

environmental deterioration we are all so concerned about might have been avoided before it started.

Nevertheless, ecology is only at the beginning of a new phase of discovery. An enormous amount of high-grade research needs to be done, and done quickly, before some of society's major questions about environment can be answered. What is coming to the fore is a concept of the ecosystem, and a wholly new set of procedures for studying ecosystems on a regional scale, so that the requisite research in many field projects must now be multidisciplinary in practice as well as in the library.

Much is now known about ecosystems, which range in scale from drops of water to 10-acre corn fields, and some extremely ingenious theories are available to account for the behavior of complex systems and to help model them so that their variations under experimental manipulation can be tested by the computer. These computer-simulation techniques are particularly helpful when dealing with forest stands, small lakes, estuaries, and so on which are too large to be experimentally manipulated in the usual ways. But the results of all such studies, however informative, cannot be extrapolated upward to larger patches of nature, containing more environmental and biological diversity (and especially those *containing* man and his artifacts), without loss of rigor and predictability. Ecology therefore needs to deal with systems on a large enough scale to be both meaningful and helpful, and it is for this kind of research that new modes of team work and multidisciplinary collaboration are being developed.

Some of the reasons for conducting team research on ecosystems have been spelled out in the Study Committee's plan for a national institute of ecology, on the pattern of the National Center of Atmospheric Research (NCAR). Personally I subscribe wholeheartedly to this idea. It seems to me the first really workable idea for bringing ecology to bear on major environmental problems that has appeared during my career. I recognize that it is not a panacea. "A *research* institute is not the only mechanism, or necessarily the best mechanism" for fulfilling *all* the responsibilities of ecology to public information, education and advice. Nevertheless, having talked a good deal with atmospheric scientists over many years and also during this year's term with the National Science Foundation, I am profoundly impressed by the appropriateness of the NCAR model and intend to do all I can to demonstrate its application to ecology. It seems to me there are very many points of similarity—procedural, educational, organizational, and motivational—between ecology as we see it now and atmospheric science as it was seen by a number of farsighted scientists 15 or so years ago. More than that I will not say just now, because I know that Dr. Cole will be discussing the Study Committee's plan with you this morning.

In urging the need for this new mechanism for conducting largescale team research on ecosystems, I do not want to be heard as insisting that ecology must abandon its older ways of doing things. Ecological research as conducted in hundreds of independent institutions by thousands of able scientists and their students is very good and getting better all the time. Scientists prize their intellectual independence, and that independence of inquiry must be preserved. There must always be room for the cantankerous or even misanthropic genius who will vigorously reject the idea that this research and teaching should be "programmed" by "those bureaucrats running that National Institute," or in fact programmed by any mind but his own. I say only that *some* kinds of ecology can *only* be done by well organized teams, that those kinds are among the most important as well as the most exciting kinds, and that some portion of the profession is ready and eager to conduct these kinds of ecology in the field.

Not being partial to being programmed myself, I have come rather slowly to this way of thinking. One of the main sources of my present conviction is the emerging U.S. portion of the International Biological Program, the portion that is now regarded as central and is called *Analysis of Ecosystems*. As this Subcommittee knows very well, the IBP has not sold itself equally vigorously in all quarters, and "friendly critics" have not been lacking who fear that the entire enterprise may be a gigantic boondoggle. Speaking now as an old Eagle Scout who knew what a boondoggle was before 1933, I assure you that IBP is not a boondoggle, that it is in many ways the most exciting program in natural science to appear since the IGY (International Geophysical Year), and that a very great deal of shaping, organization, scrutiny for high quality, etc., has taken place during the last few months within and around the U.S. IBP Committee.

To show where my own priorities lie within the framework of this program, and to stress the points that I think need stressing if the program is to appeal widely on its merits, I have drawn up a little statement about the IBP which is