilov in 1935 or thereabouts. This is an astonishing thing itself, that within this Nation we have never seen fit to do a complete detailed study and survey of all the food products of the Western Hemisphere. There are many food plants appearing in the markets of Central America that not a single scientist in this country can identify specifically for lack of this kind of information.

Now, we cannot depend on wheat, hybrid wheat, hybrid corn and these very few crops that we cultivate in this country to feed the population of the world. They simply will not work in many regions and it is going to be necessary, in the generations ahead to constantly cross and hybridize modern varieties with more primitive, native strains in order to gain strength in the crops used in various regions in the world. This is again an illustration of the lack of a foundation, the lack of understanding, the lack of the kind of strength that I think we need to live more carefully in the generations ahead. We are on a very precarious thread when we depend for our survival upon a few hybrid varieties of crops that we happen to have worked out for this country.

Mr. DADDARIO. Why is that so? Why is it that we have not people who on their own initiative have——

Dr. GATES. Scarcity of people.

Mr. DADDARIO. We have people who are involved and they have gone about as far as they can. There are just so many fields which need to be studied and you are pointing out not only the need for people but the need of these people to do specific things.

Dr. GATES. We have very, very few people in this country who can work in detail with—again, I will say the systematics and the science of useful plants. I want to be careful there because I am not just saying

corn and wheat and some of the agricultural crops but I am saying useful plants generally. There are less than a handful of centers in this country that can even be considered very competent in this matter.

Mr. DADDARIO. Have you, Dr. Gates, come to any conclusion about the way in which the IBP is being organized and managed? Do you have any feeling about it? Any agreements or disagreements about that?

Dr. GATES. I think, generally, it is very well organized.

I think a lot is going to depend on if confidence develops toward the program and proper support and funding to see the community responsibility, to see the responsibility of the scientists and if people will really dedicate themselves to this task. I am sure they will. I believe the national committee and subcommittees and the panels that have been formed are doing a very forthright and honest job of getting down to the evaluation of projects and formulation of projects. I have no basic disagreement with this whatsoever. I went through the IGY. In fact, I was quite active in the IGY, director of the program for the National Bureau of Standards and with that experience, I certainly can look on this with some objectivity. I do feel that basically the organization looks quite sound.

Mr. DADDARIO. I perhaps shouldn't ask you, what is your idea in the amounts of money that are required?

Dr. GATES. I thought you might and I can't really properly answer this. I am not a member of the national committee.

Mr. DADDARIO. That is the reason I hesitated but I thought perhaps because you had organized IGY, you would have an idea how these things run.

Dr. GATES. I haven't sat down to make an analysis. It will be large. It won't be trivial.

Mr. DADDARIO. It won't or should not be.

Dr. GATES. Perhaps should not be.

Money is a limiting thing. Wherever the money stops, much of the work will stop. People are limiting too. I did make a point in my discussion of training. We are very late in getting started training. We should have been doing this 3 years ago. But we must have a strong training program. We simply cannot achieve the objectives of the IBP without a very vigorous training program. This, in my opinion, is where much of the emphasis must come rather soon.

Mr. DADDARIO. What kind of training and in what numbers?

Dr. GATES. Training of young scientists for these field projects involving measurement and analysis, data handling—this occurred during the IGY. Suddenly, because of the IGY we had the urgent need for many scientists to man stations throughout the world including Antarctica. We had a crash program of training for personnel in Antarctica in that case. Every one of those persons has a good job since. The training, the growth of the number of people in geophysics began to create the demand and it has been a very interesting thing to look back on, the self-stimulation that occurred within the geophysics profession because of stimulus that the IGY gave it and the same thing I am sure will happen here. But we do need people; we are very hard pressed for people at the present time. I get across my desk every week or so letters asking where we can find any colleagues for our staff. There are vacancies all over this country and we need a training program and when we do, and even though a lot of these peo-

ple will go into project work during the IBP, they will be available for these posts in the years ahead.

Mr. DADDARIO. Because other countries are, of course, going to be involved in the International Biological Program, I do think it is important for us to recognize the efforts that they are making and it is important that we do what is necessary. Do you have any idea as to what training programs other countries have developed? Are they aggressive about it? More so than we?

Dr. GATES. I really can't tell you.

Mr. DADDARIO. Do you have any idea, Dr. Galler?

Dr. GALLER. Yes, sir, Mr. Chairman. I may point to the efforts in the United Kingdom, as a model training program; it is essentially an on-the-job training program. As the IBP projects are activated, young people become associated with these projects as field and laboratory assistants and advance their academic training. In point of fact similar kinds of training programs were activated during the IGY. Graduate students were brought in to assist and ultimately became deeply involved, committing themselves to academic careers in geophysics.

Mr. DADDARIO. Dr. Gates, I must take advantage of the fact that you are here to ask you about your recommendation of the National Ecological Institute. I understand from the few remarks you made in reference to it that you contemplate it as being something separate and apart from any agency presently existing and I wonder why.

Dr. GATES. Yes, I, having experienced other matters of this type, particularly the development, the whole complete development for the Center for Atmospheric Research, because I was in Boulder at the time and somewhat involved in it. I think something of that order should be considered. These things cannot be done within existing agencies because of their mission or objectives. Many agencies have special missions and the compromise that exists in doing basic research is often quite severe. And certainly in terms of the natural habitats of the earth, the biota of the earth and all this interaction of which I speak; of plants and animals and the environment, this is as complex, if not much more so than the atmosphere itself, and it took a National Center for Atmospheric Research to begin to produce a strong stimulus toward understanding the atmosphere better and dealing with it. I feel the same thing will be needed in ecology. I must admit we haven't thought this out in detail yet. If we had, we would be very pleased to discuss it, but it certainly should be in front of us as something to consider. And to think about rather seriously.

Mr. DADDARIO. You don't consider the National Science Foundation as a natural habitat then for your idea?

Dr. GATES. Let me answer you in this way. Is not the National Science Foundation the custodian for the National Center for Atmospheric Research? It goes through the National Science Foundation for the UCAR Corp. Perhaps this could be handled the same way.

Mr. DADDARIO. The reason I ask you is that it seems to me we do fall into the habit of coming up with recommendations which involve the establishment of new agencies. Sometime or other, that has to stop. Certainly if there does exist a mechanism through which support for a needed national project of this kind—so that it can be developed through this agency—it seems to me we ought to take advantage of it.

Dr. GATES. Yes.

Mr. DADDARIO. We would have less problems getting support that way than to just recommend that a new agency be created.

Dr. GATES. Not a new agency.

Mr. DADDARIO. The national foundation, as I have seen it structured, has within it sufficient flexibility so that support for these very ideas that you so ably put before us today could be properly generated.

Dr. GATES. Let me mention one more thing in this context. There are quite a few institutions that could handle the training of ecologists for the next few years very effectively and of systematists. The institutions exist. Some of the manpower exists. It would stretch us some. We would be very hard pressed in certain areas, but we could undertake, straightaway, if funding were available, the appeal, begin to attract young people into the field and train them at a number of our great centers in this country. I just want to make that point. Centers do exist for training in the areas with which we are concerned for the IBP. These include universities, they include some of these great systematics Institutions that I mentioned earlier. But the Ecological Institute is something for the future, over and above this.

Mr. DADDARIO. How many ecologists do we have?

Dr. GATES. If you look at the roster of the Ecological Society of America—I would have to be reminded here, I am just guessing, 3,000 members, 2,500—3,000 are members of the Ecological Society. I don't think this is an answer to your question because this includes vast numbers that are interested in ecology but are not really ecologists.

Mr. DADDARIO. They have sort of an affiliated membership?

Dr. GATES. They are direct members but—

Mr. DADDARIO. How about theoretical ecologists?

Dr. GATES. Less than a half dozen; scarcely that.

Mr. DADDARIO. When you talk about the need for theoretical ecologists, what do you envisage?

Dr. GATES. I envision quite a new thing that has not existed, and that is people trained in ecology as it now exists with strong fundamental work in biology, but in addition good training in physics, and good training in mathematics and the use of computers. By theoretical ecology, I mean those people who can take the data that is accumulated in the field from observation and bring it onto their desk and organize it and derive from it causes and effects and out of this theory. Hypothesis and theories really pull all the threads together into a coherent fabric which will give ecology a real body.

You see, the difficulty at the present time—this subject is so complex and so diverse if you look in the literature, there are vast amounts of data, of wind, of radiation, of moisture, of water, of plants and animals and all the characteristics, and that is it. It is descriptive. We need to pull this all together into coherent models that show the events, the cause and effect, the way things go, the laws that exist in ecology, whether it is population dynamics, whether it is the food chain, whether it is an interaction of the atmosphere with the plants and the animals; I happen to be involved in this quite deeply in my own work and that is why I feel so strongly. This, in fact, is the reason I gave the introduction that I did, that I was trained in physics, experienced in geophysics and came back into ecology.

This can be done. It is not being done and it has to be; without it, we are only in the descriptive stage of this science.

Mr. DADDARIO. Present technology is conducive to it.

Dr. GATES. Yes. What could be done in the science of ecology is infinitely beyond what is being done.

Mr. DADDARIO. If you had available to you a sufficient number of theoretical ecologists to what tasks would you apply them? Not that you would have enough so you could apply them everywhere. You would have to establish priorities.

Dr. GATES. All right. Let me give you an example. We are proceeding in this direction now. We have worked out, for the first time, specifically how the environment, how the atmosphere, is coupled to a plant, how the wind specifically affects the plant, the air temperature, the humidity and the radiation. From this the plant responds by having a certain temperature and by transpiring and losing water at a certain rate. From the plant temperature and the amount of light, the physiology of the plant is driven, the enzymes system, the metabolic system. This is a long, complex, story that I can't obviously describe in detail here. But how the plant responds, how it grows, when it dies, how it is limited, how far up the mountain it can go, how far north it can go, why this plant is so specialized for this part of the country and doesn't grow elsewhere and then when you put these plants together, side by side, they compete with one another for the moisture, for the sunlight, for the wind and so forth. Now, we have worked this out. We are beginning to work out precisely how these factors are coupled to the plant and how this competition takes place. It hasn't been done before. Now, out of this, we can for the first time begin to predict for a given type of plant community, the water usage. We were in the West this last 3 weeks as I said, we were looking at the sagebrush of the arid zones of the West. We know now specifically how the environment, how the climate interacts with the plant moment by moment, hour by hour, day by day, because it is changing all the time, and with this sagebrush in this particular arrangement of density of plants, for the first time we can begin to compute and predict precisely the water usage of this stand.

This becomes of vital importance whether you are talking about sagebrush or the Ponderosa pine or the oak-hickory forest or the crop because this technique that we apply to ecology is also going to be applied to agriculture and in the whole matter of watersheds and the evaluation of transpiration of water by plants, for the first time in detail we can say what will happen and if the weather changes from sunny to cloudy, from hot to cold, or anything else—we can say what will be the response of this plant community. This is the direction we are working. This is where I would apply one of the great efforts; in addition to the matter of competition, there are many questions here, but this is an indication of the—I know almost exactly the kinds of lines, the type of line that we would follow in training people and in carrying out research projects.

Mr. DADDARIO. In your remarks you interspersed with your mountains, rivers, and lakes the problems of the highways and the cities. Would you apply some of theoretical ecologists to the problems of the cities and would it work out in the same way as you have just

described about the sagebrush, enlarging it, recognizing that there are additional complications?

Dr. GATES. Yes, I feel that the city is a separate field of emphasis, essentially, but the techniques are the same.

Mr. DADDARIO. Yes, that is what I mean.

Dr. GATES. The city climate is completely different than the climate that existed there before the city was built. Of course, we should begin to understand more and more what does it mean when you start putting vast paved areas around this region in all directions, changing the whole water cycle of the region, changing the wind cycle, because the buildings are now big barriers, changing the radiation because the sunlight gets absorbed in traffic and everything gets hotter, but the same techniques can be applied.

I do think it is a separate category. Any one of us can't do too many things so I certainly wouldn't involve myself in it, but the basic techniques are the same.

Mr. DADDARIO. But as you apply the theory, the techniques being the same, you could certainly come up with better judgments than those available to us about some of the problems inherent in our increasing population.

Dr. GATES. Very much so. By the way; I should emphasize something I just sort of touched upon lightly. One of the reasons ecology can do some of the things that I insist it can do in the way of theoretical ecology is the availability of the large computer. We couldn't handle all this data and all the complexity two decades ago but now we can, so this has produced a completely new approach and potential to the field of ecology, the availability of these large computers.

Mr. DADDARIO. Do you find that your classes are increasing in size all the time, Dr. Gates?

Dr. GATES. This isn't a good question for me, because I moved 2 years ago from Colorado to St. Louis to head up the botanical garden and we had to start from scratch. I have a few graduate Ph. D. students and one postdoctoral working with me. I would like several more. They are hard to find.

Mr. DADDARIO. Sid, I am sorry I won't be able to get to you again. But you are available and because you are, we gave Dr. Gates an opportunity to speak first.

Mr. YEAGER advises me we will have a meeting next week. We will schedule a witness who is not from out of town and we will try to put you on first.

Mr. YEAGER. Some of the flora, Dr. Gates, in your part of the country may not be doing so well, but some of the fauna are; namely, the St. Louis Cardinals?

Dr. GATES. Yes; I noticed that this morning.

Mr. DADDARIO. That is a pretty parochial interest though.

Dr. GATES. I will accept that.

Mr. DADDARIO. Dr. Gates, we will have additional questions to send to you. Your advice to us is extremely exciting and important. What you have said is important especially because it shows shortcomings in our society in an area where we ought to be doing much better, just because we are the kind of country we are and because we have developed the way we have developed. We certainly need this kind of information and advice and concern.

I am sure we will be calling on you as these hearings continue for additional advice. I appreciate the fact that you were here and it has been an exciting morning and if we did have a lot of young people listening to you I am sure you would already have gotten some recruits.

Dr. GATES. Thank you very much. I appreciate being asked.
(The questions and answers requested are as follows:)

Q. 1. With reference to the erosion of land, which is an irreversible process, do you believe that the evolution of a theoretical ecology could slow this process significantly?

A. 1. The development of a strong theoretical ecology would aid significantly with most applied ecology problems, such as erosion. Actually the problems of erosion can be dealt with in an immediate practical manner by direct application of classical ecological knowledge, although a thorough theoretical analysis will strengthen the decision making and improve our judgment concerning land management.

Q. 2. We are told the world's greatest reservoir of fresh water, the Great Lakes system, is already seriously polluted in part and may become so entirely. Testimony suggests that both Lake Erie and Lake Michigan are in a serious condition and that, in the case of Lake Michigan at least, it may be impossible to repair the damage. How might the application of ecological understanding help alleviate this problem?

A. 2. There is absolutely no doubt that ecological understanding has been and will need to continue to be applied to the disasters threatening the Great Lakes. The cost to the inhabitants of the shores of the Great Lakes has been enormous. The correction of the lamprey problem some years ago in the Great Lakes was a direct consequence of careful ecological studies applied to the fish of the lakes. The Great Lakes are a clear example of the need for ecological management. Man has blundered into near disaster with regard to the pollution of the lakes. In order to correct this problem it is necessary to consult the aquatic ecologists for careful advice with regard to the chemistry of the lakes and the delicate balance of organisms. The Great Lakes Research Institute at the University of Michigan is the best source for advice on this matter. Here is an excellent example of the importance in having an institute established to deal with a major ecological situation—the Great Lakes. If this institute had not been established 10 or more years ago we would have been in a much worse position today for an advisory group concerning the pollution of the Great Lakes.

Q. 3. You have testified that the nation seriously needs a new science discipline of theoretical ecology. Could you describe the composition of such a discipline in more detail? What sub-disciplines might it include?

A. 3. Theoretical ecology is that branch of ecology which primarily uses mathematical models to describe ecosystems and to describe processes and events within ecosystems. There exists today a very large body of observational data concerning ecosystems and great amounts of information about the physiology, anatomy, genetics, etc. of individual organisms. The time is now at hand to put much of this information into a framework which will tie all the bits and pieces together into a coherent system. This can be done only with the use of large computers. By means of mathematical analysis and the computerized management of large amounts of information it is possible for the first time to give theoretical ecology a strong analytical foundation.

The establishment of theoretical ecology does not imply that all biological phenomena or events can be reduced to the "cold" analytical treatment of mathematics. It simply says that mathematics and physical theory offer a strong basis for objectively analyzing complex ecosystems. I can guarantee, that when a substantial number of first rate theoretical ecologists begin to work with the study of ecosystems that there will be great advances in our level of understanding of some of these ecosystems. It is absolutely essential that this be brought about.

From theoretical models of ecosystems, or of components of ecosystems, many direct predictions of the end results of events set in motion within an ecosystem are possible. If the vegetation of a hillside is modified from grass to trees what will be the change in water usage and runoff? Modify the climate of a crop, a grassland or a forest and how will this change its productivity? Introduce a new animal into a community and how and to what degree will the entire

food chain and animal populations change because of this introduction? Answers to many questions such as these will become available in direct proportion to the degree to which ecology is given a strong theoretical, analytical base.

People trained in theoretical ecology would receive strong mathematical training, considerable background in physics and chemistry, and in computer science. In addition they would receive thorough training in ecology and considerable amounts of biological science with less of some courses from the more traditional biology curriculum. Rather than spending much time in the field the theoretical ecologist would spend his time working out models and fitting observational data (much of it already in the literature) into a coherent framework according to models which he formulates. Precisely the same relationship exists between the theoretical physicist and the experimental physicists, the theoretical and laboratory chemists, and the theoretical and observational astronomers as I suggest is needed for the ecologists. Weather forecasting is now being placed on a substantial numerical basis and much of atmospheric science is being given a strong framework in the theoretical physics of fluid motion, atomic collisions, thermodynamics, etc.

Q. 4. With regard to the systematic collections which you have indicated are too few and too poorly supported, what magnitude of assistance—either Federal or private—is needed in your judgment to rectify this problem?

A. 4. The situation relative to the major systematic collections of this nation is as follows: I did not imply there were too few of these systematics collections; however, I did say that they were very poorly supported. Most of these large collections (Smithsonian, New York Botanical Garden, Missouri Botanical Garden, Field Museum, The Gray Herbarium at Harvard, Philadelphia Academy of Sciences, Field Museum, Bishop Museum, and many others) were started and supported entirely by private funds. Today every single major collection is operating on a marginal or submarginal budget and nearly every collection is poorly or desperately housed. Some collections have been lost to fire because of inadequate housing. For many of these institutions it is a desperate task to hold body and soul together until some sort of direct aid becomes available. Each of these large collections is a critical national asset for reasons I spelled out in my testimony.

I give here some very off-the-cuff estimates of the magnitude of financial aid these institutions require in the immediate future.

1. For new buildings—\$50 million.
2. For annual operating costs to defray only a fraction of the maintenance of the collections—\$3 million per year.
3. For additional expenditures to collect and record the unknown flora and fauna of the world before it is too late—\$500,000 to \$750,000 a year.

These great, unique collections are every bit as tangible and as much in demand as any telescope, oceanographic ship, radio telescope, or nuclear accelerator. Mr. Chairman, please compare these costs with the cost of building and operating a single large nuclear accelerator. The costs listed above are far all of the great systematic collections in America.

Q. 5. If a National Ecological Institute were to be established as another of NSF's national centers, what initial cost do you believe would be necessary to get the institute functioning? In your judgment what locale or locales might best be the site of such an institute?

A. 5. The cost for establishing a National Ecological Institute would be comparable to the cost involved in establishing the National Center for Atmospheric Research. A National Ecology Institute probably would cost substantially less than NCAR in absolute amount. If NCAR cost \$40 M then NEI might cost \$25 M. The reason for this is that the NEI would not involve some of the enormously expensive physical facilities that NCAR required—such as a National Balloon Launching Facility and a Flight Aircraft Facility for atmospheric research. Clearly I cannot give you a good estimate of cost—except to compare with the NCAR experience.

The cost of a National Ecological Institute. Initial cost outlay for buildings, equipment, and staff—\$20 million.

Annual Operating Costs—\$2.5 million.

First few years probably considerably less than this because of the necessity to build staff. The costs for establishing the National Center for Atmospheric Research might be used as a guide here.

Location of a National Ecological Institute. It is important to locate it near a major university. Preferably located centrally in America. Certain advantages to locating it in Boulder, Colorado, adjacent to the National Center for Atmospheric Research. Many facilities could be shared. Many staff members could serve both institutes. There would be considerable mutual interest in problems of the atmosphere—the biosphere and the interface between the two.

Q. 6. With regard to aiding underdeveloped countries with the problem of feeding their populations, is it your belief that present American technology exported to such countries would solve the problem? Would you explain?

A. 6. Will American technology exported to underdeveloped countries solve the problem of feeding their populations? This is a loaded question to the extent that the answer to it is difficult and involved. It would be very hard to give a categorical answer and easy to be misunderstood. I do not believe that any one of us can answer this question. We simply do not know that we can solve the population and nourishment problems of an underdeveloped country. We may solve or alleviate a current crisis or problem and create a greater problem for the future. However, ecological understanding of the area in which an underdeveloped people are living will be a far better and more cogent basis for dealing with the problem than will an effort to rush in with aid based purely on our experience within our own country. I did suggest that it might be very valuable to understand and know the useful and edible native plants (and animals) of a region in order to develop hybrid crops and herds based on these. Certainly we do not have a good understanding of and complete collections and documentation of the food plants (and other useful plants) of vast areas of the world.

Ecological understanding will directly improve our ability to feed the population of the world. There are numerous examples of man attempting to farm the landscape using agriculture and animal husbandry and receiving a poor return, but when the same landscape is used for native animals and native vegetation, its productivity increases enormously. Often the real problem is not a matter of how to make agriculture work in all regions of the world, but rather to take much of the native flora and fauna and learn how to use these for effective food sources. To retain much land area for lakes and streams filled with fish for food would be an example. Or learn to hybridize deer or antelope (domesticate them) so that they will graze in the natural vegetation and be useful food products. The problem is to produce new hybrids of plants and animals which will be useful for food and at the same time will be able to live and produce in something more closely approximating the natural ecosystem for the region. Many areas of the world are not suitable for cultivation in the conventional sense. When man learns to maintain natural habitats and ecosystems and then learns how to use the produce from them he will be much better off than plowing them up and planting them with a few hybrid crops. This is why I stated in my earlier testimony that we must understand the native food plants of all regions of the world. These native food plants are a part of the natural ecology of these regions and may be a major food source when properly utilized in the future.

Ecologists can advise in an extremely useful way in many regions of the world where exporting agriculture from the U.S. to these areas may be much less productive. We need to train ecologists for these tasks and carry on ecological research in these little understood underdeveloped regions of the world.

Q. 7. Are there any other international programs aside from the IBP which ought to be pursued in efforts to solve the current ecological crisis?

A. 7. This is a difficult question to answer without considerable deliberation. Certainly international cooperation with the study of the oceans of the world is essential.

If we strengthen our national effort in ecological science to the degree necessary, the international programs will follow. As a nation we should demonstrate to the world that we not only have an ecological conscience but that we recognize the field of ecology to be of critical necessity to mankind. This nation will benefit economically and in every other way from a strong scientific program in ecology.

International cooperation in plant and animal systematics is essential. We need greatly increased efforts to collect and identify the flora and fauna of tropical regions. International programs in Central and South America in systematics will be of great value to this nation and represent a wonderful basis for international cooperation. It is also essential that African flora and fauna be studied before it is too late. The tropical regions are undergoing very rapid change

and destruction under the impact of man. Great international programs in systematics fit hand-in-glove with international programs in ecology. The IBP will be a great beginning, but other programs will need to follow it.

Q. 8. What main lessons have been learned from the IGY which conceivably could be applied to the IBP?

A. 8. Lessons learned from IGY to apply to IBP.

1. Direct funding from Congress to NSF in support of IBP programs specifically.
2. Strong training programs were established early in the IGY. The same should be done for the IBP. We need to train people for specific projects. This takes 2 or 3 years at a minimum.

3. Establishment of primary Data Centers. Arrangements must be made to handle the masses of data coming out of the program.

4. Special publications set up to handle the manuscripts and monographs produced as the end product of the IGY (IBP) research.

Q. 9. What U.S. institutions might best handle the training of new ecologists?

A. 9. Many institutions could aid with this task.

1. The strong universities which have a demonstrated interest in ecology—Michigan, Duke, Rutgers, Minnesota, Wisconsin, Utah State, etc.

2. Private institutions which have direct affiliation or strong ties to a university in close proximity, such as the Missouri Botanical Garden, New York Botanical Garden, Smithsonian Institution, Philadelphia Academy of Sciences, etc. It is important that some of the training of ecologists takes place where there are large systematics collections and systematists.

3. At a National Ecological Institute.

Q. 10. You suggest that the IBP will be effective only if supported by Congress. Do you envision any particular method or methods by which this might be done?

A. 10. It would be desirable to have the funding of the IBP as a direct allocation from Congress through the National Science Foundation to the scientists themselves. The IBP projects must not be forced to compete with other research projects for funds—as the situation exists currently. This brings a definite cross current of conflict within the scientific community. The procedure used to fund the IGY could be used for the IBP.

Mr. DADDARIO. This committee will adjourn pursuant to the call of the Chair.

(Whereupon, at 11:25 a.m., the subcommittee adjourned, subject to the call of the Chair.)

HOUSE CONCURRENT RESOLUTION 273—CONCURRENT RESOLUTION EXPRESSING THE SUPPORT OF THE CONGRESS, AND URGING THE SUPPORT OF PERSONS AND ORGANIZATIONS, BOTH PUBLIC AND PRIVATE, FOR THE INTERNATIONAL BIOLOGICAL PROGRAM

WEDNESDAY, AUGUST 9, 1967

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND ASTRONAUTICS,
SUBCOMMITTEE ON SCIENCE, RESEARCH, AND DEVELOPMENT,
Washington, D.C.

The Subcommittee on Science, Research, and Development met at 10 a.m., in room 2325, Rayburn House Office Building, Hon. Emilio Q. Daddario (chairman of the subcommittee) presiding.

Mr. DADDARIO. This meeting will come to order.

Dr. Galler, come forward, please.

Our first witness this morning is Dr. Sidney R. Galler, Assistant Secretary for Science of the Smithsonian Institution.

We are happy to have you here, Dr. Galler. I think we ought to just go right ahead.

STATEMENT OF DR. SIDNEY R. GALLER, ASSISTANT SECRETARY, SMITHSONIAN INSTITUTION

Dr. GALLER. Thank you, Mr. Chairman. I have a brief statement that I would like to read with your permission.

Mr. Chairman, I am honored at being invited to come before this subcommittee and present my personal views regarding the International Biological Program, particularly, the U.S. national contributions to this program. Before proceeding, I would like to make it clear the views and opinions that I am about to present are my own and do not necessarily reflect the views or policies of my employer, the Smithsonian Institution, any agency of the U.S. Government or the U.S. National Committee of the IBP.

Mr. DADDARIO. That is quite a complete disclaimer, sir.

Dr. GALLER. Perhaps the principal justification for my appearance before your committee today is the fact that I, along with several of my colleagues, began to campaign for an international ecological research program as far back as 1958. At that time and almost as a direct consequence of the International Geophysical Year, a small group of biologists led by Dr. O. E. Reynolds and Dr. Roger D. Reid

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from the Office of Naval Research recognized the need for developing an international ecological research program that would capitalize on the knowledge gained from the IGY and facilitate studies of the natural history of the earth's surface. It was hoped that an international ecological research program would yield a body of fundamental knowledge that could be used in identifying, characterizing, and possibly solving some of the pressing problems confronting mankind, problems that are fundamentally biological in origin. This need as expressed by several of us in 1958 was communicated to the Executive Director of the AIBS—that is the American Institute of Biological Sciences—and although I am not familiar with the sequence of activities that followed or the relationship, if any between our efforts and subsequent events, I must say that I was most gratified when the International Biological Program came into existence and particularly when the National Academy of Sciences formed the U.S. National Committee of the IBP.

I believe, Mr. Chairman, and gentlemen of this committee that the International Biological Program, including the efforts of the U.S. National Committee, represents one of the last great opportunities to design and carry out an ecologically oriented research program that could provide mankind with the basic information about the planet earth, particularly the biota of its surface, so essential for the solution of problems dealing with the interaction of man with his environment. The fact that advances in modern technology, especially in the highly industrialized countries of our earth, enables man to change his environment in a relatively short period of time speaks to the urgent need to be able to predict the consequences of these changes. The IBP could produce both the trained personnel and the knowledge required to predict and assess those consequences.

However, I must say in all honesty that I have some serious misgivings with regard to the direction and support of our national effort in the IBP. To begin with, I think that it would be wise to keep in mind at all times that ours is a national program, one that is not necessarily synonymous or even compatible with existing Federal programs. As a national program designed, developed, and maintained by our scientists the IBP must receive both intellectual and financial support from all sectors of our community, not just the U.S. Government.

In my opinion, there is a potential danger involved in considering the program to be one exclusively supported and operated by Federal agencies. This is not to discount the great importance of obtaining adequate Federal financial support for our national program but rather to stress the need to keep the program free and unfettered by agency missions.

The U.S. National Committee must retain full responsibility for the scientific destiny of the program. The present status of our national program suggests to me that the existing arrangement for financing the program is a patchwork arrangement that does not permit the U.S. National Committee to exercise its full initiative. This could lead to the subordination of the objectives of the program to the short-term, although quite important, mission-oriented objectives of our Federal R. & D. agencies. I submit that as a biologist with some experience in the management of fundamental research programs,

the signal success of the International Geophysical Year is, and I stress the word "is" rather than "was", due to the flexible administrative and financial arrangements that were made to insure both adequate financial support and full freedom to conduct the research program. Because of these arrangements, the benefits of the IGY will continue to accrue for decades to come.

It is my carefully considered opinion that the U.S. International Biological Program will flourish only if an adequate fund is established for its support, a fund that is basically independent of the annual appropriations for the support of the mission-oriented research programs of the R. & D. agencies.

Also, I should like to repeat a statement that has been made by many of the distinguished witnesses that have appeared before your committee. There exists a very serious shortage of trained scientists to carry out the research projects for the IBP. I refer particularly to the need for experienced ecologists and systematists. Both are in very short supply. The IBP offers the best hope for training young, competent, highly motivated, scientists who will be in a position to carry on the research projects initiated under the IBP. Unless we all recognize the urgent need for developing effective training programs, I fear that the great hopes and expectations for the IBP will not be fully realized.

A closely related problem is the need for more adequate support for the IBP staff, the so-called housekeeping support. Looking back again to the IGY as a model program, it is worth noting that a number of distinguished scientists and administrators deferred their programs of personal concern in order to participate on a full-time basis in the staff support for the IGY. As a member of the Environmental Physiology Subcommittee of our national committee, I have had the privilege of observing the dedicated and thoroughly overworked staff of the U.S. National Committee in action. I submit that unless we recognize immediately the need to augment that staff, it may not be possible to develop the full potential of the IBP.

Another personal opinion that may be worth noting concerns the need to develop an effective system for gathering, collating, storing, retrieving, interpreting, and disseminating the great quantities of data and specimens that will begin to flow as the IBP enters an active research phase. The problem of information exchange was recognized early in the development of the IGY and resulted in the establishment of world data centers that were designed to handle scientific information obtained from special programs of the IGY.

For example, the IGP established an Oceanographic Data Center, a Meteorological Data Center, an Aurora and Airglow Center, and other centers for receiving and disseminating data. While I do not advocate the establishment of exactly the same kind of system for the IBP, I do feel strongly that unless we recognize and solve the problems of data exchange and specimen processing in the very near future we will find ourselves in a position of gathering great quantities of data but not fully utilizing them.

In conclusion, Mr. Chairman and distinguished members of the Subcommittee on Science, Research, and Development, I look upon the IBP as an international biological problem-solving activity of the

greatest importance in guiding man's manipulation of his terrestrial environment. In this connection, even though it is designated as an International Biological Program, we should not consider the program as one reserved exclusively for biologists. Quite the opposite, the program should encourage scientists from many other disciplines to join with ecologists, systematists, and other biological specialists in searching out the knowledge about the natural history of our planet earth that we as individuals and members of the community of man must have if we are to decelerate and eventually counteract our rush toward self-destruction in the name of progress.

Mr. Chairman, that is the conclusion of my statement.

Mr. DADDARIO. Dr. Galler, you make quite a point of separating the program from the mission-oriented agencies and stress further that the financing ought to be entirely independent of those agencies. I do see some merit to that. That point has been raised from time to time. Yet, I wonder if we aren't putting ourselves in a position here where we would not be using the competence that exists within those mission-oriented agencies. To separate the funding from them completely would not or may perhaps serve a negative purpose.

Dr. GALLER. I quite agree with you, Mr. Chairman. That really was not my intention or the objective of my statement. If we hark back to the International Geophysical Year, there were two very outstanding characteristics that I found most attractive.

First, there was a nucleus of financial support made available to the U.S. National Committee of the IGY via the National Science Foundation. This nucleus of support is what I would call the base-flexible support, that allowed the U.S. National Committee of the IGY to initiate on its own volition a number of very important basic projects that become more or less the "launching pad" for our U.S. National IGY effort. In addition to that core, many other agencies, mission-oriented agencies, joined in the support of the IGY and joined not only financially but also with scientific manpower and facility resources available to these agencies.

I wish to emphasize that I am not advocating that we separate the IBP from the resources available from the mission-oriented agencies, either in terms of manpower or financial support. What I am suggesting is that the U.S. National Committee of the IBP should have a sum of money made directly available to it, for its use, possibly through the National Science Foundation, that would permit it to initiate certain projects without having to go in every instance, hat in hand, to a Federal agency and ask for support of a high-priority research project.

Mr. DADDARIO. You are suggesting, then, a structure which as I see it would allow a certain amount of independent activity through funding of this kind and then interrelated agency activity which would correlate itself within this structure.

So that there would be additional financing and to which, as has been suggested here, the IBP would find itself responsible?

Dr. GALLER. Yes, sir.

Mr. DADDARIO. Now, if we could carry that out further, how does this break down on a proportionate basis? We have had recommendations here that there be some \$50 to \$100 million expended. And I

know that it would be difficult, taking into consideration the types of projects which are involved suggested and the many more which will be coming in, to come up with some kind of a figure. But what are you contemplating here as a mixture, that is which way the great amount of funding be spent?

Dr. GALLER. Mr. Chairman, I don't mean to beg the question in terms of a proportion; I would like instead to offer a suggestion.

First of all, after listening both to the witnesses before this subcommittee and some of the discussions at the U.S. IBP National Committee and the subcommittee levels, I have been impressed with the kind of Indian rubber fiscal yardstick that we have been using, one that stretches from \$10, \$20 million, to \$100 million plus.

I feel that the U.S. National Committee and its subcommittees must develop a "hard" priority list of projects. I hasten to add, there does exist a list of scientifically meritorious projects that have been culled from the many that have been proposed. These projects that are now being advocated for support are all highly interesting, scientifically.

However, although I am not a fiscal expert, I do appreciate the tight money situation that we find ourselves in and I recognize from a practical point of view that we will have to be highly selective in implementing the many meritorious proposals that have come before the U.S. National Committee.

What I am suggesting is that before we accept any of the fiscal estimates that have been presented to this subcommittee, we should seek concrete evidence that there has been a hard, searching assessment of the proposals that have been put forth, to insure that only those proposals that are of overriding scientific importance have been selected and serve as the basis for the fiscal estimate for the U.S. IBP.

Secondly, as far as the R. & D. agencies are concerned, it has already been pointed out that they are supporting a number of IBP-related projects that are related to the agencies' missions. These projects will be supported whether or not they receive the blessings of the IBP because they are primarily mission oriented.

Many of these projects are indeed closely associated with IBP objectives and will augment the core program of the IBP but they are independent of the IBP. I would not be prepared, Mr. Chairman, to suggest a percentage breakdown of support directly for the IBP and support through the various agencies, unless the agencies first commit themselves to direct support, financially, of the IBP.

Through the IGY, if we were to look back to the IGY I think we would discover that a very substantial part of the support came from the R. & D. agencies.

Mr. DADDARIO. Well, then, if we take what you have said, you have no serious opposition to the mission-oriented agencies finding themselves involved in this program?

Dr. GALLER. Absolutely not, sir.

Mr. DADDARIO. Would you look with horror on our getting this program off the ground by the participation of these agencies, even on what appears to be a haphazard basis? We might not be able to originally make a determination as to the amount of funding, but might use it as the means through which we could give impetus to further funding in the future. I am not talking about far off in the future, but when

things do change, then separate funding could be directed for IBP purposes.

Dr. GALLER. I would subscribe completely to that, Mr. Chairman, with one caveat.

I have no objection to launching our IBP financially through agency subscriptions, although I recognize that at best this would be an interim, initial approach.

However, if this seems to be the practical way of supporting the IBP, it would be my own hope that the agency subscription would be in the form of dollar support that would be transferred or in some way assigned to a single agency, possibly the National Science Foundation, which would serve as the "banker" for the U.S. National Committee providing support to the USNC-IBP through a grant or a contract with the National Academy of Sciences.

I suggest this as an improvement over what appears to be the present mode of financing, which is to provide direct financial support only for the so-called housekeeping expenses of the U. S. National Committee. However, the research projects have been independently supported by the several agencies. The most that the U.S. National Committee or its subcommittees have been allowed to do is to review the proposals submitted to the several agencies to assess proposal relevance to the U.S. National Committee's program; however the financial decisions are made exclusively by the agencies. I think there is a danger in following this system, because it means that IBP proposals received by the agencies compete with proposals received from many other sources.

They are reviewed not only in terms of scientific adequacy but also in terms of relevance to the agency's interest and missions. Thus it, could easily occur that a proposal considered to be important for the IBP would go unsupported under the present arrangement by virtue of the fact that there would be more existing mission oriented proposals for the several agencies to select for support.

So I would hope, agreeing with the point that you have made, Mr. Chairman, that the agencies would participate financially, but would participate by providing grant support to the U.S. National Committee to use in ways it considers appropriate.

Mr. DADDARIO. There are some practical obstacles in arranging for such freedom of funding. As we have discussed this from time to time with witnesses, it seems that we substantially run into the agency problem of needing to be accountable and to arrange their own method of accounting for their own support for specific kinds of projects. Until you reach that time when you can arrange for funding which is free of agency control, you will perhaps continue to have some problems in supporting all those things which the National Committee would like to support.

Dr. GALLER. This has always been a problem, Mr. Chairman. However, I can recall on many occasions when I was associated with the Office of Naval Research we would arrange for the transfer of funds to another agency that served as the "lead agency" for the support of an important program. These problems are not really insurmountable. Of course, everything depends on men of good will. There are administrative problems, what I would call the paper pushing prob-

lems, connected with transferring funds from one agency to another. But basically, I, as a former program manager in ONR was quite satisfied with the transfer arrangements, and the leadership of the National Science Foundation in husbanding the programs, for which we had transferred our funds to it. Actually, NSF would provide a grant or a contract with a university or a group of universities to implement a major program.

Mr. DADDARIO. If you were to carry this suggestion through and have the National Science Foundation serve this function, how would the National Science Foundation carry out its responsibility both to survey, to control, and to make determinations about how those funds were to be spent?

Dr. GALLER. One way, Mr. Chairman, would be for the U.S. National Committee to prepare a proposal for submission to the NSF, the proposal to include a tight budget, and by a tight budget I mean a realistic budget. Then presumably the National Science Foundation would provide either a grant or a contract to the National Academy of Sciences for the support of the U.S. National Committee's program.

Now, I must hasten to add that I am not advocating this course because I have not consulted with any of my colleagues at the Academy and I have no way of knowing—

Mr. DADDARIO. Your opening paragraph frees you from any problems in this area.

Dr. GALLER. Thank you. Certainly we have entered into these kinds of relationships many, many times in the past, in which the National Academy of Sciences receives a grant or a contract. Certainly it is one of the most responsible scientific and financial organizations in the country, and indeed in the world. I would have no hesitation whatever in having full trust in the National Academy of Sciences ability to maintain the necessary review and evaluation of the National Committee financial undertakings.

Mr. DADDARIO. You indicate by that, then, that the National Science Foundation should serve a holding function so far as the funding is concerned, and that the administration, control, and support of the programs ought to be performed through the National Academy's committee.

Dr. GALLER. Yes, sir; I feel very strongly in favor of this.

Mr. DADDARIO. Your concern about the staff and your feeling that it needs to be augmented ought to be expanded. Could you tell us just what your recommendations are?

Dr. GALLER. Yes, sir.

I feel, and again harking back to the international geophysical program, there are many functions, staff functions that the Committee cannot assume. Essentially the Committee consists of scientists who take time out from their many other activities and dedicate a certain fraction of their time to the IBP, but they are not full time. The continuity must be maintained by an adequate staff who can serve in the role of research administrators, to insure that the broad policies as laid down by the U.S. National Committee are implemented. The staff must make arrangements for all of the logistic support necessary for conducting field programs. The staff can insure that there are developed adequate information exchanges. The staff can do the

many routine things that need to be done if our pious hopes are to be translated into active research programs.

Mr. DADDARIO. And how many people are you talking about?

Dr. GALLER. I don't know, sir. I really do not. I do know that the present staff, in my personal opinion, is overworked, I won't say they are underpaid, I don't know, but certainly I don't believe—

Mr. DADDARIO. They usually go together.

Dr. GALLER. There are not enough members on the staff, in my personal opinion.

Mr. DADDARIO. But the IGY does give you a reference point to go back to as to how you would be able to get staffing. It attracted men who were willing to get away from their own personal activities for a certain period of time and to bring their experience to bear on these programs.

Dr. GALLER. I would say 25 persons would be a fair estimate.

Mr. DADDARIO. Of this type?

Dr. GALLER. Yes, sir.

Mr. DADDARIO. Then you stress that this program is a national program, even though we are talking about an International Biological Program. How do you relate the emphasis that you give to it being a national program to the hopes and ambitions of an International Biological Program?

Dr. GALLER. I intended in stressing the word "national" not to suggest that it was not international in scope and in value. The International Biological Program is in point of fact a federation of national programs from the participating countries.

By "national" I intended to point out that the importance of our national effort in the IBP is sufficiently great to involve all appropriate sectors of our community, and that we should make every effort to seek material support as well as intellectual support from private foundations, from universities, as well as from the Federal Government.

I recognize, again, from a practical point of view that the bulk of the support would probably have to come from the Government. But I do feel that this is not the only source of support that we should look toward, for the conduct of our IBP.

The other point that I was trying to make in stressing the word "national" is that the objectives of the program are broader than the objectives of any single agency, whether it be public or private. While the IBP includes many of these objectives, they are not ranked in the same priority order as that given by an agency. The total value, nationally, of the IBP program is greater than that of any single programs of any individual organization. This I believe makes it a national program.

Mr. DADDARIO. Is the National Committee presently put together in such a way so that it could attract to it foundation private funds of one type or another, or would this also have to go through the National Science Foundation?

Dr. GALLER. I would think, Mr. Chairman, that the National Academy of Sciences is in a position legally to accept support proffered by private organizations. It would not have to go through the National Science Foundation, in my opinion.

Mr. DADDARIO. The mixture of those funds ought to come from that direction, rather than through a Government agency?

Dr. GALLER. Yes, sir.

Mr. DADDARIO. Does your feeling about putting together basic information about our planet and particularly the biota of its surface include the subject of the President's message to us yesterday on the possibility of the sea-level canal being built across the Isthmus of Panama by the year 2000?

Dr. GALLER. Yes, sir, it does.

Mr. DADDARIO. In that message, Dr. Galler, there was no stress laid to our coming to some determination about the effect that this would have. The message seemed to refer only to the way in which it could be done, the need for it, and the time during which the construction would take place so that it would be completed.

Now, what ought we be doing?

Dr. GALLER. Mr. Chairman, I think that the proposed interoceanic sea-level canal offers itself as a unique opportunity to embark on a long-term ecological program that has as its primary objective the gathering of fundamental data that could be used to predict some of the consequences, if any, that would result from breaking through a land area into two oceans that have been separate for many millions of years.

Now, I for one am not about to suggest or even imply that there will be any consequences, but I hasten, again, to add I don't know.

I have not discovered as yet a responsible scientist who is willing to predict that there will be serious consequences, but I have yet to find a responsible scientist who is willing to predict that there will not be any consequences. The truth of the matter is we do not know.

If we hark back to the Suez Canal as a kind of crude analog to the transisthmus canal cannot we find that there has been a transport of biota from the Red Sea into the Mediterranean and vice versa as a result of the construction of the Suez Canal. Also there is some evidence that the monumental engineering enterprise known as the Aswan Dam will have some ecological consequences, in terms of changes in the salinity, the dissolved nutrients, and volume of fresh water flowing into the Mediterranean Sea. Ultimately the biota may be affected. There are a number of examples of major engineering enterprises that have resulted in changed environments. Thus, I believe that we would be well advised both as a nation and as a community of scientists to undertake a long-term study of the ecology of the area that provide what Dr. Gates recommended when he appeared as a witness, that is, provide us with a means for predicting consequences that might take place—before they occur.

I should add, Mr. Chairman, that I am much impressed with the sincerity and the dedication of the Interoceanic Sea Level Canal Commission appointed by the President, by the staff, by the engineers of the U.S. Army Corps of Engineers, by the scientists on the Atomic Energy Commission participating in the program, and by the Batelle Memorial Institute that is serving as a principal contractor to study the radiobiological aspects of the program. I am very much impressed with what Batelle is doing. I submit, however, we should be doing much, much more than we are.

Mr. DADDARIO. This would be one governmental activity which is presently in such a position so that it could conform as a national project.

Dr. GALLER. Indeed, sir.

Mr. DADDARIO. With the International Biological Program, too.

Dr. GALLER. Yes, sir.

Mr. DADDARIO. Mr. Lukens?

Mr. LUKENS. Thank you, Mr. Chairman.

I must admit that I am a novice in this field and yet it is somewhat intriguing to me, Dr. Galler. I think you made an excellent presentation. I have some basic questions perhaps which may seem to be coming from misinformation or a lack of information but I would like to pursue them anyway.

No. 1, how many subcommittees are there on the National Committee that could result in requests similar to that you made before this committee today? I am a bit concerned about opening Pandora's box. If we allow one subcommittee to be funded separately or through the NSF, then others may come along with a similar request and with the same justification. I would like to know how many other subcommittees do you think could possibly make the same request of this committee.

Dr. GALLER. Congressman Lukens, I feel that the likelihood of any subcommittee coming directly to Congress with a financial request is nonexistent. The organization of the U.S. National Committee—and forgive me for not having perhaps explained this fully—the organization of the U.S. National Committee is such that it sits at the apex of a pyramid of subcommittees, each subcommittee organized to delve into a particular research problem area. I am not here as a member of a subcommittee nor as a member of the National Committee. I am here as an individual.

However, the subcommittee actually formulates the working programs. These programs are then submitted to the U.S. National Committee for Review and Assessment. And ultimately the program that develops will be the program not of any one subcommittee but the program of the full U.S. National Committee.

Mr. LUKENS. So as a result, this request actually, although made as an individual, perhaps is the result of an effort of the National Committee of the IBP?

Dr. GALLER. No, sir; not the result of the National Committee, but in support of the Committee's efforts.

Mr. LUKENS. All right. Now, along the same line, sir, is it not possible that other programs of other scientific disciplines could also originate in this same request? I just wondered what the likelihood is of similar or identical requests in other scientific disciplines. They are multitudinous, are they not?

Dr. GALLER. Yes. Mr. Congressman, I certainly feel that there is such a possibility. There has been in the past, there is at present and I think there will continue to be so in the future.

Again, the IGY is one example. The International Quiet Sun Year is another example. But if you keep in mind that these are not splinter requests. These are requests for support of unique national scientific endeavors that are carefully coordinated among many groups, and

that involve some of our most distinguished competent scientists. These requests are unique.

I don't visualize many such requests coming through at any one time.

Mr. LUKENS. All right.

Now, we have talked in numbers of persons that could be made available for this program. And you mentioned the number of 25, which I didn't understand. Does that relate to the persons that did service under the original IGY or is that the requested number you thought would be possible to service this program in the future?

Dr. GALLER. Mr. Lukens, in point of fact, I think that in its heyday, at the height of its activity, the IGY had a staff that was larger than 25, but I will defer to my colleagues who really have the complete information.

Mr. DADDARIO. It might be helpful if you could get that for the record for us.

Dr. GALLER. I will be very glad to get that for the record, Mr. Chairman.

(Information for the record is as follows:)

According to the staff office of the U.S. National Committee of the International Biological Program the staff personnel of the IGY fluctuated between 20-25 persons during the most active phase of the IGY.

Dr. GALLER. The figure of 25 is kind of an educated guess on my part based on my experience over the last 20 years. Considering the potential magnitude of our national effort in the IBP I believe that 25 would not be an unrealistic number of staff for the U.S. National Committee.

Mr. LUKENS. For your peace of mind, I might state that I intend to support this request with qualification. Basically, I am interested in how fast we are going to move. I think there is no doubt we have to pursue it. I am all for it, but I would like to know whether we are pursuing this in an orderly way.

Now, to follow this—if you don't mind my taking a couple of minutes.

Mr. DADDARIO. No. Take all the time you want.

Mr. LUKENS. To follow this, what do you think will be the ultimate outcome in terms of a 5-year projection of this program? Would you see any reason to divorce financing or financial requests from the NSF or the National Academy of Science at any point in the future?

Dr. GALLER. I think that as a formal program the IBP, as would be the case in any formal program, should have a cutoff date. It should not go on indefinitely. However, it will go on de facto because the research that is being initiated as part of the IBP will in turn instigate other kinds of research in the future that will involve an increasing number of scientists, technicians, and graduate students. But at some point, the formal program in my opinion should come to a formal conclusion. The IGY has come to a formal conclusion. But I submit that the research initiated during the IGY continues in many forms, in many countries, and in many disciplines.

As a result of the impetus given by the IGY we are receiving fundamental information and will continue to receive fundamental information that is being used in an increasing number of ways. I would hope that the same would hold true for the IBP.

Mr. LUKENS. There can be one additional basic reference point for the launching of this program. At the present time, are there any personnel in a formal program of the IBP, or is it simply a sluff-off or a peripheral benefit from the IGY or existing organizations such as the NSF and NAS?

Dr. GALLER. No, sir, Mr. Lukens. There is a program. It involves scientists. It is new. It is not a sluff-off.

Mr. LUKENS. Pardon me.

Dr. GALLER. It is not a sluff-off from the IGY. It has been initiated by the U.S. National Committee of the International Biological Program through its various subcommittees. There have been a number of working conferences planned. Some of them have already been carried out. There will be an increasing number of planning sessions and as funds permit active research projects will be initiated.

Mr. LUKENS. How are they now funded?

Dr. GALLER. At present they are funded through individual agencies. To the best of my knowledge there is no money set aside for the funding of the active research projects by the U.S. National Committee.

Mr. LUKENS. And how many persons are involved, in total, approximately.

Dr. GALLER. I can't speak to that. I will be glad to get that for the record, Mr. Lukens.

Mr. LUKENS. I would like to make that request.

(Information for the record is as follows:)

According to the staff office of the U.S. National Committee of the International Biological Program the total membership of the U.S. National Committee organization consists of approximately 96 persons organized as follows:

National Committee, 18 members; 9 subcommittees, 2 panels, and several integrated research programs, 78 members; total, 96 persons.

Mr. LUKENS. Thank you, Mr. Chairman. I think that is all I have.

Mr. DADDARIO. Dr. Galler, the points Mr. Lukens raises I think are important.

Dr. GALLER. Yes, sir.

Mr. DADDARIO. You have called attention to the fact that the IBP could produce the trained personnel and the knowledge required to predict and assess those consequences which were those problems you referred to prior to having made that statement?

Dr. GALLER. Yes, sir.

Mr. DADDARIO. These effects which are now taking place on this earth which we know so little about. One of the reasons as I understand it why there is so much interest here is that we do not have trained personnel. We have not developed a national capability to assess these consequences. The IBP gives us a vehicle through which we can accelerate our capabilities both in the training of personnel and in the development of the average. This would naturally leave as a residue a capability of finding its way into the structure of our governmental and private life.

Dr. GALLER. Yes, sir.

Mr. DADDARIO. So we will be better able as a nation to assess those problems which are disturbing the fabrics of our society than we are now capable of doing.

Dr. GALLER. Yes, sir, I quite agree, Mr. Chairman.

I hope that I have not given the impression that I am not well impressed with the programs in ecology and environmental science that have been initiated by the several Federal agencies. Quite to the contrary, I am impressed. I am very pleased to see the initiatives that have developed in the Department of Interior, in the Atomic Energy Commission, in the Office of Naval Research and in the National Science Foundation. With the exception of the National Science Foundation, however, these programs of ecological assessment are essentially agency-focused programs. They address themselves to the solutions of problems that fall within the missions of the several agencies.

The IBP offers us the fabric to construct the canopy of a truly national program, and more than a national program, a national ecology policy that we do not have at present.

Mr. DADDARIO. Well, I don't know that I would be as you so laudatory about the programs which the mission-oriented agencies have in this field. I think they are doing a great deal of work which is helpful. At the same time, I fail to see, in our atomic energy program, for example, the development of a capability which does in fact predict the consequences of these changes. We have had numerous people come before us giving testimony as to the consequences of these activities, and their inability to come to any determination as to whether they are good or bad. I just wonder if you ought to be so satisfied?

Dr. GALLER. I am not satisfied, Mr. Chairman, but I do feel that in general the agencies are doing an excellent job.

Even if we are given all the money requested and assigned the best scientific resources available, it does not necessarily follow that we will come out with answers to difficult problems except over an extended period of time. Some of the consequences that we are discussing today really will not fully reveal themselves except over extended periods of time. So that is a factor to be considered in reviewing existing research programs.

But there is much more that must be done, Mr. Chairman, and gentlemen, that is not being done. I voice my personal prejudices and enthusiasms when I express the view that unless we recognize that ecology deserves a major investment at the national level, we will continue with this present patchwork arrangement that is unsatisfactory for solving national problems that transcend the missions of any single agency, public or private.

Mr. DADDARIO. I would agree that it will take us a long period of time to determine the nature of the consequences if we continue to follow the same line of approach as we are now in the habit of doing. But if we, during the course of the development of our technology, determine that a consequence could come and that it will take us a long time to make a determination to its effect, in most instances adverse, there ought to be built into the research at that time the ability to eliminate the ill side effects.

I don't believe that we are doing a good job in this particular area. We are allowing things to happen about which we have no knowledge and we make no planning to enumerate it at its source. The point

has also been brought to us time and time again that it is not beyond our technical, scientific, and intellectual capability to develop a procedure through which we can at least reduce the consequences which in most instances cause unknown harm.

Dr. GALLER. Mr. Chairman, I disagree with anyone who claims that we do not have the intellectual potential and the technical ability to reduce and indeed to prevent many of the deleterious consequences that have occurred. What we have not had so far is an explicit national action plan that is well coordinated and that includes all sectors of the community that should participate.

I would like to point out that in my opinion the kind of ecology that we are practicing at the engineering level is what I would call pathological ecology. It is pathological in that we are trying to provide ecological therapy for major ills that have resulted from inept and ignorant attempts to manage major factors of the environment of man.

I would like us to develop prophylactic or "preventive" ecology that would enable us to obtain the fundamental ecological information needed to predict what could occur and feed these predictions back, possibly through a National Institute of Ecology, to engineers, to technologists, to industry, to city planners, and others concerned with the design and construction of major projects that involve environmental manipulation.

Mr. DADDARIO. That sounds better than your other statement.

Dr. GALLER. Thank you, sir.

Mr. DADDARIO. Mr. Brown?

Mr. BROWN. I would be interested to know in a very brief way, if you could present it, just what factors led to the synthesis of a plan for something like the International Biological Program or the IGY. Considering the fact that we live in a technological civilization in which science has been developing at an accelerated rate for 150 or 200 years, why is it that it becomes necessary to single out an area of science and focus attention on it? Is it that that area has been lagging in the general course of the development of knowledge or is it that the problems in that area, the pathological syndrome that you have referred to, have focused attentions in that area? What factors led to this kind of a separating out of an area of the more or less seamless web of knowledge in order to focus attention on it at any one time.

Dr. GALLER. Mr. Brown, I think you have mentioned the two most important factors: One a growing awareness that we have been lagging in certain essential areas of fundamental science, and secondly, a dawning, a growing awareness that the ecological solutions that we thought we had were not real solutions; they were temporary panaceas. In searching for causes we begin to discover that the causes in this case are largely technological. It is well and good to support individual basic research projects in any field of scientific endeavor. I am a very strong advocate of free and unfettered fundamental research, but I submit that there is an implicit national mission and justification for such research, the welfare of the community.

One of the factors that led to the creation of the IBP was the growing awareness on the part of scientists primarily "old-fashioned"

kinds of biologists, the men and the women who have lived close to the earth, observing the environment, that the technologists do not have the answers to fundamental, urgent problems. They recognized that the problems will not be solved until we developed comprehensive, coordinated research programs. And what are the foundations for the program: The training of the right kind of scientists, adequate financial support to give them the opportunity to participate in a national program, and provision for an even and full flow of fundamental information from the basic sciences to technology.

Mr. BROWN. I find no disagreement there, but in looking back over the history of this country I find that we have always been sympathetic to the pursuit of science. Take the simple example of the Federal support of science and learning through the land-grant colleges beginning 100 or so years ago, which led, perhaps as much as anything, to the revolution in agriculture, which is a biological phenomenon. This was accomplished without in the same sense focusing upon the problem of agriculture or biological sciences. We didn't say that we needed a technological revolution in agriculture so we could raise the food with our Nation with 10 percent of the population instead of 80 percent and become a vast technological metropolitan civilization, and yet it occurred.

In looking at the IGY, for example, we focused attention here on an area of science which for a generation has had the most financial support and has drawn the most competent scientists. So it doesn't appear that we were filling an area of the greatest need at that particular point, at least in the sense that there hadn't been a great deal of resources already poured into that area. How do you react to that?

Dr. GALLER. Well, I react to it in the following way: First of all, I am not sure that the geophysicists would agree with you that prior to the IGY they had been receiving adequate support. In fact, I think I can speak confidently that they would disagree with you, Mr. Congressman. But I do feel that certainly oceanography, one area of geophysics that I have some familiarity with, physical oceanography certainly received more support than biological oceanography. That support originated during World War II, actually the terminal years of World War II. That support of geophysics was in retrospect a very sound investment not only because it helped solve urgent problems confronting military agencies, but because it facilitated the training of a cadre of scientists that grew until it had reached critical mass. By the fifties, we had developed enough interest, enough facilities, and a large enough nucleus of scientists to be able to launch an IGY. And the reason they were interested in launching an IGY is because collectively they had reached a point where they recognized that investigations of geophysical phenomena transcended political boundaries. More to the point—the geophysicists recognized that certain fundamental questions could be resolved by carefully planned, well supported, cooperative research programs carried out at the national and international levels.

So I would say that the concept of support of the IGY was quite different from the support of individual geophysical projects in the universities and private laboratories in the United States.

And I think the same thing holds true for the IBP. Biologists, especially ecologists have come to recognize that certain fundamental questions will be resolved only through carefully coordinated team efforts. The IBP is pooling the intellectual resources that have accrued through the years as a result of Federal support of basic research. We are now at a point where a coordinated IBP program, can provide a "payoff" not possible through support of large numbers of uncoordinated, individual research projects.

Mr. BROWN. I think our differences may be of degree rather than kind, but I think it is obvious to any scientist, or most laymen, that basic scientific research has always been international in scope, and that the knowledge gained in one country generally is disseminated widely. Now, the mechanisms of research, the coordination of laboratories and so forth, have not been so international, but the flow of the research findings within the mechanical limits that were possible, has always been international. It is one of the encouraging aspects of our civilization, that at least in this area we had an international community, not strictly national.

So the point that you make about the need for an international approach to basic research seems to lose a little of its thrust, unless it involves international coordination of organization and hardware.

Dr. GALLER. It does, Mr. Brown. It involves that.

Mr. BROWN. Getting back to the original question, if we are focusing attention on areas of greatest need—and you lay some weight to this point—why aren't we attacking, then, for example, one or more areas of the social sciences, which every scientist would agree are the most in need. They have never kept up with the physical sciences in any way, shape or form. Is it because they are not yet ready even to go as far as we have here in focusing attention on their needs?

Dr. GALLER. I don't know what the reasons are, but I would like to suggest that the separation of the social sciences from the biological sciences is in some way an artificial one, as is the separation of the sciences from the humanities. This separation is a monument to the ego, the special ego of each separate discipline. This is a luxury we can no longer afford. The kind of problems that we are confronted with today, require mutual respect and close cooperation between the physical, biological, and social sciences. And in my view, except for the technical training aspects, there is little to be said for the separation of the social sciences from the biological sciences.

Mr. BROWN. Well, I might agree with you personally, but still it is not the way our society tends to look on it. They probably draw a sharper line than the scientists themselves do.

Of course, insofar as you put the focus here on ecology and systematics, if my understanding of these terms is correct, I think you would be dealing with social science in the sense of a science of man rather than the fragmented approaches which are characteristic of—well, of biology and some of these other scientific fields per se. Ecology involves a comprehension of man and his environment which requires a knowledge of man, and a knowledge of man is what we generally think of as the social sciences.

Dr. GALLER. Yes, sir.

May I also state, Congressman Brown, the fact that ecology and systematics are two of the most undersupported fields of the biological

sciences. Now, I have seen all kinds of statistics gathered to demonstrate how much money is going into ecological research and I submit that only a small fraction of that money is going into fundamental ecology. We have a tendency to confuse basic ecology with what I call environmental management technology—the applied research. But the underpinnings, the fundamental ecology, that synthesizes information from many disciplines and gives us the model for environmental management, that is receiving far, far too little support. And systematics, the most neglected of the biological sciences for the last 50 years, is terribly undersupported and we are paying a penalty for this now. We are gathering specimens and data, and there are only a handful of specialists around who can identify the specimens and provide the data needed by other scientists.

Mr. BROWN. I gathered from your statement that you feel that insofar as the IGY is concerned, it represented the evolution of science or knowledge in this field to a new plateau, that there were certain permanent benefits in terms of ability to handle, disseminate, collect, and manage knowledge. Permanent benefits that resulted therefrom represent a new step up in our ability to handle this problem.

Dr. GALLER. Yes, sir.

Mr. BROWN. Do you feel a similar result would take place from the International Biological Program?

Dr. GALLER. Yes, sir; I do.

Mr. BROWN. This, of course, is the fundamental problem that faces all of science as it continues to evolve. New and better ways of handling the mere massiveness of knowledge and coordinating it in a better way. It is not restricted to biology or physics or geology or any other field.

Is there a possibility that the other fields of science will benefit from the developments just in the data handling process that derives out of the program we are talking about?

Dr. GALLER. They could, very easily.

Mr. BROWN. I have no further questions.

Mr. DADDARIO. Mr. Yeager.

Mr. YEAGER. Dr. Galler, you mentioned that you felt that the IBP must receive from all sectors of the community and not just the Government.

What nongovernmental sectors are you thinking of?

Dr. GALLER. There are a number of major foundations in the United States. I believe that it would be beneficial to involve them. There are a number of agencies at the State and municipal level who have contributions to make, and I believe they, too, should be involved.

Mr. YEAGER. Would there be a possibility, not in terms of dollars but in terms of people and facilities, that the universities, for example, might contribute in areas where they are not presently planning to?

Dr. GALLER. Very much so, Mr. Yeager.

Mr. YEAGER. To your knowledge, has any concerted effort been made to sound out these nongovernmental sources for funds?

Dr. GALLER. I think some effort has been made; yes. I can assure you that what I am suggesting is not novel. It has been suggested by many others associated in some form or another with the IBP. But here again it becomes a matter of time and people. With the present

staff, it is not possible to do all of the things that would be desirable or really essential.

Mr. YEAGER. Including the fund raising?

Dr. GALLER. Yes.

Mr. YEAGER. One final question. Would you care to comment on the level of effort that has been made so far in our own program with that being made by other countries?

Dr. GALLER. I really can't make a comparison, Mr. Yeager, because I think that our country lagged behind somewhat in developing its national plan. I would say we are somewhere in the middle. The Western European countries, I think, are in the forefront. The Latin American countries are, I regret to say, far behind us. But I would say in terms of the planning to date, I am very pleased with our national committee. I think that the programs that have evolved are first rate scientifically.

Mr. YEAGER. Thank you.

Mr. DADDARIO. We could keep you here a little longer. Our series of questions that come to mind as a result of some of the things you have said, some of the points Mr. Brown has so well raised, I think ought to be provided for the record. If we could take advantage of you further, we will see that we have these questions forwarded to you.

Dr. GALLER. Be glad to answer them, Mr. Chairman.

Mr. DADDARIO. Thank you so much.

Dr. GALLER. Thank you, Mr. Chairman.

Mr. DADDARIO. Our next witness is Dr. Carleton Ray, assistant professor of pathology, School of Hygiene and Public Health, Johns Hopkins University.

We are pleased to have you here this morning, Dr. Ray. Sorry to keep you waiting for so long.

Dr. RAY. Not at all.

Mr. DADDARIO. We are very anxious to hear from you.

**STATEMENT OF DR. CARLETON RAY, ASSISTANT PROFESSOR OF
PATHOLOGY, SCHOOL OF HYGIENE AND PUBLIC HEALTH, JOHNS
HOPKINS UNIVERSITY**

Dr. RAY. First, I think I would like to bring to the attention of Mr. Lukens the pamphlet: "The U.S. Participation in the International Biological Program."

It gives an outline of the IBP. It also gives the leadership of the IBP and its aims.

Mr. DADDARIO. I am sure he has a copy. But if you would leave one here—

Dr. RAY. I am afraid I can't, this is my only one.

Mr. DADDARIO. All right, Mr. Yeager advises me that we have some and that all of the members of the committee have received their copies.

Dr. RAY. Very good.

I have a prepared short statement.

Mr. Chairman and members of the Subcommittee on Science, Research and Development, I am honored to express my views on the

International Biological Program before you today. It happens that my views are fairly strong ones, very much in favor of a special appropriation to the IBP by Congress, but I must stress that although these views are shared by many of my colleagues participating in the IBP, the following is a personal statement.

There appear to be two schools of thought on the IBP.

First, it is a worthy enterprise to which adequate lipservice may from time to time be given—as so often is the case in international affairs—and for which the funding may be handled by existing agencies, Government and otherwise. Second, the IBP is a unique program analogous to the International Geophysical Year and is not encompassed by any agency or series of agencies. The second view, to which I subscribe, reminds us of the success of the IGY and points to its special appropriation. The second view states, IBP is either worth doing right as its own entity or not worth doing at all.

Nevertheless, we must begin with the aims of the IBP in order to determine its worthiness. IBP has as its theme the biological basis of productivity and human welfare. In determining whether this is a worthy cause, I am reminded of Charlton Ogburn's book, "The Winter Beach, William Morrow, 1966," pages 16 and 17, which reads:

Without being one who delights in illustrations of man's puniness in the limitless cosmos, a traveler may yet find it easier to bear the spectacle of the monumental ugliness we have wrought from the spoil of our countryside, the trash with which we have defaced it, the suppurated encrustations that our cities have become, by reflecting that the earth through its characteristic processes can, and one day doubtless will, shrug it off in favor of a fresh start.

A dramatic and somewhat emotional statement, agreed, but no less true. If we do indeed care for future generations, which I mostly doubt we really do, we simply must find out what those "characteristic processes" are on a world level, or the dire predictions of the biologists and demographers from Malthus to present will come true.

Can the IBP find out about these processes? On an international level, it appears to have the best chance so far and we note that its theme is directed exactly to that end—productivity and human welfare. And whereas I can think of no single really good reason to take from one pocket to put into another, I also can think of no single reason why we need a man on the moon prior to our learning to live right here with nature. Clearly, we do not know how at present, and to enforce this idea, I again resort to a quote, this time from Dr. Bostwick Ketchum in a letter of the Marine Productivity Subcommittee:

Only through the development of this basic understanding of the multitude of processes which combine to make the study of marine productivity can we hope to insure that future developments can proceed with minimum risk of destruction of our natural resources and beauty.

It happens that my field of interest is marine ecology, more specifically the polar marine mammals. I should now like to address my remarks to marine ecology. We are told, in the face of the depletion of space and resources by land, that the sea's unlimited resources shall solve all our problems. The Prophets of Panacea inform us of mineral and biological wealth unbounded. Yet we would remind this school of thought of several things:

(1) The sea remains to this day an area of international *laissez faire* where development proceeds largely willy-nilly and it is precisely in this way best to destroy resources, living or otherwise.

(2) The sea is by no means limitless. Much of it is desert, in fact.

(3) Much of the sea's resources are already depleted: whales, salmon, coastal fishes—the list is probably endless.

(4) It is tremendously expensive to operate by sea, especially where depth is concerned. That is, the sea is just a difficult place to be and much of what is promised takes on the aura of "pie-in-the-sky."

(5) The processes of productivity in the sea are unknown mostly. We have but scratched the surface in spite of all the underwater vehicular travel. Here we return to the first point—how best to capture knowledge, which we all must admit is a first step, but on an international basis.

(6) Lastly, in the history of human affairs there have been two major revolutions, the agricultural and the industrial. Now we are in the midst of a third, the marine, wherein man attacks and conquers (as is our egocentric wont to say) the 71 percent of the planet Earth, as an urgent necessity following the decimation of the land. But if we speak of revolutions, let us remind ourselves that in knowledge and technique, man is still preagricultural by sea. He is still the hunter-gatherer there. Significantly, the IBP PM Subcommittee calls for a major research effort in the field of planned modifications of the environment, by which is meant, at least in part, increased productivity, farming, if you will.

The worthiness and necessity of a vastly expanded U.S. marine sciences effort is well-known to us all. As cases in point, we recall the reports "Effective Use of the Sea," The White House, June 1966, and "Marine Science Affairs—A Year of Transition," the First Report to the Congress on Marine Resources and Engineering and Development, February 1967. Now these worthy publications are replete with graphs, charts, data, and words which boil down to and provide some fire for "we'd better get moving or else." But they are also replete with international implications and complexities which will only be solved by a world effort.

I maintain and am willing to defend within the limited realm of my knowledge, that the long-range International Biological Program is the best means today for getting moving as far as finding out about the sea's biological resources goes.

I am also willing to defend the proposition that for all sorts of reasons the sea's biological resources are its most important by far. We are biological organisms and in spite of Judeo-Christian tradition which teaches *use of* the land, we now know that we must *live with* the land (and sea) or perish. To reap biological harvest from the sea is not likely to damage other resources, but to reap geological and mineral harvest carelessly can ruin the biological harvest. In this connection, the sea is not like the land for the waters are a continuum and no matter what is done or where, eventually it is worldwide by sea. Hence the real concern over atomic waste disposal by sea.

Now, specifically, why a special appropriation for the IBP? For one thing, it is perfectly possible to have a program approved by IBP, yet turned down by one or many of the funding agencies in the Federal or private systems. Many or most of the IBP's aims are not appropriate for these agencies which tend to be much more jurisdictional and provincial than the IBP calls for. For another, with-

out being specific, it is not good in practice to force such large enterprises as IBP to go begging to agencies which too often have their own interests to defend and which do not like doing the bookkeeping and financing for other agencies.

No; I do not suggest or even foresee the creation of another empire. But I do see the need to fund world problems differently than more local ones, and I do see the absolute necessity for putting some toothy funds behind House Concurrent Resolution 273, which endorsed the IBP. I'm afraid we are going to look pretty silly if we don't. The conflicts of interest will be too obvious and the program will not, I would venture to say, achieve wide support from the biologists.

The funds most needed are those for ecological and other field-oriented research, for training of personnel, and for data-sorting centers. There should be a real effort made to keep the overhead minimal and Parkinsonism at bay. As things stand today, however, the IBP has no real teeth, can endorse, but not fund, and cannot possibly do the job it has set out. It is a bit like trying to build a good pie with 100 bakers directed by a committee. The IBP's pie needs the vintage fruits of research from an oven stoked with the wherewithal of a congressional appropriation.

Thank you, Mr. Chairman.

Mr. DADDARIO. Mr. BROWN.

Mr. BROWN. I would be interested to know, if you have thought about it, to what extent do you feel that the IBP could be supported by the mission-oriented agencies in contrast with the more general types of support that you are discussing here? Is there a percentage that you could pick out of the air?

Would you say that perhaps 20 or 50 percent of the program could be supported by the mission-oriented agencies?

Dr. RAY. I really don't know, sir. I must point out that I am a biologist with not too much administrative background and I have to phrase my answers in that context.

I would be hesitant to pick a percentage. I think that this will come from the proposals that are received by the IBP itself.

It may be that the IBP National Committee can very well direct a proposal to an appropriate agency which has within its scope the funding of such a program. It may be, as is the case with some that I know of and am connected with, that there is no appropriate agency.

I think it would be a little premature for me to say exactly what that proportion might be.

As you pointed out earlier, in discussing the matter with Dr. Galler, there is international cooperation among scientists anyway. There will continue to be international communication, IBP or not. However, some programs, as those having to do, for instance, with marine mammal population, with which I am connected, can't be funded through any single agency I know of.

Mr. BROWN. We have discussed in this committee the ways of handling this administratively. I guess the most often referred to process would be through the National Science Foundation and then through the Foundation to the committee or to other groups that would be interested. Do you see any problems in connection with funding that would go through these channels, specifically the National Science Foundation?

Dr. RAY. From the biologists' viewpoint I can see no problems whatsoever. I would be delighted with the arrangement myself. From the administrative viewpoint I could see that some problems in jurisdiction might arise. Some people in various agencies would rather administer only research in their own jurisdiction, rather than through the IBP. For instance, if I were to submit a proposal to the IBP, as I have, and had them approved, these proposals having been reviewed by competent scientists, even this at present does not lead directly to funding. It does lead to a vast amount of paperwork, but does not necessarily mean very much. It may help with some agencies, and in some other agencies I suspect it could possibly even hinder the funding of a certain program. But personally I think the arrangement as used for IGY would be a very satisfactory one—that is, through the NSF.

Mr. BROWN. I have no further questions, Mr. Chairman.

Mr. DADDARIO. Dr. Ray, I just wonder if we ought to go so far as saying that it is either worth doing right in its own entirety or not worth doing at all. It seems to me this is an important proposition which we ought to try to start even if we run into some funding problems and if we can't sell it in its entirety. I don't know that we ought to separate the IBP and its ability to handle all of its problems by itself from partial funding from some of the mission-oriented agencies.

I say that for this reason. When this committee looked into the whole basic research picture of the Government and when we made certain recommendations to restructure and strengthen the National Science Foundation, we had to wrestle with this problem of who ought to do basic research and who ought not. We came up with the idea that mission-oriented agencies would be better off if they were allowed to continue doing as much basic research as they could possibly get support for. As I look back on that argument, I see some problems emerging at this time. The support we should give is to develop a viable program here, and to see to it that we also involve the mission-oriented agencies. So we will develop within them a feeling for the importance of this program. When this has been developed it will in its remnants, as Dr. Galler has pointed out, leave something with us which will be part of the structure of our whole scientific effort.

Dr. RAY. I think that I would like to modify my own statement to agree with you in getting the mission-oriented agencies fully involved. I don't mean for a minute to say that they shouldn't be or that they are not coping with problems that are international in nature, as they will for a long time and have in the past. I don't mean by saying that we should do it right or not at all that I belong to the group of either getting everything you want, like a baby with a lollipop, or sitting down and weeping. I don't agree with that at all. However, I do think that the program that is outlined by IBP is pretty large and very important. I think we can definitely say that we have reached a point in the course of human affairs where the biological and social problems are probably the most serious problems that we face today. I think this was pointed out before the U.N. by an atomic scientist a few years ago. I think that if we really want to go through with this program, if it is worth doing right, we have either got to get the central organization and funding for it or modify the program as outlined or I fail to see how it can come to fruition in the 5-year period it is given. And that is what I mean by that statement.

Mr. DADDARIO. Well, I think it is important for us to discuss it. I didn't take it really that that was what you intended to mean.

I agree with you, it is important. This committee finds generally that when we have a request such as this come before us from the administration, that there is a great deal more to it than either they or we originally saw. We have tried to feed into these hearings as much thinking as we possibly could, so that we might be able to generate around it the kind of support through testimony such as yours, which will have a meaningful effect on the other Members of Congress. I do agree with you that it is a very important subject.

We have had the recommendation made to us, as an example, sometime in the past by one of the witnesses that we ought to be developing at this time the scientific and technical capabilities which the country now has available in the field of theoretical ecology.

Dr. RAY. Yes.

Mr. DADDARIO. This fitted in with social science in a very strong way.

What is the feeling amongst you about where we stand now in this whole area? What do we need to do? Why do we need to do it? How many people ought to be trained? How far away are we from having the kind of national capability that we need in this whole field of biology and ecology?

Dr. RAY. That is a loaded question and difficult for me to answer. I think we are quite a way from accomplishing this myself. But I must define the way that I personally think about these things.

I have been involved in the field, for instance, of conservation for a long time. Not all scientists are essentially conservationists or vice versa. Now, immediately, when you take in the field of conservation you bring in the social sciences and a certain amount of consciousness, rightly or wrongly, about the effects of what you are doing biologically.

It appears to me that the biggest problem we face is not the solution of why a river is being polluted, for instance, or what happens to a bit of atomic waste dumped in the north and how far it takes to go south. This can be found out—well, it can't be found out quite that authoritatively, but eventually that data can come and we have a nice report sitting on our desk. The problem is what are we doing about it? Now, here is the place you pointed out, and Mr. Brown, it is well and good to find out about these problems, but it is like birth control in India. What exactly do we do about it? How do we integrate this body of knowledge? And here is where I think the IBP has its major role, not only in data gathering and data processing but possibly putting before various agencies, from State to international, the information that they need to solve some of the very great social problems. Because the IBP is all about human welfare. This is the place we lag. Perhaps there should be an international social sciences to go along with the IBP, I don't know.

I suspect we are going to have to come to that. I think the scientific technicians as well as scientific personnel will always rise to meet a demand. I believe we are going to be able to train the number of people in all sciences as these demands come up; even marine science, which lags far behind. I am in underwater marine science, diving, and vehicles, and such things. We don't have very many technicians, but they are proliferating at a fantastic rate.

However, I really don't see very much hope in only gathering data and technicians. It is as if you had two lines on a graph. The problems go up and the solutions go up but they never go up quite as fast. This is the area that is so important and this is the place where the IBP should receive a maximum amount of support, into solutions. We are putting before us lots of problems. We have problems coming out of our ears. But I think we are a long way from implementing the problems. We always see problems in retrospect, as came up in your discussion about the canal. We should see problems before this arise, if we possibly can.

Mr. BROWN. May I interject a point here?

You are getting suspiciously close to a field which is outside the scope of science, one of normative judgments. You speak of correcting the problem—take a simple problem like atomic waste disposal in the ocean. You are assuming that it is good not to pollute the ocean with atomic waste. Now, that is a normative judgment and outside the field of science. It may well be that the field of science will merely tell us how we can best live with an ocean fully polluted with atomic waste. Are you contemplating some philosophical aspect of this approach to the biological program? Are you assuming maybe that this ought to be sort of an international philosophical year as well as an International Biological Program?

Dr. RAY. It would be perhaps advantageous to combine the two.

Mr. BROWN. You are even assuming that conservation is good. It may well be that so far as science is concerned the best kind of a world will be one in which we eliminate everything except man and live by the synthesis of raw materials from the atmosphere or something like that.

Dr. RAY. Well, I wouldn't care to live that way myself, obviously.

Mr. BROWN. That is normative judgment.

Mr. DADDARIO. Is it a normative philosophical judgment or not? Isn't it subject to scientific judgment?

Dr. RAY. This is what I was going to get to. I think the things you have raised are scientific judgments. For instance, you can call on all sorts of examples. What are the limits that we put biologically around ourselves: psychologically, living in the kind of world we have to live in; radioactively, how many people can a certain area support in terms of productivity; what would be the consequences psychologically of eliminating all creatures of man. I think it would be disastrous.

Mr. BROWN. Aren't we thinking of putting men on the Moon, which has none of the things you are talking about, and creating an environment there? What makes you think it is more valuable to have the kind of environment we have on Earth than it is on the Moon or Mars or Venus?

Dr. RAY. I think here we are getting philosophical. I wouldn't want to be deterred from the main point, which is that certain problems definitely do exist. They are subject to scientific analysis both in pure science and practically. Once we solve these problems scientifically, once we have this data, then you get into the field of social science. Even when we do not know the effects of our actions—and here I agree with the chairman—it is advisable to build into our planning sub-programs to find out what the effects are. These are things that are all subject to scientific analysis.