Effects of a Polluted Environment: Research and Development Needs (1977)

Pages
112

Size
8.5 x 10

ISBN
0309300398

Panel on Effects of Ambient Environmental Quality; Environmental Research Assessment Committee; Commission on Natural Resources; National Research Council

Analytical Studies
for the U.S.
Environmental Protection Agency

Effects of a Polluted Environment

RESEARCH AND DEVELOPMENT NEEDS

A Report of the Panel on Effects of Ambient Environmental Quality to the Environmental Research Assessment Committee

**Commission on Natural Resources
National Research Council**

**NATIONAL ACADEMY OF SCIENCES
Washington, D.C. 1977**

NOTICE

This report is one of several commissioned by the Environmental Research Assessment Committee for use in its study of the role of research and development in regulatory decision making in EPA. The views expressed herein are those of the Panel on Effects of Environmental Quality and do not necessarily represent those of the Committee.

The project of which this is a part was approved by the Governing Board of the National Research Council, whose members are drawn from the Councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the panel responsible for the report were chosen for the their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors, according to procedures approved by the Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

This study was supported by the Environmental Protection Agency.

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PREFACE

The Panel on Effects of Ambient Environmental Quality was one of four panels commissioned by the Environmental Research Assessment Committee (ERAC) to identify scientific and technical information needed for effective regulatory decision making. The reports of the panels are part of the assessment by ERAC of the role of research and development in the U.S. Environmental Protection Agency (EPA), an analytical assessment that is, itself, one part of a more extensive study by the National Research Council of the acquisition and use of scientific and technical information by EPA in its regulatory decision making.

The prime objective of the ERAC study was to examine the processes by which information is acquired by EPA through research and development. Because these processes have both managerial and scientific aspects, the Committee divided its work into two parts. One part, concerned with the organization, coordination, and management of research and development to support the agency's mission, is the subject of a separate report by the ERAC itself. The other, which deals with the identification of technical opportunities for research and with strategies for guiding research planning, was divided among the four panels. The report of the Panel on Sources and Control Techniques deals with research needed on the generation of residuals and strategies for their control. The report of the Panel on Fates of Pollutants deals with research needed on the transport, transformation, and accumulation of pollutants in the environment. The report of the Panel on Environmental Impacts of Resources Management deals with research needed on the environmental consequences associated with the development and use of natural resources.

This report, prepared by the Panel on Effects of Ambient Environmental Quality, deals with the identification of fundamental and important needs for information on the effects of pollutants and other environmental changes on humans, other living things, and the nonliving environment. In its charge to the Panel on Effects of Ambient Environmental Quality, the ERAC asked the panel to recommend research strategies and priorities that would produce information on the effects of environmental agents on human health and the environment, critical to the EPA's needs in setting ambient standards and other regulatory decisions. The objective of this panel's study was, therefore, to identify questions that EPA needs answered and to suggest effective ways to apply the nation's diverse capabilities to the task of finding those answers.

The discussion and recommendations presented in this report are a synthesis of the contributions of individual panel members. In the course of the study, panel members prepared background papers; these papers are listed in the Appendix of this report. Copies are available upon request from the Environmental Studies Board, National Research Council, 2101 Constitution Avenue, Washington, D.C. 20418.

The Environmental Research Assessment Committee wishes to express its appreciation of the contributions made by the members of the Panel on Effects of Ambient Environmental Quality in the preparation of this report, for the cooperation of the members of the various agencies and institutions, and for the assistance and support of Dr. William J. Scott of Children's Hospital, Cincinnati, and Dr. Leroy H. Wullstein of the University of Utah, who served as consultants. It also wishes to acknowledge the contributions made by the staff, particularly the dedicated work of Dr. Edward Groth III who, as Staff Officer for this panel, provided invaluable direction, support, and editorial assistance.

John M. Neuhold
Chairman
Environmental Research Assessment Committee

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SUMMARY

The purpose of this study was to identify fundamental and important needs for information on the effects of changes in the quality of the environment, and to recommend research programs and strategies for meeting those needs. Several organizing concepts and central themes related to research on effects of pollution are emphasized throughout the report:

1. The complexity of environmental exposures and causative processes and the almost limitless number of substances that are or may become environmental contaminants make a substance-by-substance approach to research on effects impractical. Instead, this report examines categories of effects that may be produced in various living and nonliving receptors. Research on these effects cannot be all-inclusive; instead, it must build and refine a framework of general principles, based initially upon detailed understanding of a limited number of effects of specific substances.

2. The task of understanding effects is multifaceted, and each disciplinary approach (e.g., epidemiology, toxicology) can shed light on only a part of a problem. In any area, be it human health, ecology, or effects on climate, a multidisciplinary, problem-oriented strategy for research is essential. In order to achieve an adequate understanding of a cause-effect relationship, congruent results are needed from laboratory tests, measurements of effects in the field, and studies of mechanisms of action.

3. There are definite limits to what can be learned from research on the effects of pollutants. Inadequate basic knowledge and the sheer complexity of processes being studied make it impossible except in isolated instances to answer with certainty the kinds of questions posed by, for example, interactions among multiple causative agents or the long-term effects on climate of the combustion of fuels. Regulatory decisions usually must of necessity be based on partial information and on extrapolated estimates of risks; nevertheless, research can narrow the range of uncertainty. Furthermore, long-term research on fundamental scientific

problems is the most effective way to unravel such mysteries, and studies of this kind must be supported for their value to future decision making.

4. We recognize the need to establish priorities for allocating resources among research programs on different aspects of the effects of a polluted environment. We believe firmly that it is vital for a national research strategy to maintain diversity and balance among different problem areas (e.g., health, ecological effects) and research approaches (e.g., toxicological testing, simulation modeling). Research planning must also balance the need for research to support immediate decisions with the need to increase fundamental scientific understanding. That said, this report does identify some priorities among information needs within particular problem areas or research approaches. Our recommended priorities are based on an assessment of an appropriate comprehensive strategy for all research on effects of pollutants; they reflect judgments of the usefulness of different kinds of information to the advancement of knowledge needed for both current and future environmental management decisions. We recognize that these priorities may not be entirely appropriate for the Environmental Protection Agency (EPA), with its specific legislative mandates and deadlines and the shifting political demands being made upon it. Our charge, however, was to address information needs in the broader context.

GENERAL CONCLUSIONS AND RECOMMENDATIONS

Chapter 2 addresses problems that apply to research in several different problem areas, and recommends some elements of a general strategy for research.

The solution to some problems that are vital to understanding effects of environmental changes requires a long-term commitment of effort that is usually not possible in an atmosphere of rapid responses to a shifting array of new issues. We therefore recommend that such research be conducted in institutions without day-to-day regulatory responsibilities.

In our judgment, the top priority that has been assigned to research on effects on human health in the past has been somewhat at the expense of investigations of other kinds of impacts. In the future, added emphasis should be given to research on effects of pollution on critical ecosystem functions, such as the flow of energy and cycling of nutrients.

There is a pressing need for estimates of the costs of pollution that are properly grounded in economic theory and make the best use of available data. We recommend that economists and natural scientists interact as early as possible in the planning and conduct of research, so that data gathered will be useful for economic as well as scientific analyses.

In order to take advantage of the opportunities for research presented by well-defined episodes of heavy pollution, an improved capability should be established for rapid investigations of the effects of such incidents. Such a rapid-response function should draw upon the resources of both EPA and the Center for Disease Control.

The way in which research results are used in the decision-making process is also examined in Chapter 2. We conclude that policies should be adopted to foster extensive peer review and critical evaluation of the scientific basis for regulatory decisions. The failure to make public a rigorous assessment of research data has in the past been a part of the cause for reversal of some of EPA's decisions. We recommend, therefore, that EPA publish detailed summaries of the evidence it considers, and of the scientific basis for its judgments of the validity and credibility of specific studies.

RESEARCH NEEDS ON EFFECTS ON WEATHER AND CLIMATE

Pollution-induced changes in climate, should they occur, would probably have more far-reaching consequences than any other effects discussed in this report. Research to model the mechanisms of climatic change is unlikely to resolve critical uncertainties in the timespan during which crucial policy choices must be made in such areas as energy development. Research should be undertaken, therefore, using somewhat simpler models, to estimate at least the upper and lower limits of potential climatic effects.

Chapter 3 assesses research needs on the effects of pollution on weather and climate. The highest priority for study in this category is to define the limits of possible climatic change that might result from carbon dioxide emissions from the combustion of fossil fuels. Other important information needs include the biological and climatological consequences of modification of the ozone layer, and impacts on local or regional weather, climate, and biota of sulfur emissions from power plants. It is most important to recognize the international character of each of these problems, and thus of any contemplated remedial actions.

RESEARCH NEEDS ON EFFECTS ON HUMAN HEALTH

Measures to protect human health from effects of environmental pollutants will continue to require multifaceted research support, as examined in Chapter 4. At present, the greatest need is for more and better information on the health status and mortality rates of populations exposed to polluted environments; the highest priority for research, therefore, should be given to epidemiological studies. Improved registries of mortality and morbidity data are an essential prerequisite to such research. Recommended areas of emphasis in epidemiology include studies of the roles of environmental factors in causing cancer and chronic degenerative diseases; increased surveillance of populations for mutations and birth defects; and studies to correlate behavioral effects with other indicators of toxic response.

In the field of environmental toxicology, we recommend that the highest priorities be assigned to the refinement of bioassays for rapid screening of potentially toxic substances for such properties as carcinogenicity and mutagenicity, and to advancement of techniques for testing exposures to mixtures of agents. Additional areas of toxicological research that should be emphasized include studies to determine the functional correlates of behavioral effects and investigations of developmental defects caused by prenatal exposures to pollutants.

The third important area of research related to health effects is investigation of the mechanisms of action of toxic agents. We recommend that research be concentrated on the development of increasingly detailed knowledge of a relatively small number of critical metabolic pathways for a limited number of agents, rather than dispersed among efforts to describe parts of the picture for a large number of substances.

RESEARCH NEEDS ON EFFECTS ON WILD AND DOMESTIC ANIMALS

As in other problem areas, an integrated multidisciplinary approach is required to determine the effects of specific environmental pollutants on particular species of animals and to support the formulation of some general principles. In Chapter 5 we recommend that highest priority be placed on closely coordinated laboratory and field studies to examine the subtle effects of chronic low-level pollution on the behavior and reproduction of animal populations. Closely related needs are for studies of the consequences of such effects for populations in ecological communities, and for information on the physiological and ecological mechanisms of action of pollutants. We also recommend active exploration of the usefulness of epidemiological studies of domestic animals, both as a tool to detect effects on animals and as a sentinel for possible hazards to humans living in the same environments.

RESEARCH NEEDS ON EFFECTS ON AGRICULTURAL AND FOREST PLANTS

The greatest need for information about pollution-induced injury to plants is to determine the effects on agricultural crops and forest vegetation of chronic exposures to low-level contamination of the environment. The development of such knowledge will require coordinated research programs, described in Chapter 6. The critical needs include monitoring the atmosphere and precipitation in agricultural and forest areas to determine contaminant levels; laboratory chamber studies to obtain dose-response data for vegetation damage under controlled conditions; more extensive field studies to measure injury under actual outdoor growing conditions, including some work using experimental enclosures; and investigations of the mechanisms of pollutant action and plant response.

RESEARCH NEEDS ON EFFECTS ON BIOLOGICAL COMMUNITIES AND ECOSYSTEMS

Research on effects at the community and ecosystem levels of biological organization requires quite a different approach from that used to study effects on organisms or species. The elements of such an approach are described in Chapter 7. In order to measure and predict effects on critical ecological characteristics such as community structure and diversity, productivity, energy flow, or nutrient cycles, research programs should integrate the results of laboratory studies using microcosms and other simplified model systems, controlled experimental perturbations of biological communities in the field, simulation modeling techniques, and long-term field observation of critical indicators of the structural and functional integrity of ecosystems. Experimental perturbations in the field deserve highest priority for support of environmental decisions. The study of nutrient cycles is highlighted as an example of an area in which research resources might best be concentrated in order to learn more about the effects of pollutants on ecological processes.

RESEARCH NEEDS ON EFFECTS ON MATERIALS

Damage to inanimate materials due to environmental pollutants is extensive and costly. The most critical need as far as understanding and preventing such damage is not increased study of the problems, but improved coordination of existing research programs and more effective dissemination and use of available knowledge. For this reason, our highest-priority recommendation in Chapter 8 is that lead responsibility for federal government research on environmental effects on materials be assigned to a single agency. In addition, some particular research areas that deserve emphasis are studies of the mechanisms of deterioration and methods to prevent such damage where it occurs in the form of corrosion of metals, damage to organic materials, and destruction of irreplaceable objects such as works of art, monuments, rare books, and historic documents.

INSTITUTIONAL ARRANGEMENTS

Our recommendations for research programs are accompanied by suggestions for assignments of responsibility for the conduct, support, and coordination of the research. We have emphasized the roles of many different agencies of the federal government in environmental research; but, where appropriate, roles for investigators in state agencies, universities, industry, and other private research institutions are also described.

Suggestions for apportioning responsibility among agencies are included in our recommendations for research on most problems. These are based on knowledge of past contributions and current programs and of the legislative mandates, research needs, and mix of expertise of the different agencies; and on our judgments of the kinds of research programs best suited to carry out particular recommended investigations. In some cases, even though certain research is very important for environmental protection, we recommend that EPA should not devote its limited resources to such studies, because sufficient research of high quality is already being done elsewhere. In many areas, we recommend that a number of agencies and institutions should cooperate to use their complementary strengths and capabilities to carry out needed programs. The numerous examples of problems that require multifaceted study add significant weight to the need for close coordination and collaboration among agencies with common research interests, a need that is so often cited but so seldom effectively achieved. Needs and mechanisms for coordination of federal environmental research are addressed in detail in the report of the Environmental Research Assessment Committee (National Research Council [NRC] 1977).

REFERENCE

National Research Council (1977) Research and Development in the Environmental Protection Agency. Analytical Studies for the U.S. Environmental Protection Agency. Volume III. A Report of the Environmental Research Assessment Committee, Environmental Studies Board, Commission on Natural Resources. Washington, D.C.: National Academy of Sciences.

CHAPTER 1

INTRODUCTION: NEEDS FOR INFORMATION ON THE EFFECTS OF A POLLUTED ENVIRONMENT

Many changes in the character of the environment that result from human activities can affect human health, plants, animals, biological communities and ecosystems, materials, or weather and climate. Some of these effects are beneficial, some adverse, and some of indeterminate significance. Most changes in the environment that are inimical to human health or well-being are unintended results of activities pursued to enhance that well-being. When human behavior, institutions, or economic pursuits must be adjusted to reduce or prevent those adverse effects, other social objectives or strategies for achieving them may have to be modified or sacrificed. Selection of the optimal strategy for protecting or enhancing environmental quality requires that decision makers either reconcile such competing goals, forge a compromise, or, at worst, choose one over another. Such decisions must ultimately rest on social, rather than scientific, considerations.

While scientific and technical information is not in itself a sufficient basis for decisions, it is an essential prerequisite. Decisions to protect the environment require reliable scientific information on:

- the physical and chemical nature of environmental pollutants;
- the size, discharge characteristics, and distribution in space and time of sources of substances of interest;
- processes of transport, transformation, degradation, and removal of contaminants within and among the various compartments of the environment;
- concentrations and durations of exposures that may be encountered by diverse receptors;
- the effects of such exposures on those receptors;

- the biological, economic, and social consequences of those effects;

- the availability and effectiveness of techniques to control or reduce the level of contaminants in the environment; and

- the costs and benefits of those control measures.

Among these categories, information on effects of environmental changes is of pivotal importance for decision-making, for without knowledge of certain, likely, or possible effects, there would be no impetus to take action. Furthermore, information on the magnitude of effects must be compared with estimates of the costs of protective measures in order to decide what action is appropriate (NRC 1975).

AN APPROACH TO ASSESSING RESEARCH NEEDS ON EFFECTS

Ideally, decision makers would like to know all of the possible effects on all types of receptors of every potential or actual change in the environment (including but not limited to introduction of contaminants), before taking regulatory action to limit or prevent such a change. Since it is patently impossible to obtain information in such blanket fashion, research must concentrate instead on the development of detailed knowledge of selected specific effects of a limited number of environmental agents. From that information, broad principles for evaluating the risks of changes in the environment may be deduced, to be tested and refined by subsequent research. Planning for such investigations must be well-grounded in an understanding of the nature both of effects and of research. Unless knowledge is organized into a logical framework of this kind, it is very difficult to identify insoluble problems or gaps in information, or to establish priorities for the effective allocation of limited research resources to efforts to close the gaps (Caldwell 1976).

The Nature of Effects of Environmental Pollutants

An effect of an environmental pollutant is expressed as a change in some receptor exposed to the pollutant. The receptors may be humans, animals, plants, and microorganisms; biological communities and ecosystems; natural and man-made materials; or the geophysical systems that determine the weather and climate. In each of these categories, effects may appear as changes in many different tissues, organs, systems, states, or processes. Effects in organisms, for instance, may include altered enzyme activity, a tumor in the lung, or changes in behavior or reproduction. Examples of changes in ecosystems include altered ecological succession, reduced species diversity, and changes in the capacity to assimilate wastes.

The approach used in this report is based not on specific pollutants or classes of environmental contaminants, but rather on categories of effects, as expressed in a variety of receptors, that might be produced by many different environmental contaminants or combinations of agents.

The effects examined include injury to human health, damage to wildlife, vegetation, and ecological systems, corrosion of materials, and changes in climate. The order in which they are discussed does not imply relative priority, and the interrelationships among effects should be recognized. Effects on wildlife or ecological processes may presage effects on human health, and the consequences to mankind of certain effects (for example, climatic change) may ultimately be far more serious than any direct threats to health posed by the pollutants involved. Each type of effect discussed in the report has, to a varying degree, social and economic consequences; determination of these is often a prerequisite for decision making.

The distribution of effects in space and time is important. Effects may be local or global; may involve a few individuals or an entire species; may be transitory or continue for centuries after the onset of environmental change; and may be reversible or cumulative and ultimately irreversible. A great many degrees can be identified within each of these dimensions.

Patterns of susceptibility are another important aspect of effects. Sensitive subsets of varying sizes within a population of organisms or subunits of an ecosystem may be at risk from exposures that are relatively harmless to the rest of the population or the system. In humans, for example, fetuses, infants, the aged, and people with certain diseases or special inherited or physiological conditions may be hypersensitive groups. Among other living organisms, differences in sensitivity among species may be pronounced.

Some environmental contaminants can have effects that are neutral or beneficial to the receptor; for example, some trace contaminants of drinking water are also essential nutrients (NRC 1974). Other contaminants, such as chemical pesticides, can have substantial socially desirable effects when used for their intended purposes, although pollution may be an inevitable result of such use. We recognize the importance of information on such beneficial effects, in that all of the consequences of a given environmental change must be evaluated by decision makers. Nevertheless, the primary thrust of environmental regulation is to protect against adverse effects, and our concern in this report must therefore be almost exclusively with these.

Causative Factors

Most research on the effects of environmental pollutants has focused on individual chemicals or physical agents (for example, noise, heat, particles). Agents have been tested singly, or at most in combinations of two or three, with other conditions controlled as part of the experimental design. In the environment, however, innumerable pollutants may be present simultaneously, intermittently, or sequentially; and factors such as temperature, light, moisture, and physiological and ecological states and processes are likely to vary a great deal. Pollutants and these other factors may interact in additive, antagonistic, or synergistic ways to produce the observed effects. It is essential that both researchers and decision makers recognize and take into account the complexity of causative processes.

Mechanisms

It is important to know not only what effects occur in response to exposure to a polluted environment, but also how they occur. For organisms and for ecosystems, information is needed on the biochemical, physiological, and pharmacological mechanisms of uptake, translocation, accumulation, storage, biochemical transformation, and removal or excretion of pollutants, and on mechanisms of toxic action and response. The need includes information on the mechanisms of interactions among agents that may occur at any stage in this sequence. Information on mechanisms can clarify cause-effect relationships, and may provide a basis for prediction, prevention, or treatment of effects.

The study of mechanisms is particularly important in cases in which environmental agents undergo transformations. Some chemical reactions in the environment and metabolic processes in many organisms can degrade and detoxify contaminants (for example, see discussion in NRC 1977). In other cases, chemical or biological processes may produce substances that are far more toxic than the original material (for instance, see Wood et al. 1968, Hall 1971). Greater understanding of these processes is an important need.

Dose-Response Relationships

Dose is the amount of a substance in the environment to which a receptor is exposed, summed over time. The magnitude and timing of exposures, of the appearance of effects, and of recovery when exposure ceases are important dose-response parameters. Acute effects occur in minutes to hours, and subacute effects in days to weeks, in response to exposures to relatively high concentrations of agents. Chronic effects, in contrast, appear only after extended periods, sometimes approaching the lifetime of the organism. Chronic effects may result from long-term, low-level exposure, or may be due to short-term, larger doses, but appear only long after exposure to causative agents has ended. Probably the greatest current needs for additional information about effects of environmental contaminants involve chronic effects (Congressional Research Service 1975).

Many environmental agents reach receptors by more than one route, and at different rates by each pathway. For example, humans may be exposed to a given toxic substance in air, drinking water, food, pharmaceuticals, and in occupational environments. It is important (though difficult) to be aware of and account for all such exposures and their contributions to total dose. Some contaminants accumulate in living tissues, making the cumulative dose over time significant even at extremely low levels of exposure, or at infrequent but high exposures.

A critical and controversial issue is whether a no-effect level or threshold of toxicity exists for a given effect of an environmental agent, and, if it does exist, whether such a level can be identified. For some biochemical and pharmacological phenomena, such as the action of analgesic drugs, the existence of a no-effect level is generally accepted; in other cases, however, such as carcinogenesis, the concept is vigorously debated (Bingham and Falk 1969; Kotin 1976; Anonymous 1976). In most testing systems, some level of exposure can be identified at which there is no detected adverse effect. This fact may be due to one or more of the following considerations: (a) sample size--effects may occur at all exposure levels, but with such a low frequency at the levels tested that their detection would be statistically significant only with a much larger population of exposed organisms; (b) measurement techniques--effects may be present, but not detected by the particular testing procedures available; (c) biological defenses--excretion, detoxification, DNA repair, or other mechanisms may enable the organism or system to cope with a low level of exposure.

The Nature of Research on Effects of Environmental Pollutants

Effects of environmental pollution, and especially chronic effects of low-level exposures, may be expressed so subtly or obscurely that they are difficult to detect and measure, and conclusive proof of causation may be all but impossible, even after damage has been done. We believe that in many cases it is not acceptable to postpone decisions until such proof is available, especially when the only truly convincing evidence would be the occurrence of, for example, serious damage to human health or a major change in climate. Action must instead be taken to prevent adverse effects, using presumptive evidence and valid scientific estimates of the nature, magnitude, and probability of the effects. For instance, the risks to human health inherent in long-term exposure to low levels of toxic substances have been estimated from studies of the effects of higher doses of the same substances on laboratory animals (e.g., Hoel 1976). Similarly, the responses of some plants and animals with well understood physiological processes to measured exposures to pollutants in the laboratory have been used to model possible responses of other organisms to the same pollutants in the ambient environment.

Reliance on indirect evidence of this sort, however, introduces additional uncertainties into the assessment of risks. The underlying biological principles that might support the validity of some such extrapolations may not yet be known. For example, there is not yet an accepted theoretical basis for a quantitative transfer of data on carcinogenicity from one species to another, e.g., from rodents to humans, although most empirical results support a qualitative extrapolation. Some sophisticated statistical techniques have been developed for estimating the magnitude of risks to humans of very low-level exposures, using data on far higher exposures in animals or humans; but in many cases the biological models that must be assumed in making such estimates have not been (and may never be) verified (Hoel et al. 1975). Similarly, there is not yet any reliable way to translate data on effects of a pollutant under controlled laboratory conditions into estimates of the effects that the same pollutant might have in intermittent exposures, in the presence of other pollutants, and with numerous other uncontrolled variables in the ambient environment. Data on the effects of a given pollutant can sometimes be used to predict effects of chemically similar compounds; however, knowledge of the relationship between structure and biological activity is still too incomplete to make this approach widely applicable, and some erroneous conclusions might be reached if structure-activity correlations were relied upon as the chief basis for assessments (Van Duuren et al. 1972).

Some circumstances of environmental exposures or characteristics of the causative process make it quite difficult to design studies that reliably link effects to their causes. In many cases, an entire population of humans, plants, or animals is exposed to a potentially toxic substance, making it impossible to use a comparable unexposed segment of the population as a control group. Instead, the population must be examined over time, or comparisons made with groups elsewhere that have not been exposed to the agent. Introducing such geographic or temporal differences into the comparison introduces additional variables (for instance, other environmental factors and changes in the population itself) for which there is often no control. In other cases, the appearance of effects may be long delayed. For example, some developmental abnormalities may not be expressed until relatively late in the life of an organism (Coyle et al. 1976, Spyker 1976); some kinds of human cancers have latent periods of 20 years or more (Bingham et al. 1976); and some mutations, because of their recessive nature, may be observed only in later generations.

The Limits of Research

There are many questions about the effects of environmental pollutants that science can pose, but cannot answer. Some questions, such as the extrapolation of estimates of risks of cancer discussed above, may be inherently unanswerable, or may need to await the development of additional fundamental knowledge. Others may be theoretically answerable, but impractical to address because of the cost, time, and effort required. For example, it has been estimated that a study to determine experimentally whether genetic effects of radiation in mice are linearly related to doses at exposures that would increase the natural incidence of mutations by 0.5 percent, with statistically significant results at a confidence level of 95 percent, would require eight billion mice (Weinberg 1975).

In many cases, therefore, proof of effects of environmental changes may not be attainable through experimental research. Decisions will of necessity have to be based on extrapolation and statistical assessments of risk. In the face of continuing and unresolvable major gaps in fundamental knowledge, research on some problems may offer little promise of producing meaningful answers. Such questions must be identified, so that necessary social decisions may be made on the strength of whatever _is_ known, and other (nonscientific) considerations (NRC 1975).

Research on the Economic Impacts of Pollution

Like our knowledge of the biological measurement of effects, our current ability to assign economic values to effects is characterized by significant gaps in available information and limitations in existing theory. Economics can provide a mechanism for comparing the values of some otherwise noncomparable entities, through the use of market prices and substitute measures. However, subjective value judgments must be made if dollar values are to be assigned to such outcomes as risk to human life or the survival of bald eagles. Determination of the full social impact of a change in the environment requires consideration of costs in labor, energy, loss of amenities, depletion or destruction of natural resources, and other indirect but significant consequences.

Environmental economists can use available models to infer values for some effects and can produce estimates, however crude, of the costs of pollution. The reliability of such estimates is limited by scientific uncertainties in the prediction of effects, as well as by the difficulty of obtaining data in the form required by economic models. Improved economic measurements of effects depend, therefore, not only on advances in economic research and in data collection, but also on improvements in knowledge of the nature of adverse effects of exposures to environmental pollutants and of the likelihood of their occurrence.

The Multidisciplinary Approach

The study of the effects of environmental pollutants is a complex undertaking requiring the coordinated participation of many scientific and technical disciplines. For example, evaluation of the role of toxic environmental agents in diseases of humans or animals depends on contributions from epidemiology, pathology, pharmacology, toxicology, clinical medicine, and other specialties. Research programs that facilitate the interaction of workers in several disciplines on interrelated aspects of a problem, beginning with the posing of research questions, have shown considerable promise for producing information of great value.

Quality of Research Results

In order to make the information that is available to decision makers current, reliable, and credible, research must be based on sound scientific principles and methods, must ask relevant and critical questions, and should draw as much as possible upon the best scientific talent of the country. The objectivity of research is also very important. Unfortunately, many research investigations are designed to prove a point or to gather information to support only one side of a controversial issue. Such work is often conducted by interested parties in adversary situations, and its credibility is consequently suspect. Despite obvious potential biases, however, the quality of some such research may be high, and the results extremely valuable. To increase public confidence in the quality and integrity of the research used for decision making, peer review and similar safeguards against potential bias should be used extensively in gathering and evaluating information (NRC 1975).

PRINCIPLES FOR ESTABLISHING RESEARCH PRIORITIES

Research resources are finite and must therefore be applied to areas that offer substantial opportunities to produce results of the greatest value for decision making. This need applies particularly to the limited research budgets of regulatory agencies such as EPA.

Because of the subjective component inevitable in any ranking of the relative importance of research topics, the criteria used should be stated explicitly. The considerations that follow are ones we have used; others might use them, with their own emphasis, for setting research priorities. Some of these criteria might be given greater weight than others, but they are not listed specifically in order of relative importance.

The panel agreed unanimously that research resources should be applied to the study of important effects, but we could find no completely objective definition of importance. The importance of effects may be proportional to all or some of the following variables:

- the magnitude of impacts experienced by individual human beings, animals, plants, other organisms, inanimate materials, or ecosystems affected by changes in the environment;

- the numbers of people, animals, plants, and so forth, so affected;

- the amounts of energy, food, natural resources, ecological processes, or other amenities that may be made unavailable (or available) because of changed environmental quality;

- the economic value of benefits or damages, or the costs or benefits to individuals or society of living with the effects;

- the geographical extent of effects;

- the time over which effects may be felt;

- whether effects are reversible or irreversible.

Additional criteria beyond the importance of effects include:

- the social and economic consequences of regulatory action (or inaction);

- the immediacy of either the consequences of environmental change or the need to begin research;

- the extent to which identified information gaps influence regulatory decisions (for example, see North and Merkhofer 1975).

- the magnitude of information gaps; (this intuitive criterion may be misleading, if lack of information is due to a dearth of basic principles to guide research or if the topic fails to meet the other criteria stated here;)

- the existence of adequate basic knowledge, methodologies, and theoretical constructs to make research feasible;

- the resolvability of questions with a practical amount of resources and time;

- the availability of capable trained personnel, appropriate facilities, a source of funding, a favorable research environment, and so forth;

- the breadth of applicability of results.

Popular interest in certain problems and resulting political pressures may drive research in the direction of seeking information on specific effects, regardless of whether other criteria are met. Conversely, research that meets all previous criteria may not seem justified to an appropriations committee, a bureaucracy, or the Office of Management and Budget. While political decision makers have a legitimate role to play in the definition of priorities, continual shifts of attention to new "pollutants of the month" can create an unstable atmosphere that makes it difficult to maintain the continuity of research.

The criteria spelled out here are intended to be applied to research programs that are undertaken by, or in support of, agencies responsible for protection of the environment. Investigation of fundamental principles in all areas of biological and environmental sciences is a necessity as well; in fact, it is difficult to distinguish some of the needs identified here from "basic" research. Many kinds of work on effects of pollutants will depend on the continued support of basic, nontargeted research if they are to be feasible.

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CHAPTER 2

GENERAL RECOMMENDATIONS

Each of the following chapters presents recommendations for research on the effects of a polluted environment on a specific category of receptors, such as man, materials, or ecosystems. The recommendations in this chapter are more general and apply to research on many or all of the topics discussed in later chapters. They are concerned with institutional arrangements and research strategies that will increase the usefulness of research on effects for making environmental protection decisions, facilitate the conduct of some important kinds of research, and strengthen the process through which research findings are used in regulatory decision making.

RESEARCH STRATEGIES FOR SOME RELATIVELY INTRACTABLE SCIENTIFIC PROBLEMS

> The primary responsibility for research on fundamental but scientifically difficult problems related to effects of environmental changes should be assigned to agencies or institutions other than those with immediate regulatory missions.

Many of the most critical scientific issues related to decision making on environmental problems probably cannot be resolved by research efforts within the next several years. Some fundamental questions about effects of environmental changes that fall into this category include:

- identifying effects of pollution on climate;
- identifying no-effect levels in plants, animals, and humans;
- predicting changes in whole ecosystems;

- extrapolating from results under controlled laboratory conditions to ambient conditions; and

- obtaining quantitative measures of interactions among multiple contaminants.

Issues of this kind cannot now be resolved, for reasons that may include the following:

- they are intrinsically so complex that scientific methods for solving them have not yet been developed;

- they involve effects that may be widely separated from the initial cause or receptor;

- they involve effects that may be qualitatively no different from those produced by natural processes;

- they involve effects that may develop over a long time scale; and

- they are concerned with the statistics of rare events.

These characteristics often make existing methodologies inadequate for testing assumptions, or make experimental investigations very difficult and expensive. Despite such obstacles, efforts to resolve these questions should continue to have high priority. Some of the effects involved, such as climatic change or long-term perturbations of ecosystems, may be irreversible; and their consequences, such as changes in world agricultural productivity or a decline in the yields of ocean fisheries, might render many current environmental concerns trivial in comparison. Yet, because of the continuing need for information to support a shifting array of more immediate decisions, and because the time needed to produce answers to most of these currently intractable problems generally cannot be estimated with any confidence, regulatory agencies such as EPA have tended to assign such investigations low priority in the allocation of research funds.

Research on these fundamental problems is needed to reduce uncertainties for future environmental decisions, and attempts to resolve them ought not to be set aside in favor of the more immediate responsibilities of regulatory agencies. Long-term funding should be made available, and the work ought to be performed under arrangements that make continuity of effort possible. Examples of agencies that might appropriately support research in these areas are the

National Institutes of Health (NIH) or the Department of the Interior (DoI) for some of the biological research, and the National Oceanographic and Atmospheric Administration (NOAA) or the National Science Foundation (NSF) for problems related to climatic change. The global nature of some problems makes an international approach to both research and policy making very important, although such an approach is seldom easy to achieve.

IMPORTANCE OF EFFECTS ON NONHUMAN ORGANISMS

Research priorities should reflect not only a concern for direct impacts on human welfare but also the inescapable dependence of humanity on critical ecological processes.

The National Environment Policy Act calls for a balanced commitment to the protection and enhancement of both human health and ecosystems. However, because of the relative ease of recognizing direct threats to human health and the great social weight attached to them, policy decisions are most heavily influenced by evidence of hazards to man, and a large proportion of research has been directed toward assessment of such impacts. Research on ecological effects is usually justified in terms of its value to humans, and work has concentrated on effects on organisms that have important economic or cultural significance (for instance, agricultural crops or vertebrate wildlife species).

There is no point to disputing society's right to assign high value to effects on humans and other organisms perceived as important to humans. We believe, however, that the well-being and possibly the survival of most vertebrates, including man, depend on the continued functioning of basic processes of ecosystems, such as the cycling of elements or the conversion of energy into biomass. From an ecological perspective, the greatest threats to the life-support system would result from harm to those organisms that play the largest roles in the flows of energy and materials through ecosystems, on scales from local to global. The vast proportion of each of these flows is processed in most cases by green plants and algae and by decomposers such as bacteria and fungi (American Association for the Advancement of Science 1967). Vertebrates high in food chains account for only a tiny fraction of energy and nutrient flows, and effects on these organisms are therefore less critical to ecosystem functions.

Current research priorities do not adequately account for the relative long-term ecological significance of potential effects on the structure and functions of ecosystems. Realignment of these priorities is needed, but is unlikely to occur until recognition of our dependence on relatively subtle and obscure complex processes is incorporated in institutional definitions of what it is in the environment that needs protection.

ECONOMIC MEASUREMENT OF EFFECTS

A sustained and continuing effort should be made to develop estimates of the costs of environmental pollution that are properly grounded in economic theory and make the best use of existing data.

Regulatory decision makers need estimates of the costs of environmental pollution that are properly grounded in economic theory and are based on the most reliable and up-to-date scientific evidence. Although this need has been clear for some time (see, for instance, NRC 1975), the current level of effort to provide sound economic analysis for environmental decision making is, in our judgment, still seriously inadequate. The need for improved economic analysis in EPA is also examined in the report of the Panel on Sources and Control Techniques of the Environmental Research Assessment Committee, (NRC 1977a), and in the report of ERAC itself (NRC 1977b). Emphasis in these reports is on analysis of the full range of costs and benefits of different control techniques and regulatory strategies. Our concern here is more particularly with research to estimate the economic value of effects of a polluted environment, or the costs of pollution.

The economic analysis of the costs of pollution requires techniques for extending the dollar or monetary price measure to things that are valued by individuals but that do not pass through markets, in order to provide a basis for comparing the benefits and costs of environmental changes. Not all environmental effects can be captured by these dollar measures; only those that directly affect the human activities of consumption and production, broadly defined, can be estimated. For example, those ecosystem changes that do not affect human activity in some way cannot be measured in dollars. This does not mean that these changes have no value or cost, but rather that we have reached the limits of economic analysis.

Regulatory decision makers at present lack reliable estimates of the costs of environmental changes in monetary terms. This lack is only partially attributable to limitations in economic theory. Economic theory and models are available that provide a basis for defining and measuring many of the significant types of economic effects of environmental stresses. Some major exceptions are morbidity, mortality, and aesthetic impacts.

> Close coordination between the social and natural sciences should be pursued in the design and conduct of research, to ensure that data that are also meaningful to economists are gathered in scientific research.

There are three important reasons for the shortage of reliable estimates of economic costs of pollution. First, some of the studies leading to monetary estimates of such costs have lacked an adequate theoretical justification for the interpretations of the data used. Early estimates of air pollution damages derived from studies of property values are a case in point (Freeman 1974; Polinsky and Shavell 1975, 1976). Second, in some other cases, the use of existing economic models would require major efforts to gather data that are not presently collected on a regular basis. If research were designed and carried out to collect primary (scientific) data appropriate for economic analysis, improved estimates of pollution damages could be obtained for effects such as impacts on water-based recreation, deterioration of materials, and damage to agricultural crops. The major constraint on improved data gathering is cost, but expanded efforts to do well-defined case studies in specific geographical regions seem to be justified. Such studies might provide a basis for extrapolating to national estimates of damages, which would be very important for many regulatory decisions (Freeman 1976).

Probably the most serious barrier to better estimates of the economic costs of pollution is inadequate knowledge of the underlying relationships between environmental conditions and their effects on humans, other organisms, materials, and climate. It is essential, in our view, for cooperative interaction between economists and natural scientists to begin *early* in research on such effects--as early as in the posing of research questions. Such coordination would offer the most promise of casting data-gathering efforts in a frame of reference that would support later economic analysis.

ESTABLISHMENT OF QUICK-RESPONSE STUDY TEAMS

The capability should be established for prompt and thorough investigation of outbreaks of illness or other effects suspected to be associated with sudden changes in pollutant levels in the ambient environment.

Some changes in environmental quality result from pollution incidents with sharply defined onset times and spatial boundaries. Such episodes often lead to clusters of cases of adverse effects, which present opportunities to gather important information on the effects or to identify the causes (which may be unknown). Examples of such incidents include the accidental introduction of polybrominated biphenyls into cattle feed in Michigan (Isleib and Whitehead 1975); the intense pollution of the area around Hopewell, Virginia with the pesticide Kepone; and the severe episodes of urban air pollution produced by unusually stable weather conditions in different parts of the country on several occasions in recent years (Storer 1975). In such cases, the usefulness of what can be learned from an incident often depends on how quickly needs for specific data can be identified and the necessary measurements made. Evidence gathered long after the event is likely to be more difficult to obtain, less complete, and of less obvious value for demonstrating cause-effect relationships.

Public health departments have developed some very effective capabilities for rapid study of outbreaks of infectious diseases and food poisoning. Broader applications of the same techniques to environmental problems are being developed, but are not yet available for routine use. The essential ingredients of such a program include:

- developing a sequence of procedures to be followed, ranked in order of the immediacy of needs for certain data;

- assembling and training multidisciplinary teams to conduct field investigations, and providing the flexibility to mobilize the teams on very short notice; and

- providing strong laboratory support.

Investigations of pollution incidents ought to include measurements of the distribution and concentration of contaminants in the environment (if the identity of these is known) and examination of a wide range of receptors for potential effects. Depending on the nature of the episode, humans, domestic animals, wildlife, plants, and critical ecological organisms (e.g., soil bacteria) may need to be studied to determine what effects, if any, are occurring. Where effects are present but the cause is unknown, their manifestations must be carefully recorded, and samples of tissues of affected organisms and of the air, soil, water, or food to which they are exposed must be obtained and analyzed. The complexity of the investigations that need to be conducted requires that the team assembled include diverse specialists, such as a clinical physician, an epidemiologist, a veterinarian, a plant pathologist, an analytical chemist, and so on.

EPA and other agencies conducted some studies of this sort in the Hopewell (Kepone) incident, and some similar investigations, primarily involving outbreaks of suspected pesticide poisoning, have been conducted by the Epidemic Intelligence Service of the Center for Disease Control (CDC) of the U.S. Public Health Service. The capabilities that exist now should be strengthened and called upon in response to future incidents. EPA is in closest touch with reports of most pollution-related outbreaks of disease and other damage, and is best equipped to measure and assess a broad range of environmental conditions and consequences. CDC has the best expertise and capability to perform rapid medical evaluations in the field. The most effective investigations of future incidents will require active and coordinated participation by both CDC and EPA, as well as other appropriate agencies. To date, the desirable level of interaction has rarely been achieved. We recommend strongly that more formal institutional arrangements be developed to provide for joint rapid-response efforts by EPA and CDC to study the effects of future pollution incidents.

PUBLICATION OF THE BASIS FOR SCIENTIFIC JUDGMENTS

EPA should adopt policies to ensure that the basis for its scientific judgments regarding the quality of evidence of adverse effects becomes part of the formal public record for each regulatory decision.

Scientific evidence of actual or potential adverse effects on man or on the environment carries great weight in EPA's regulatory decisions. For example, evidence of possible carcinogenicity was critical in the decision to ban DDT.[1] To make equitable decisions, the Administrator of EPA must have scientific information that is thorough, balanced, accurate, and based on a rigorous and objective assessment of the quality of available evidence and the remaining scientific uncertainties. In the introduction to this report, we recommend extensive peer review of scientific data as one method of ensuring the quality of the scientific input to EPA's decisions. We feel that formal policies should be adopted that would foster such peer review. Such measures are particularly needed where critical evidence has been developed by a party in an adversary context, either by EPA itself or by industry or other interested parties.

Regulatory decisions clearly are based on social considerations, as well as on scientific evidence. Nevertheless, the process would be improved by more detailed and explicit statements of the scientific judgments that support such decisions. For example, where contradictory evidence can be cited on a scientific point, it is important to know what the scientific basis is for accepting some studies as valid, and rejecting others. Decision documents have tended to cite only the evidence that supports the decision. In some cases, it has not been possible for an outside observer to know what evidence EPA has reviewed, whether uncited (conflicting) research data were examined, or why only certain studies were considered valid. This problem is compounded when information is used that has never been published in the open literature, as is sometimes the case with both EPA and industry data.

The problems created for EPA and for others by the lack of more explicit statements of scientific judgments can be seen clearly in the records of judicial review of some of EPA's decisions. For example, the Federal Court of Appeals for the 3rd Circuit in 1975 remanded effluent limitations guidelines for the iron and steel industry to EPA because "other information available to the agency", cited in the Administrator's decision document, was not specified, and consequently meaningful judicial review of the Administrator's decision was impossible.[2] Similarly, the Federal Court of Appeals for the 4th Circuit in 1976 reversed EPA's effluent limitations for the tanning industry, and castigated the agency: "The record, however, implies that these conclusions are the product of guesswork, and not of reasoned decision-making... No scientific data or other demonstrative evidence was given to substantiate these final effluent levels".[3] Such conclusions by the courts are, unfortunately, not isolated; many of those EPA decisions that have been reversed have been faulted for improper or inadequately supported use of scientific information.[4]

EPA takes pride (with some justification) in being an "open" agency; that is, the public has extensive access to the decision process and opportunity to participate in it. Nevertheless, EPA's decisions continue to be attacked, often with assertions that the Agency considered only selected data, or erred in its assessment of the quality of available research. A more complete and explicit statement of the scientific basis for decisions would have salubrious effects on public awareness of and confidence in the scientific rigor of EPA's assessment process, and ultimately, perhaps, would improve the process itself.

We recommend, therefore, that EPA establish a policy along the following lines: before a regulatory action is taken, information should be published that would list all studies deemed relevant to the proposed action, identify the scientists who performed the research and their sources of funding, and summarize the results of each study. The studies that are to be relied upon to support proposed regulations should be identified and the reasons for rejecting the conclusions of studies that came to materially different results should be stated.

We believe that a policy of this sort should be followed at all stages of the sequential processes of decision making, adjudicatory hearings, and judicial review of EPA actions.

NOTES

1 Committee on Principles of Decision Making for Regulating Chemicals in the Environment, Environmental Studies Board and Committee on Toxicology, National Research Council. Unpublished case studies on the decision to ban the use of DDT include papers by Angus A. MacIntyre, Doctoral Candidate, University of California at Davis; Max Sobelman, Vice President of Operations, Montrose Chemical Corporation of California; and Charles F. Wurster, Associate Professor, State University of New York at Stony Brook. These papers are available in limited numbers upon request from the Environmental Studies Board of the National Research Council, 2101 Constitution Avenue, Washington, D.C. 20418.

2 American Iron and Steel Institute v. EPA, 526 F.2d. 1027 (3rd Cir. 1975). See page 1063.

3 Tanners' Council v. Train, 8 ERC 1881 (4th Cir. 1976). See page 1884.

4 For other examples of cases in which the courts held that EPA had not provided an adequate scientific explanation for its decisions, see Hooker Chemicals and Plastics Corp. v. Train, 537 F.2d. 620 (2nd Cir. 1976), [pages 636-637]; American Meat Institute v. EPA, F.2d. 442 (7th Cir. 1975) [pages 459 and 466]; International Harvester Company v. Ruckelshaus, 478 F.2d. 615 (D.C. Cir. 1973), [page 648]; Appalachian Power Co. v. EPA, 477 F.2d. 495 (4th Cir. 1973), [p. 507]; Environmental Defense Fund v. EPA, 465 F.2d. 528 (D.C. Cir. 1972), [page 541]; Portland Cement Assn. v. Ruckelshaus, 486 F.2d. 375 (D.C. Cir. 1973), cert. den. 417 U.S. 921; FMC Corp. v. Train, 8 ERC 1731 (4th Cir. 1976), [pages 1737 and 1739]; Ethyl Corp v. EPA, 7 ERC 1353 (D.C. Cir. 1975), and 8 ERC 1785 (1976), Cert. den. 8 ERC 2200; Kennecott Copper v. EPA, 3 ERC 1682 (D.C. Cir. 1972); Texas v. EPA, 6 ERC 1897 (5th Cir. 1974); South Terminal Corporation v. EPA, 6 ERC 2025 (1st Cir. 1974); Appalachian Power v. Train, 9 ERC 1033 (4th Cir. 1976), [page 1052]; CPC International v. Train, 9 ERC 1301 (8th Cir. 1976), [pages 1309-10]; Dupont de Nemours and Co. v. Train, 541 F.2d. 1018 (4th Cir. 1976) cert. granted 96 S. Ct. 3165, [pages 1033, 1035, 1036, 1039].

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CHAPTER 3

EFFECTS ON WEATHER AND CLIMATE

In the face of continued growth of the world's population and enduring pressure on the planet's food-producing capacities, large natural fluctuations of weather and climate increasingly threaten human survival and well-being, particularly in less advanced countries. Projected human-induced modifications of the earth's surface and the atmosphere, and some changes that have already occurred, could cause effects on climate of a magnitude, spatial extent, and duration comparable to some natural fluctuations. Given the current state of knowledge of the mechanisms of climate change, it is not possible to distinguish between natural and human-induced changes, at least on global and long-term scales. Artificial change, moreover, may increase the variability of climate, which is a crucial factor in the supply of and demand for food.

Available methods of predicting the effects on weather and climate of man-made environmental changes are inadequate, and may never be perfected; it is generally acknowledged that, at best, decades of work will be required to improve them significantly. We cannot now predict what climatic changes will occur; we can only indicate what might occur. The possibilities are disturbing. The potential magnitude of the consequences for humanity of possible climatic changes makes this a subject deserving the concerted attention of both the research community and those charged with protection of the environment.

The situation is complicated by the fact that many of the possible man-made changes in climate that have been considered are global in character. Pollutant emission or other environmental modification in one country may affect the whole world; a nation may not be able to protect itself by its own regulations. Furthermore, most of the climatic changes that have been envisioned are neither universally deleterious nor universally beneficial to humanity. Pressure for regulation is unlikely to be equally strong in all regions or nations, and proposals for regulation by some sections of the world community might be actively resisted by others.

IMPACTS OF CARBON DIOXIDE EMISSIONS

Research should be undertaken to set upper and lower bounds to possible global temperature changes that might follow a given increase of the carbon dioxide content of the atmosphere.

The most important climatic question facing the world today is probably the magnitude of changes that may result from an increase in the CO_2 content of the atmosphere caused by burning fossil fuels (NRC 1977). Some authoritative projections of fuel combustion, combined with existing geochemical and climatic theories, indicate that a critical point in regard to CO_2 may be reached within the next 20 to 50 years. National and international policies defining acceptable limits on the use of fossil fuels must be established within that span. Such definitions must be based on reliable estimates of the magnitude of changes that might occur in global temperatures and consequent changes in climate, polar ice cap masses, and sea level.

Methods involving further development of comprehensive general circulation models seem most unlikely to produce such estimates within the time available. EPA, the Energy Research and Development Administration (ERDA), and other agencies responsible for energy and environmental policy should therefore support or encourage studies using less time-consuming methods, recognizing the risk of failure inherent in this approach. Models available today should be used, with some necessarily arbitrary simplifying assumptions, to estimate the consequences of projected emissions of CO_2. The results will not be precise predictions but should at least establish some reliable upper and lower boundaries for possible effects. This information is a major requirement for setting sound policies governing combustion of fossil fuels.

This problem must be viewed in a larger perspective, since fossil fuel combustion is bound to the need to feed humanity and keep it warm over the next two centuries. Research should therefore also be undertaken by some federal agency to examine the international, very long-term aspects of energy conversion and environmental change.

EFFECTS ON THE OZONE LAYER

Research should continue to be pursued to determine the potential biological and climatic consequences of modification of the ozone layer.

Atmospheric scientists have identified a number of threats to the stratospheric ozone layer in recent years, including fluorocarbon aerosol propellants and refrigerants (Molina and Rowland 1974, NRC 1976), high-altitude emissions of nitrogen oxides in aircraft exhaust (Johnston 1971, NRC 1975a), and nitrous oxide produced from fertilizers by soil microorganisms (Crutzen 1974, Council for Agricultural Science and Technology 1976). The effect under study is a reduction of the concentration of ozone in the stratosphere, slowly reversible, with a time constant of decades.

Understanding the modification of the ozone layer appears to be a much more tractable question than that of general climatic change. The meteorology, physics, and chemistry of the ozone layer are being vigorously studied, and it seems likely that a consensus on acceptable quantitative estimates of the reduction factors, accurate to within a factor of two or three, will be reached within a very few years. Nevertheless, much more information is needed on the biological and climatic consequences of increased ultraviolet irradiation and of related changes in radiation balances. These problems have been reviewed by the National Research Council (1975a). EPA has recently been assigned a lead agency role in federal research on these subjects. The social and political consequences of possible regulatory actions also need to be studied, particularly if fertilizer use should be affected, and the international aspects of control measures must be taken into account. Although the causes of the perturbations could originate in any or all countries, the deleterious effects, at least those directly concerned with human health, are likely to be limited in their occurrence to a relatively small geographic area (see NRC 1975a, Appendix C: 177-221).

EFFECTS OF SULFUR EMISSIONS

Research should be directed toward solutions to the physical problems of weather and climate modification connected with sulfur and other emissions from combustion of coal.

Problems of weather and climate modification connected with sulfur emissions include local to regional reductions of solar radiation at the surface due to aerosols, regional increases in acidity of precipitation, and minor modification of local rainfall patterns (Metromex Investigators 1976). These problems may at times cross international boundaries (Bolin and Persson 1975).

Pollutant sulfur enters the atmosphere mainly by combustion of coal and heavy oil. It is emitted in the form of gaseous sulfur dioxide, but a proportion is quickly converted to sulfuric acid in the form of small particles. Sulfur emission is normally accompanied by emission of solid particles, and the emission control technology currently in use does not remove the finest of these particles. The solid particles are particularly efficient in reducing the solar radiation reaching the ground, since they absorb as well as scatter it. It seems very likely that control technologies will be developed to minimize sulfur emission and to reduce even further the emission of particles, but such technologies will be expensive. The greatest information need in this area is probably on the biological and economic consequences of acidified precipitation and weather modification by sulfur and particle emission, which would need to be balanced against the biological and social impacts of using more expensive low-sulfur fuels.

INSTITUTIONAL ARRANGEMENTS

Given the present state of knowledge, research on possible perturbations of weather and climate by environmental modifications cannot, in general, be separated from other aspects of climate research. In the United States, such studies are being carried out by the Departments of Defense, Commerce (NOAA), Interior (Bureau of Reclamation), and Transportation (Federal Aviation Administration [FAA]), and by the National Aeronautic and Space Administration (NASA), ERDA, EPA, and NSF. An Interagency Committee on Atmospheric Science exists under the Federal Council for Science and Technology, but it seems unlikely that the optimal administrative arrangement has been achieved. A very large proportion of the funding is devoted to in-house projects and to work in the large national laboratories. Most of the NSF budget for climate dynamics and the atmospheric sciences goes to the maintenance of the National Center for Atmospheric Research, and to the pursuit of major international programs that call for long-term spending commitments. There is little flexibility that would allow support of unsolicited innovative research in universities and independent research institutes.

A recent study, Understanding Climate Change (NRC 1975b), estimated the expenditure on climate-related research in 1974 "excluding essentially operational or service related activities" as $18 million per year. It called for a graduated increase to $67 million by 1980--a call that has not yet been heeded. That particular NRC study was concerned mainly with the problem of global climate prediction on all time scales and had little to say about the usefulness of limited investigations of specific problems, as recommended above.

The question of stratospheric modification and its consequences was examined extensively in the Climatic Impact Assessment Program (CIAP) of the Department of Transportation. This work is being continued in a more selective way in the Upper Atmospheric Programs funded through FAA and NASA. Within this program EPA has been designated the lead agency for research on biological and climatic effects. At this early stage the administrative arrangements and the scope of the program appear satisfactory. The CIAP studies emphasized the international nature of problems; continuation of this aspect is essential if future regulatory action is contemplated.

Within EPA, research on sulfur emissions at present has high priority at all levels, from techniques of fuel desulfurization to field observations of transport and transformation of the emissions and consequent weather modification. A large, well-conducted field experiment with funding from EPA, NSF and state sources is continuing in the St. Louis area, and ERDA is planning a major survey of the fate and effects of sulfur emissions. If the dangers of overemphasis on data collection at the expense of critical review and analysis by independent scientists can be avoided, the level of effort seems commensurate with the importance of the problem, but frequent review is vital.

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CHAPTER 4

EFFECTS ON HUMAN HEALTH

Evidence has long been available that certain polluted occupational or ambient environments can cause or contribute to both acute and chronic degenerative diseases. More recently, as knowledge of the importance of environmental contaminants in health and disease has increased, it has also become apparent that exposures to toxic agents in the environment may produce other effects, such as cancer, mutations, birth defects, and behavioral abnormalities (McCullough 1975).

Government agencies charged with protecting health and the environment from effects of toxic substances face a substantial challenge in identifying the risks involved in any given case. If the purpose of taking action is to prevent harm, decisions must be based on presumptive evidence, before disease has been become overt. The kinds of evidence needed to establish a basis for action include:

- identification of causative agents in the environment;
- demonstration of adverse effects, such as morbidity or mortality, in exposed populations;
- production of toxic effects in laboratory animals; and,
- reduction or elimination of harm when exposure is reduced or ceases, or when the population is protected.

Such sequential chains of evidence have been established in some cases of occupational exposures (for example, to beta naphthylamine and nickel [International Agency for Research on Cancer 1971-1975]). The same general kinds of evidence are needed to evaluate the effects of contaminants in the ambient environment.

Several research approaches are needed to establish or refute causal relationships between environmental pollutants and effects on health. Epidemiological studies can show patterns of disease or death that, when correlated with patterns of exposure to environmental contaminants and other factors, suggest possible causal relationships. Toxicological research can demonstrate qualitative similarities between effects observed in humans and effects in laboratory animals. Additional important information can be gained from investigations of the biochemical and physiological mechanisms of action of environmental agents and of the functional and structural changes a chemical or metabolite may produce in tissues and organs. No one research approach can, by itself, provide adequate information on the potential health effects of a chemical in the environment; congruent results are needed from both human experience and laboratory studies. Ideally, cause-effect relationships suggested by epidemiological associations could be confirmed by both toxicological testing and the elucidation of metabolic mechanisms. In each specific case, research needs will vary according to the nature of the effects involved, the kinds of data already available, and the potential success of particular investigatory approaches.

EPIDEMIOLOGY

Epidemiology is the study of the determinants of the incidence and distribution of disease in populations. Observation of associations between patterns of disease and patterns of exposure to environmental contaminants can be a powerful tool for generating hypotheses about cause-effect relationships. When supported by clinical or experimental findings of similar effects, epidemiological studies can provide the most convincing evidence that an environmental agent has had an impact on the health of a population.

There are, however, limits to what can be learned from epidemiology. Spurious correlations may be found. The quality of available data is often poor; data on mortality are not always recorded in consistent forms, and there are gaps and inconsistencies in data on morbidity. Information on exposures to potential causative agents is generally inadequate. Few communities monitor the air or water for more than a small number of substances, at a few locations. Information on what was in the environment 20 or 30 years ago (the latent period for some diseases) is often impossible to obtain; furthermore, people move from place to place. There is no way to know exactly what mixture of contaminants and other factors (such as cigarette smoking and diet) a person or a population encountered before its disease status was observed.

Nevertheless, there is much rich ground to be broken in environmental epidemiology. A great deal of information exists on the incidence of various diseases; some of it is in useful form, (such as the cancer atlas compiled by the National Cancer Institute (NCI) (Levin et al. 1976), although much has never been examined systematically. When information on ambient environmental exposures to an agent is unavailable, it is often possible to study a small population with a history of high exposure, for instance in an occupational environment. The study of such populations can generate valuable qualitative data to guide further investigations.

The Data Base

As a prerequisite to epidemiological research, improved systems are needed for collecting data on mortality and morbidity and for correlating them with information on exposures to possible causative factors.

The need for a national registry of death certificates is particularly critical, so that researchers can determine which members of a population under study have died and where their death certificates are filed. Mobility of individuals makes a national index essential. The importance of such a national death registry has long been recognized (U.S. Department of Health Education and Welfare 1970), and methods for its implementation are currently being reviewed by the Department of HEW. Nevertheless, this is such a fundamental need for epidemiological research that we believe it cannot be overemphasized.

National registries for major diseases are also needed. Progress is being made toward the development of a national cancer registry, an effort that should continue. There are no comparable data systems for many other important diseases that may have environmental determinants, such as heart disease or diabetes.

Collection of mortality and morbidity data would be best coordinated through the National Center for Health Statistics. A variety of methods should be explored for obtaining data, such as census surveys or registration procedures for national health insurance programs (if and when the latter are implemented). A simple, effective procedure is needed to correlate data on occupational history, smoking habits, and other personal or environmental information about individuals, with death and disease registries. One available method is to include Social Security numbers on death certificates and records; this approach is not fully satisfactory, however, and more effective ways to provide systematic linkage of data need to be developed.

The most important limitation on the collection and use of health statistics in epidemiological research is not a technical or economic consideration, but rather the ethical problem of invasion of privacy. We believe that, particularly in cases of individuals who have died, the benefit to society of research on environmental factors that may have contributed to those deaths outweighs any possible harm to individuals that might result from allowing investigators access to personal medical records. Nevertheless, privacy is a social issue of great concern today, and Congress has moved toward increased protection of the individual. We feel that the impacts of existing or proposed privacy legislation on epidemiological research should be carefully examined, so that balanced choices can be made in cases where these values may be in conflict.

Institutional Arrangements

Epidemiological studies on environmentally-caused diseases will be most effectively conducted through a coordinated effort involving several federal agencies, plus university and other research teams supported by grants and contracts. It is important that the complementary strengths of assorted agencies be used in mutually reinforcing rather than competitive ways. EPA is best equipped to determine what agents occur or may occur in the environment, and has a strong program in some areas of environmental toxicology. On the other hand, the strongest capabilities for determining the health status of populations and studying the causative processes of diseases are found in several of the National Institutes of Health, such as NCI, and in such agencies as the National Institute of Occupational Safety and Health (NIOSH). EPA should continue to draw on the expertise of such agencies, rather than establish parallel capabilities within its own research operations. In the past, EPA's needs for epidemiological support have not always been satisfied. More effective cooperative programs, which may need to be structured to fit the problem being studied, must be devised to carry out the major epidemiological studies needed in the future. Some suggestions for such programs are included in the sections that follow.

Cancer

A selective surveillance program should be established to examine the incidence of cancer in communities of varying characteristics that have comprehensive environmental monitoring programs.

The statement has been widely repeated that 70 percent or more of all cancers are caused by environmental factors (including those in foods, drugs, cigarette smoke, and occupational settings), but this conclusion is not yet supported by adequate evidence on the specific agents and mechanisms responsible. Some very clear temporal and geographic patterns in the occurrence of cancers have established a case for environmental origins; however, dose-response relationships in humans have not been described for most carcinogens. It is still not known, therefore, whether current low-level exposures to asbestos fibers or chloroform in water supplies, cigarette smoke in closed rooms, arsenic and other trace metals in ambient air, or many other substances known to be carcinogenic at high exposures pose significant risks to human populations. The possibility that a no-effect level may exist for each potential carcinogen in the environment has not been resolved, and the combined impact on health of simultaneous exposures to many such agents at very low levels is unknown.

The uncertainties inherent in translating experimental data on carcinogens into environmental standards create a critical need for epidemiological research on cancer. Attention should be concentrated first on those areas for which the most complete environmental monitoring data are available. Data on the geographic distribution and incidence of many forms of cancer are being collected and analyzed by the NCI (Levin et al. 1976); expansion of tumor registries for the study of environmental cancers should build upon this existing base. For the study of environmental carcinogenesis to be effective, a stronger effort must be made to coordinate the complementary capabilities of EPA, NCI, the National Institute for Environmental Health Sciences (NIEHS), NIOSH, and other agencies. Because the information most urgently needed in this area today is on the identities and levels of potentially carcinogenic substances in the environment, EPA should be given an important role in the program. The major biomedical components of the research, including collection and analysis of cancer mortality and morbidity data, should reside in NCI, NIEHS, or other appropriate specialized research agencies.

A new five-year, five-million-dollar Program for the Assessment of Carcinogens in the Environment (PACE) has been proposed by EPA, and $1.1 million has been appropriated to begin the effort. Sound design, adequate support, and effective interagency coordination are needed if this critical research program is to succeed.

Birth Defects

A nationwide surveillance program should be established for early reporting of birth defects, and research should examine the possible contributions of environmental agents in causing such defects.

About 5 percent (150,000) of the babies born in the United States each year have birth defects. Of these defects, no more than one third have a known cause; current understanding suggests that the remainder may be caused by interactions between genetic and environmental factors, or among multiple environmental influences (Wilson 1975). Extensive experimental and clinical evidence has shown that some drugs and certain chemical, physical, or biological environmental agents can produce specific birth defects. Examples include the drug thalidomide, ionizing radiation, rubella virus, and methyl mercury. Although most demonstrated cases of defects caused by each of these teratogenic agents involved unusually high exposures, the potential of these and similar agents at low levels in the ambient environment for causing birth defects needs to be investigated carefully.

Birth certificates are routinely monitored to compile data on occurrence of birth defects in only a few areas, such as the states of Washington and New York. The cost and effort involved in obtaining such records are small, while the costs of not monitoring births could be great. Specific birth defects due to particular environmental agents are unlikely to occur so often that they will easily be noticed, unless special efforts to discover them are made.

Responsibility for establishing a national program for the surveillance of birth defects should most reasonably reside with the National Center for Health Statistics and the Center for Disease Control, since these agencies have experience in organizing such efforts. EPA is likely to be a major user of the information assembled under such a program, and should contribute to the support, planning, and review of the surveillance system, so that data of particular significance for the evaluation of teratogenic effects of environmental pollutants would be included.

Mutations

<u>Measurement techniques and monitoring programs for germinal and somatic mutations in humans should be developed and used to survey populations for genetic damage</u>.

Evidence on the incidence of genetic diseases is conflicting, and the role of environmental agents in producing mutations in man is largely unknown. Methods for testing mutagenicity in experimental organisms are of uncertain relevance to man; therefore, it is important to monitor human populations to detect increases in mutations that may be due to factors in the environment. A few effective tests for mutations in human populations have been developed, but no organized surveillance programs for large populations have been established. A need exists, therefore, to develop further effective techniques for detecting and monitoring mutations in man, and for maintaining surveillance and information systems.

Research is also needed to determine the significance of mutations that may occur because of environmental pollution. Very little is known about the health consequences of either germinal mutations (which may be passed on to future generations) or somatic mutations (which are confined to the individual who is exposed to a mutagen). Without more specific understanding of the impacts of mutations on the lives of individuals, the social costs of genetic damage are difficult to assess. The importance attached to mutations seems likely to increase as more is learned of their impact on health; population data on their incidence are therefore likely to be very significant for future decision making.

Institutional and funding arrangements for the development of testing methods and surveillance programs for mutations ought primarily to include certain of the National Institutes of Health, such as NIEHS, NCI, or the CDC. EPA might participate in efforts to define the relationships between certain environmental contaminants and mutations, but knowledge of this topic is still too rudimentary to expect early epidemiological surveys to produce information of immediate regulatory significance.

Behavioral Abnormalities

Existing techniques for measuring behavioral changes should be refined and used along with other measures of toxicity to study effects in human populations exposed to specific environmental agents.

Behavior is the product of the integrated function of the body's many organ systems and metabolic processes, and any number of toxic effects may be expressed as behavioral changes. For example, heavy smog adversely affects the performance of athletes, and carbon monoxide decreases alertness. Research on possible behavioral effects of most pollutants is at too early a stage of development to permit extensive interpretation of the risk of such effects. Nevertheless, it is likely that behavioral effects can occur at levels of exposure below those that produce other, less equivocal toxic effects (see NRC 1975b). Impacts on behavior may therefore be widespread; in the long run, such effects may represent a major social cost of pollution.

Because behavioral changes may be the most sensitive signs of toxic impacts on organisms, research to advance understanding of this class of effects should have high priority in the environmental health field. The opportunity exists for behavioral assessment to play an important ancillary role in clinical medical evaluations of populations exposed to toxic substances in the environment. To make the most of this opportunity, research is needed on the relationships between behavioral effects and other (often later) toxic consequences.

Current clinical neurological tests can detect overt disease, but are inadequate for measuring many more subtle behavioral changes. Some techniques developed in experimental psychology and psychopharmacology permit objective, quantitative measurements of human behavior; examples of such tests include those for physical strength and endurance, motor coordination and control, vigilance, and performance on simple, monotonous intellectual tasks (Valzelli 1973). Additional techniques to test for behavioral changes in humans are needed for assessment of behavioral effects of environmental pollutants. The needed techniques include:

- more precise and complete measurements of neurological dysfunction;

- psychometric tests to detect early, subtle, nonspecific, subjective behavioral effects, such as fatigue or mood changes;

- quantitative measures of behavioral effects in populations, such as school absenteeism on heavy smog days.

Behavioral assessments using existing techniques should be incorporated in examinations of individuals who are exposed both to high concentrations of toxic contaminants (as in an occupational setting) and to ambient levels of pollutants. Experience should suggest refinements needed to make tests more suitable for evaluations of effects of environmental agents. Each of the tests now available measures a different array of functional changes; it is important, therefore, to select a test that can answer a specific, relevant question. If, for example, a contaminant is suspected of making people less responsive to minimal changes in their surroundings, an appropriate behavioral screen would be to measure performance on a test for vigilance, such as the ability to detect infrequent signals presented in an experimental situation. If the likely nature of behavioral changes is unknown, a battery of tests may be required.

Evidence of behavioral effects drawn from epidemiological studies ought to be followed by controlled clinical studies of human subjects exposed to the suspected agents, where this is possible. Studies of behavioral effects should be combined with other clinical and epidemiologcal investigations, such as those on chronic degenerative diseases, described elsewhere in this chapter. To carry out the needed research, the various strengths of EPA, NIOSH, NIEHS, the National Center for Toxicological Research (NCTR), industry, private research institutes, and universities should be used in complementary fashion. The most critical need is to correlate behavioral assessments with other measures of toxic effects, rather than simply to conduct separate behavioral tests. Support should therefore be concentrated on integrated, multidisciplinary epidemiological research programs.

Chronic Degenerative Diseases

Coordinated epidemiological, clinical, and toxicological studies should be undertaken of populations at high risk of chronic degenerative diseases that may be caused by agents in the environment.

Chronic degenerative diseases (exclusive of cancer) are the leading causes of death and disability. Many have known environmental associations, such as that between chronic bronchitis and particulate air pollution (Storer 1975). Others, such as stroke, high blood pressure, bronchial asthma, and diabetes, have distinctive geographic patterns, clinical characteristics, or physiological mechanisms that suggest that they may have environmental causes or at least that there is a major environmental determinant of their frequency of occurrence.

Epidemiological studies are needed to identify populations at high risk of such diseases, either because of intrinsic special sensitivities or because of excessive exposures to potential causative agents. Once identified, such populations should be examined clinically, and toxicological tests should be undertaken to provide additional insights into pathogenesis and to help identify causative agents. In some cases, substantial epidemiological investigations may be needed to define approximate dose-response relationships, especially when long-term, low-level exposures and long latency periods are involved.

The need to include functional tests in epidemiological surveys deserves special emphasis. Most chronic degenerative diseases of such organs as the lung, liver, kidney, or circulatory system initially manifest themselves as physiological abnormalities, before morphological changes are apparent. These alterations in organ function can be detected by clinical tests, usually at a stage where the course of the disease is still reversible. If epidemiological studies include, for example, tests of liver or pulmonary function, they can make it possible not only to identify the early stages in the process of environmentally-induced disease, but also to halt or reverse that process in at least some cases.

EPA's current efforts to identify populations at risk because of exposure to polluted environments are laudable, but much more needs to be done. Populations with special occupational exposures and residents of geographic areas with unusually high incidence of specific diseases offer much potential for valuable studies. To avoid unproductive competition for the limited supply of both epidemiologists and funding, collaborative efforts among EPA, NIEHS, NIOSH, and other appropriate institutions should be pursued. As in the case of cancer epidemiology, EPA's strongest contributions are likely to be in the area of characterization and measurement of environmental factors that may influence particular diseases. The determination of the disease status of populations and the unraveling of causative processes should draw on the expertise of some of the more specialized units of the Department of Health, Education and Welfare.

TOXICOLOGY

Toxicological studies involving mammalian, avian, and aquatic species are the cornerstone of research to determine adverse effects of environmental agents. In this chapter, toxicology is discussed chiefly in terms of the use of animal (largely rodent) models as surrogates to test for potential effects on human health. Toxicological studies on other domestic animals and wildlife are important for the information they produce on environmental influences in diseases of species other than man (see Chapter 5). Toxicological experiments can demonstrate qualitative effects, such as cancer or birth defects, that might occur in humans as well as in test animals, and they can suggest quantitative (dose-response) relationships. For some effects of some agents in some species, a no-effect level can be demonstrated with a given degree of statistical reliability. Toxicological research also includes study of mechanisms of action leading to effects.

Animal studies have some serious limitations, however. There are many uncertainties in attempts to relate data on one species, such as a mouse, to possible effects in other species, such as man. Quantitative comparisons between species are especially difficult. Furthermore, it is virtually impossible to match the conditions of exposure in the laboratory with those in the ambient environment, either in terms of the timing of doses or in terms of the mixture of pollutants and other variables that occurs in the "real world." When effects of very low-level, long-term exposures are the object of the investigation, statistically valid measurements require either that a very large population of animals be exposed or that doses substantially higher than those encountered in the ambient environment be used.

Tests for chronic toxicity involve elaborate, often cumbersome procedures, may take several years to complete, and can be costly in equipment, animals, and manpower. While much will continue to be learned through long-term animal studies, the development of reliable and widely applicable short-term tests that can be used as rapid assays to select agents for more thorough testing is still an important need.

This report concentrates on those areas of toxicology that deserve high priority in research on the potential effects of changes in the environment. Chronic tests for carcinogenicity are not explicitly included among the recommendations here, because we believe that importance of chronic tests for carcinogens with animal models is clear and does not need to be stressed further. Emphasis should be placed, however, on continued development of short-term bioassays to serve as screens for potential carcinogens, and on epidemiological studies of environmental factors in carcinogenesis (see above).

Short-term Bioassays

<u>Further development of short-term assays to evaluate potential effects of environmental contaminants should continue to be given high priority</u>.

The current keen interest in effects of environmental contaminants and the recent enactment of toxic substances legislation have made more urgent the need for effective techniques for screening substances to determine the need for more thorough testing. Short-term bioassays that can reliably and inexpensively identify carcinogenic or mutagenic properties of chemicals are especially needed.

Research has been intense in this area in the last few years, and encouraging progress has been made (NRC 1975a). Some bioassay systems for carcinogenesis are refinements of tests in animals; these include the use of newborn animals, transplacental carcinogenesis, the transfer of carcinogen injection sites into a secondary host, and the creation of immunodeficiencies in animals to shorten the latency period. The chief disadvantage of these test systems remains the interval between the onset of exposure and the appearance of a tumor.

Other recently developed bioassay techniques used to indicate potential carcinogenicity are based on changes induced by the agents tested in microbial and cell cultures. These changes include production of chromosomal aberrations, cytological alterations, or mutations; alterations of DNA repair synthesis; reaction with nucleic acids; and <u>in vitro</u> cell transformation. None of these tests is completely predictive. Of those mentioned, the relationship between the carcinogenic activity of some compounds and their production of certain types of mutations has been examined most thoroughly. Ames (1976) reports a high correlation between carcinogenicity and mutagenicity when compounds are tested in the *Salmonella*-microsome test. When 300 substances classified on the basis of other data as carcinogens and noncarcinogens were tested, 90 percent of the carcinogens were mutagenic. Furthermore, only 13 percent of 108 noncarcinogens tested were mutagenic. Although this knowledge is very valuable, experience does not yet warrant reliance on mutagenicity as a substitute for carcinogenicity testing. (Information on mutagenicity is valuable for its own sake, as well; see the section below on bioassays for mutagenicity.) A drawback of *in vitro* methods is the inability of the cultured lymphocytes or fibroblasts used in the tests to carry out some enzyme-activated transformations that change nonactive substances into carcinogenic metabolites. Some investigators believe that cell transformation is more directly related to carcinogenesis than either of the other tests, but cell transformations have been reported for relatively few carcinogens. Nevertheless, this potential bioassay system has been chosen for extensive study by the NCI (McCann and Ames 1976).

Research to develop and refine short-term bioassays should continue to be conducted by agencies such as NIEHS and NCI through those of their in-house, contract and grant programs that already have the appropriate research direction and momentum. EPA as a regulatory agency should conduct screening programs for environmental contaminants, using the best developments that emerge from such research programs.

Effects of Exposures to Mixtures of Agents

Emphasis should be given to toxicological studies that simulate exposure to multiple agents that may be present in an environmental setting that poses a risk to human health.

Very little specific toxicological evidence is available on hazards to human health that result from exposure to low levels of heterogeneous mixtures of contaminants. Humans are exposed to contaminants in the air, in drinking water, in foods, or in combinations of all three. Although the need to evaluate multiple-agent interactions has long been recognized, the complexity of tests involving exposures to several agents by more than one route of administration is formidable. This is a relatively new field of research; it is one in which we feel a considerable expansion of effort is needed.

In many occupational environments and in polluted urban areas, the most common route of exposure is likely to be inhalation. Inhalation toxicology therefore is one useful tool for study of exposures to mixtures of contaminants. Inhalation studies have been conducted on a wide variety of single toxicants, and on some combinations of contaminants, among which are NO_2 and carbon particles (Boren 1964), SO_2 and carbon particles (Dalhamn and Strandberg 1963), SO_2 and soot (Pattle and Burgess 1957), SO_2 and NaCl aerosols (Amdur 1959) and chromium-bearing ore and diesel engine exhaust (Stuart et al. 1970). Some studies have shown synergistic interactions of contaminants that produced reversible or irreversible disease. Some combinations have been found to be less toxic than either of the compounds alone.

A great deal of information on inhalation toxicology has been developed, including considerable knowledge of the deposition and clearance of contaminants in the respiratory tract. Further refinements of the techniques of inhalation toxicology will be required for investigations of complex mixtures of contaminants. Replication of occupational or ambient environments for controlled studies is difficult because of the complexities of generating and monitoring multiple contaminants. Range-finding tests are required to set the exposure levels for a chronic evaluation for a given test animal, and concentrations significantly above ambient conditions may have to be used. Improvements are needed in methods of generating and monitoring contaminants, and in the design of chambers and air-flow systems.

The capability to conduct complex inhalation studies has been developed in a relatively small number of government and private or university laboratories. The most fruitful approach to accomplish needed inhalation research would appear to be to continue to support such programs.

A more complete evaluation of the effects of mixtures of contaminants requires tests based on other routes of administration, such as ingestion, as well as inhalation studies. Tests combining inhalation and ingestion of different agents are also needed to mimic the actual conditions of human exposures.

Bioassays for Mutagenicity

New and improved biological test systems for mutagenicity in mammals should be developed.

Many biological assays exist for testing the mutagenic properties of environmental contaminants. Most involve microorganisms or other simple systems such as *Salmonella* or *Neurospora* that allow rapid detection of mutations. The use of mammalian microsomal preparations in combination with microbial assays has increased the value of test results, but such tests are still of questionable relevance in evaluating the risk of mutagenic effects in man or other mammals. Mice offer a better human analog, but such studies are time consuming and require many animals. *In vitro* tests using cell cultures or tissue preparations (including some derived from humans) have proved valuable. In a few instances, blood or urine from persons exposed to drugs or other agents have been tested for mutagenicity on microorganisms (for example, see U.S. DHEW 1976).

Additional biological tests that are practical, accurate, and relevant are needed, both for screening chemicals for mutagenicity and for testing the ambient environment for possible mutagenic hazards. Significant opportunities exist to develop additional useful techniques. Research on the mechanisms of mutations should continue to be pursued, both to facilitate the development of bioassays and to increase understanding of mutagenic risks; programs in this area are under way now at NIEHS. Research to accomplish this goal should probably be relatively unfocused, and would be best performed in a number of existing university or government laboratories with funding through certain of the National Institutes of Health, such as NIEHS, NCI, or through the Food and Drug Administration (FDA), rather than through EPA.

Behavioral Effects

Toxicological research on behavioral effects should emphasize the development of correlations between behavioral changes and other measures of functional impairments.

Behavior depends on the functional integrity of a great many organ systems and metabolic processes. Toxic effects on any of the body's systems, such as the nervous, endocrine, or immune systems, may be expressed in behavioral changes. Behavioral effects may in turn involve a wide array of functional responses, such as motor strength and coordination, learning and memory, speech and communication, and emotional states. Examples of behavioral effects in humans include interference with sensory and motor integration, and intellectual and emotional disabilities associated with methyl mercury and lead toxicity, respectively (Bryce-Smith 1972, Evans et al. 1975, Spyker 1976).

As noted earlier, behavioral effects can be extremely deleterious to individual humans and to society. Behavioral changes in animals may have significant ecological consequences, through impacts on processes like feeding, reproduction, and predator-prey interactions. Current knowledge suggests that subtle behavioral changes may occur at extremely low dose levels, and thus may be sensitive indicators of toxicity that might be used to detect harmful effects before irreversible damage has been done (NRC 1975b).

Assessments of behavioral effects in laboratory animals, therefore, are potentially very useful for evaluating a wide range of toxic responses. This advantage is partially offset by the complexity of behavior. So many different functions are involved in any given behavior that a battery of tests is usually required to screen for behavioral changes. Furthermore, compensatory mechanisms within the organism may mask a toxic process and its behavioral expression until overt physical damage has been done.

A more fundamental difficulty, however, lies in the interpretation of results of behavioral tests. A great number of techniques available from experimental psychology can reliably measure effects on behavior; but behavioral toxicology is still too new a field to have developed unifying concepts or principles for determining the significance of behavioral effects.

Two approaches, epidemiology and toxicology, offer great promise to shed further light on the effects on behavior of a polluted environment. Epidemiological studies on behavioral responses, discussed above, must focus on the correlations between changes in behavior and other expressions of toxicity. The most productive approach in behavioral toxicology is also to emphasize investigations that can reveal relationships between behavioral changes and other measurements of functional impairment based on such techniques as neurophysiology, histochemistry, and morphology. The relationship between a given behavioral change and underlying functional deficiencies must be understood before behavior can be used as a sensitive indicator of other, covert toxic effects, and before behavioral screens can be employed to assess the potential toxicity of substances in a regulatory context.

A secondary objective of research in behavioral toxicology should be the development of quantitative, practical, and reproducible test batteries that use behavioral changes to measure specific functional impairments. Until the meaning of particular behavioral changes is determined, however, uniform testing procedures cannot be developed (Spyker and Avery 1976).

Coordinated multidisciplinary toxicological research is required to determine the correspondence between various functional effects and their behavioral consequences. Programs with the needed combinations of training and experience to carry out such research can be found in a small number of universities and some government laboratories. Because of the fundamental nature of much of the research needed, universities should play a relatively large role in carrying out the recommended programs. Several federal agencies have an interest in applications of behavioral tests, and should be involved in the support and coordination of this research; among these agencies are the National Institute of Mental Health, NIEHS, FDA, NIOSH, ERDA, and EPA (through NCTR). Some research to develop behavioral assessment techniques and to test specific substances before making regulatory decisions has been conducted within these agencies and should probably continue. NIEHS should be best suited to coordinate research on behavioral effects of environmental pollutants.

Teratology

Teratological research should increasingly emphasize assessments of postnatal functional disorders caused by prenatal or perinatal exposure to toxic environmental agents.

Toxic substances, including environmental pollutants, can have marked adverse effects on the development process. Embryos are especially vulnerable to exposures during the organogenic phase of early development, but functional disorders produced prenatally by toxic action may appear at any point in the process from conception through postnatal maturation. In mammals, the nervous, endocrine, and immune systems seem to be most susceptible to damage; all three continue to differentiate well into the postnatal period.

Following the thalidomide incident in 1960, tests for teratogenicity have been required for the approval of new drugs; consequently, there is a substantial research base from which to conduct assessments of the teratogenic potential of environmental contaminants. However, measurements of teratogenicity have so far been largely based upon the detection of relatively gross defects, such as structural abnormalities, overt functional impairments at birth, or retarded growth. It is unlikely that most exposures to environmental contaminants will produce such clinically obvious defects; and many subclinical effects may be overtly expressed only later in life, as defects in subsequent maturational processes such as puberty. It is important, therefore, that the evaluation of teratogenic effects of environmental agents include tests for subtle functional impairments and that evaluations be continued for an extended postnatal period.

Although the assessment of minimal functional disorders is a relatively new direction for teratology, there are a number of useful tests that measure such effects. Nervous system damage as well as impairments of other systems can be detected with behavioral tests (see the preceeding section), and specific tests for endocrine, immune, and liver enzyme functions have been used successfully in a number of studies (Wilson et al. 1976).

An important related need is for research on mechanisms of action of teratogens and the influence they may have upon biochemical processes of cellular differentiation. Because of the large number of chemicals and other agents (and combinations of agents) to which an embryo may be exposed and the length of time required for complete post-natal evaluation of potential teratogenic effects, short-cuts are urgently needed to estimate the risk of teratogenicity for a particular substance or class of substances. At present, the best hope of finding such short-cuts is probably to learn more about the mechanisms of action of specific substances. Knowledge on this subject is scanty, but opportunities exist for significant progress through more intensive research (Wilson et al. 1976).

Most teratological research at present is conducted by drug companies to meet FDA regulations on evaluation of new products. If this work were expanded to include more extensive assessments of subclinical functional deficits over the life of the organism, it might be of some general use for environmental decision making. More specific evaluations of teratogenic effects of environmental contaminants are under way at NIEHS and NCTR. Some excellent work is being done in a number of universities, with support primarily from the National Institute of Child Health and Human Development and the National Foundation--The March of Dimes. Increased support of those existing research programs that are moving in the directions described here appears to promise results most useful for evaluating the teratogenic risks of pollutants.

MECHANISMS OF ACTION

Research should be expanded to develop more complete information on the mechanisms of action of a limited number of environmental contaminants and effects.

Information on the mechanisms of action of toxic substances can provide the crucial link between evidence of environmental exposures and observed effects (Miller and Miller 1974). The term mechanisms of action encompasses the full range of biochemical and physiological processes that occur in living systems in response to environmental contaminants. From initial exposure to some end point or effect, processes at levels from the sub-molecule to the organism as a whole determine the ways in which substances are absorbed, translocated, and transformed. A similar array of processes governs the responses of living systems to the presence of contaminants. Research on mechanisms seeks to illuminate, in biochemical and physiological terms for the most part, the details of the processes and changes that occur.

Critical areas of information related to mechanisms include:

- Uptake: Biological availability at the host/environment interface and at the surface of the target cell.

- Anatomic Fate: Identity of target cells; rates and extents of translocation, accumulation, storage, mobilization, and excretion of chemicals and metabolites.

- Metabolic Fate: Pathways, products, by-products, and rates of the metabolism of the substance; toxication and detoxication.

- Pathogenesis: Functional and structural changes in target cells, tissues, and organs.

Knowledge of mechanisms of action is extremely important for the inference of causal relationships. It can also provide early indications of biological effects that could make it possible to detect the initiation of a pathogenic process before overt disease appears, when preventive action or early treatment are possible. Information on mechanisms also offers a basis for deducing the potential effects of substances that have not been extensively tested. When the mechanism of action of a known carcinogen, mutagen, or teratogen is understood, a demonstration that another substance produces analogous biochemical or metabolic changes might lead to a presumption that the second substance could have similar toxic effects. Such extrapolations require further proof, but knowledge of mechanisms is the most valuable presumptive basis for choosing additional tests and for estimating risks in the absence of results of more time-consuming studies.

Examples of the need for information on mechanisms can be found in almost all of the problem areas discussed earlier in this chapter. In research on mutations, for instance, both the metabolism of mutagens and the molecular events associated with mutation need to be understood. Such knowledge could help to answer the difficult question of whether a threshold exists for mutagenic hazards. The study of environmental carcinogenesis requires information on the metabolic degradation of substances and the interactions between products of metabolism and the genetic material of cells. Because of the long latent period until tumors appear, knowledge of changes in cell or tissue function that occur early in the oncogenic process is very much needed.

It has been suggested that a small number of metabolic pathways, perhaps fewer than a dozen, are involved in the metabolism of environmental contaminants by living systems (Gehring 1975). Nevertheless, the processes may be extremely complex; for example, different biochemical pathways may be activated at different dose levels of a single substance (Miller and Miller 1974). Because of this complexity, it seems unlikely that study of limited aspects of the mechanisms of action of a large number of substances would lead to useful generalizations within the near future. A sounder research strategy would be to emphasize concentrated efforts to fill in gaps in knowledge of the mechanisms of action of a few well-studied agents (e.g., chlorinated hydrocarbons or heavy metals). Detailed understanding of the molecular kinetics of processes that occur in response to a range of doses and under different modifying conditions will be most useful both for decision making and as a guide to future research.

Vital as it is to understanding effects, research on mechanisms is subject to some important limitations. Variations in the ways a single organism or different species respond to a given agent make the meaning of many findings uncertain. Demonstration of a mechanism in a tissue culture, or even in a live animal, is not evidence that the same process occurs in other animals or man; and elucidation of the mechanisms of action of single agents does not necessarily allow estimation of the effects of combinations of agents. Understanding a mechanism of action may require years, not months, of careful research (Miller and Miller 1974).

Nevertheless, the answers such studies can provide are very important, and this field should continue to receive substantial emphasis in research planning. Several excellent existing programs, such as those at NIEHS and NCI, have been mentioned in earlier portions of this chapter. EPA should interact closely with NIEHS and other agencies involved in research on mechanisms, to ensure that the work is useful for the evaluation of potential environmental hazards; EPA need not, however, develop an extensive program of its own to do such work.

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CHAPTER 5

EFFECTS ON WILD AND DOMESTIC ANIMALS

Human-induced changes in the environment have had adverse effects on both wild and domestic animals. Damage to agricultural livestock, poultry, pets, and other domestic animals has occasionally been severe, with important economic consequences (Lillie 1970). Damage to wild birds, fish, and mammals has decreased the aesthetic and recreational value of the environment, and this in turn has had considerable economic impact (L. Stickel 1975). In ecological terms, effects on populations of one species may have consequences for other elements in biological communities that interact with the affected organisms. In addition to their intrinsic importance, effects on wild or domestic animals may occasionally provide corroborative evidence or warnings of potential risks to humans.

Research on effects on domestic animals and wildlife must develop the same lines of evidence needed to evaluate human health hazards. Specifically, closely coordinated studies are needed to assess the health status of animal populations in the field before, during, and after pollution episodes of short or long duration; to measure exposures of animals to agents suspected of causing any effects observed; to test the effects of the same agents on animals under controlled laboratory conditions; and to elucidate the metabolic and ecological mechanisms of effects. Effective study of the impacts of environmental contaminants on animals requires integrated multidisciplinary research.

Large gaps remain in our understanding of effects of environmental contaminants on animals. Much of the published research has been done in response to particular problems or specific needs for regulatory action, and lacks both the breadth and the depth required to support wider extrapolations or more fundamental principles. Furthermore, the state of knowledge is different for domestic and wild animals.

The biology and physiology of most economically significant livestock, pets, and other domestic animals have been studied extensively. Relatively detailed knowledge is available on toxic effects of some air pollutants, such as fluoride and arsenic, and of certain water contaminants, such as nitrate, that have had important impacts on livestock (Lillie 1970, NRC 1972). The nutritional and health effects of both excessive and insufficient ingestion of many other trace elements have also been well documented, chiefly from the perspective of improving animal production. Investigations of effects of environmental contaminants have been relatively uncommon, except in cases of substantial and obvious injury to livestock. Very little is known yet about, for example, the effects on domestic animals of common urban air pollutants like ozone and other photochemical oxidants (Lillie 1970). Many farms and feedlots lie within heavily polluted airsheds (such as the San Bernardino-Riverside basin), and it is important to know whether such environmental conditions have had significant effects on the health or productivity of livestock.

There are a number of striking and well-documented examples of effects of environmental contaminants on wildlife, such as eggshell thinning in fish-eating birds due to chlorinated hydrocarbon pesticides in the food chain. In general, however, information on actual or potential effects of contaminants on wildlife is far from complete. Relatively little is known of the physiological responses of wild species of terrestrial and aquatic vertebrates and invertebrates to potential environmental hazards. Unlike domestic animals, most wildlife organisms must continuously interact in demanding ways with their surroundings (to find food or a mate, to avoid predators, and so on). For wildlife, therefore, subtle effects, such as changes in reproductive success or behavior, may have consequences for populations that are as important as, or more important than, mortality or overt toxicity.

A great deal of field and laboratory research on wildlife has been conducted both on relatively easily observed acute effects and on chronic, more subtle reproductive or behavioral impacts. Investigations have concentrated on organisms with important economic significance, such as agricultural pests, pollinators, shellfish, commercial food and game fish, and the food chain organisms on which the latter depend. Effects on wild birds and mammals have been studied less intensively, although these effects have received much public attention.

Despite the substantial information base available, knowledge of effects on wildlife is far from adequate (W. Stickel 1975). One fundamental problem is the enormous number of contaminants that are widespread in the environment, and the vast array of species that may be at risk. Another important lack is knowledge of mechanisms that exist within wildlife populations that may offset or compensate for effects of pollution.

Some basic problems common to research on both domestic and wild animals have limited the usefulness of much of the available information for estimating risks. Vulnerability to most effects differs sharply and unpredictably among species; even within a species, effects can vary with such characteristics as age, sex, nutritional condition, and seasonal or reproductive state (W. Stickel 1975). Some contaminants are ingested or absorbed directly by animals, while others are accumulated by plants or other organisms in food chains. Toxic effects may appear only in animals at higher levels of the food web, sometimes well removed in space and time from sources of pollution. Indirect impacts may occur in some populations through ecological interactions (such as decreased availability of prey or pollinators) rather than through direct toxicity.

One very important area in which more information is needed is the combined effects on animals of multiple pollutants and other environmental or physiological stresses. Many studies of the toxicity of mixtures of agents have been done in the laboratory, primarily with aquatic organisms, and interactions between some pesticides and physiological stresses (such as hunger) have been examined in some birds and fish (W. Stickel 1975). In general, however, not enough is known to estimate the risk involved in simultaneous or sequential exposures to multiple contaminants that are likely to occur in the ambient environment.

To achieve the capability to predict and prevent effects on domestic animals and wildlife, some broad principles of chemical ecology must be developed. Unifying concepts are needed to understand and predict species differences, to assess the risks of mixtures of pollutants, and to resolve other currently unanswerable questions. Such a structure of knowledge should be built up through well-conceived, coordinated interdisciplinary research. Some critical programs to develop needed information are described in the recommendations that follow.

EPIDEMIOLOGY OF DOMESTIC ANIMALS

Efforts should be expanded to apply epidemiological techniques and surveillance programs to environmentally-caused diseases of domestic animals.

Agricultural livestock, pets, animals in zoos, and small birds and animals that commonly share human habitats are exposed to many of the same risks of environmentally induced diseases as people are. Specific functional effects, causative agents, and routes of exposure may be similar to or different from those experienced by man, but the end results--death, degenerative disease, cancer, birth defects, mutations, or behavioral abnormalities--may be much the same for a great many species.

Effects in animals with relatively short lifespans may, in some cases, serve as early warnings of potential chronic effects in humans, and animal epidemiological data might provide corroborative evidence of patterns of diseases in man. In many other instances, the suffering and economic damage that environmental agents cause in animal populations are important in themselves, regardless of implications for human health. Some notable pollution-related outbreaks of disease in domestic animals include fluorosis in cattle and sheep grazing in the vicinity of phosphate or aluminum processing operations (NRC 1974), and toxic effects of feed contaminated with polybrominated biphenyls in farm animals in Michigan (Isleib and Whitehead 1975). It is very likely that there are many additional distinctive (if less obvious) environment-linked patterns in the incidence of diseases of domestic animals, but unless attempts are made to find them, they may go unnoticed. Studies should be undertaken to look for such patterns, and when they are identified additional tests should be pursued, including collection of tissue samples, toxicological experiments, and clinical veterinary studies.

Capabilities for conducting animal epidemiology are relatively limited at present. Most states maintain registries of diseases of livestock, and registries of tumors in pets have been proposed in some areas, but no systematic effort to organize such a surveillance program has been made. The opportunity exists, in connection with human epidemiological studies that are likely to be undertaken in the next several years (see Chapter 4), to include information on nonhuman species exposed to the same environments (Shimkin 1974, Doll 1976). Additional studies of livestock in rural areas might also be useful. Neither all the required techniques for detecting environmentally induced diseases in animals nor the manpower needed to mount an effective nationwide program in this field is yet available. A pilot program might be carried out instead in a small number of carefully selected areas as part of studies of human disease patterns in the same locations. If this approach fulfills its apparent promise, it should receive a higher priority in the future.

TOXICOLOGICAL STUDIES

Increased emphasis should be given to toxicological tests for subtle effects of environmental agents on a wider array of wild and domestic animals.

The physiology, toxicology, and pharmacology of wild and domestic birds, mammals, fish, and other organisms differ considerably from those of man or of the standard laboratory animal surrogates for man. Advances in toxicological methods for evaluating risks to human health should be paralleled by the development of increasingly sensitive techniques for detecting effects in other species. Many available methods are adequate only for measuring acute effects of high exposures; a number of more sensitive tests exist, but have been employed with relatively few species. Most of the important chronic effects of low-level environmental pollution are likely to be insidious. Experimental methods are needed that can detect mutations, effects on behavior or reproductive processes, and subtle or subclinical disease conditions that may make the organism more susceptible to other environmental hazards, such as predation, pathogens, or adverse weather.

Some sensitive toxicological methods have been adapted to test for low-level toxicity in wildlife; for example, behavioral changes have been observed in fish and birds in response to pollutants (Warner et al. 1966, Anderson 1971, Heinz 1976). Emphasis should be placed both on extension of emerging techniques to other important wildlife species, and on increasing understanding of the biological and ecological meaning of observed effects.

Determination of the ecological significance of behavioral and other subtle effects on animals requires laboratory studies that go beyond single species and examine interactions typical of the ecological community. Research is needed to advance conceptual models and techniques for experimental study in this area. Initial efforts should be directed toward developing laboratory tests for effects on population interactions, which might then be verified in the field. The research would need to be multidisciplinary, coordinating physiological, toxicological, and behavioral studies with the sorts of investigations of effects at the community and ecosystem level that are described in Chapter 7.

It is important to examine effects of pollutants on animals of varied taxonomic groups and habits under different seasonal and physiological conditions. Research on effects of combinations of chemicals, administered concurrently and consecutively, deserves high priority. For toxicological studies to be most useful for evaluating effects in the field, it is essential to examine the influence of as many of the relevant variables as possible under controlled conditions.

FIELD STUDIES ON WILDLIFE

> _Field investigations should be carried out in parallel with experimental studies to examine the population dynamics, behavior, and reproductive success of wildlife species in polluted environments_.

To test the significance in the life of the organism of any effects revealed by laboratory toxicological tests, it is necessary to study wildlife populations in the field. Laboratory and field studies must be closely coordinated, so that ecologists and toxicologists can make comparable observations on the same species.

While laboratory studies are concerned with the responses of small, confined populations of animals, field studies must deal with effects on larger, free-living populations. Behavioral, reproductive, or other chronic effects may lead to changes in the size, age structure, or spatial distribution of populations. Field research often requires many years for full evaluation of such effects, because biological processes may allow populations to respond to or compensate for adverse conditions.

Disease, competition, predation, the weather, and inherent characteristics of the population contribute to natural fluctuations in population density, and other factors unrelated to pollution may have adverse effects on wildlife. If effects of pollutants are to be distinguished from those due to other causes, field research must be closely tied to both laboratory tests and investigations of mechanisms.

MECHANISMS OF ACTION

Increased research on the physiological and ecological mechanisms of effects is needed to support findings of laboratory and field studies.

The elucidation of biological mechanisms of action is another critical element in understanding effects on wild and domestic animals. Information of this sort can be obtained by study of the metabolism and pharmacology of chemicals in organisms and by examination of ecological processes such as bioaccumulation, food chain transfer, and degradation of pollutants. Such knowledge is needed to develop principles for defining, for example, the physiological basis of differences among species, the nature of interactions among pollutants or among pollutants and other stresses, and the relationships between effects on a given organism and those on others at different levels of a food web. Such general principles are essential for the prediction of ecological effects from limited data on the biological activity of a substance.

A second valuable result of research on mechanisms is knowledge of the physiological correlates of effects on behavior, reproduction, or other important functions; such knowledge can lead to accurate, sensitive, and simple bioassays for effects. Increased understanding of the mechanisms of toxicity might also provide a basis for the use of some specific wild or domestic animals as "sentinels" to provide early warnings of potential environmental hazards to man, or to other wild or domestic species.

INSTITUTIONAL ARRANGEMENTS

Research on effects of environmental contaminants on various animal species is being conducted in many government agencies and universities. Studies of effects on livestock and other domestic animals have been conducted or supported by the Department of Agriculture (USDA), many state agricultural research programs, the animal producing industry, some health agencies, and university scientists in several disciplines. Research on effects on wildlife is being done by or for the Department of the Interior (Fish and Wildlife Service), the Department of Commerce (the National Marine Fisheries Service, in NOAA), USDA, EPA, ERDA, NIEHS, and some state agencies. Additional contributions have come from a number of universities, and NSF has supported investigations in this field.

In general, roles and responsibilities for research among the many agencies involved have been reasonably well defined and separated, but improved coordination is still needed in some areas, such as effects on aquatic organisms.

The research recommended here can probably be accomplished best, at least in terms of federal involvement, within long-term research programs in agencies with the greatest interest and expertise in appropriate areas: USDA for livestock and other domestic animals, and DOI and NOAA for fish and wildlife. Some of the basic biological research might be done within or be supported by certain offices of NIH, especially NIEHS, and in universities.

As in most environmental research, interagency coordination is important; in particular, research in other agencies should draw upon EPA's knowledge of the current or likely state of the environment and potential impacts of new technologies, and EPA should be made aware early of any problems that might have important impacts on domestic or wild animals.

A substantial amount of research manpower and resources will probably need to be devoted to standardized toxicological tests on domestic animals and wildlife to develop information for regulatory decisions. Work of this sort (beyond that required of industry) ought to be performed by EPA, possibly through contracts with qualified private institutions. Short-term research of this sort is essential; however, many of the information needs identified in this chapter will be met only through sustained efforts that may require five to ten years to produce answers.

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CHAPTER 6

EFFECTS ON AGRICULTURAL AND FOREST PLANTS

Ample proof exists that plants have been injured by changes in the character of the environment, especially by air pollution (Jacobson and Hill 1970). Vegetation injured by pollution may be killed or visibly damaged, or the effects may be more subtle, such as reduced growth or yield, decreased reproductive success, or lowered resistance to disease, drought, or other stresses. Some current environmental problems, such as chronic low-level air pollution by photochemical oxidants or the increased acidity of rainfall, may have effects on vegetation over extensive geographical areas. Such effects on plants could lead to changes in the stability or productivity of agricultural, forest, or other ecosystems.

The predominant emphasis in research on injury to vegetation has been on the effects of air pollutants on agricultural crops and on trees. The experience of this panel, and thus the recommendations of this chapter, reflect that emphasis. Other kinds of vegetation may also be affected by pollution, however, and those effects may have important ecological and economic consequences. Although they are not dealt with further in this chapter, we feel that several additional areas of research deserve continued or increased interest and support. These include studies of the effects of air and soil pollutants on lichens, fungi, grasses, shrubs, and other native vegetation; investigations of the effects of water pollutants on algae and other aquatic plants; and studies of the effects of environmental contaminants on ornamental plants. In particular, research on effects on algae deserves high priority, both because of the important ecological role played by algae and because of the dearth of information about algal responses to most toxic pollutants.

A great deal of research has been conducted on the acute effects on a variety of agricultural and forest plants of some air pollutants (chiefly sulfur dioxide, ozone, and fluoride). Several estimates are available of chronic damages, primarily due to photochemical oxidants (NRC of Canada 1975). Dose-response data are simply not available, however, for the chronic effects of most contaminants, especially on natural plant communities and ecosystems. To develop the information it needs for decisions such as establishing or revising secondary air quality standards, EPA will need to draw on the experience and resources of many elements of the plant research community. Major contributions to research on effects of environmental contaminants on vegetation have been made by universities, nonprofit research institutes, and government laboratories, with funding largely from USDA, EPA, NSF, private industry, and utilities; this diversity in research should continue. To avoid unnecessary duplication of effort, sponsors of research should encourage timely reporting of research results.

Before relationships between environmental contaminants and vegetation damage in agricultural and forested areas can be determined, the concentrations and trends over time for important pollutants in those areas must be measured. The environment in or near urban and industrial locations has been extensively monitored, but there has been no coordinated effort to provide a continuous record of pollutant levels in agricultural and forested portions of the country. Improved monitoring is needed in order to estimate the effects of pollution on plant communities or to enforce nondegradation policies or similar protective measures (Miller et al. 1972).

The most important phytotoxic pollutants to monitor are ozone and sulfur dioxide, which now cause and will probably continue to cause the greatest amounts of injury to vegetation, at least within the United States. It might also be useful to monitor peroxyacetyl nitrate (PAN), fluorides, and nitrogen dioxide in specific rural locations, and many other chemical pollutants or trace substances, as appropriate, near specific sources.

The hydrogen ion content (pH) of rainfall, which is determined by the kinds and amounts of anions and cations contained in precipitation, may have significant phytotoxic and ecological effects. Increased monitoring in remote areas of both acidity and the concentrations of specific ionic components of wet and dry precipitation is needed to support studies of the effects of acid rainfall on agriculture and forestry.

A coordinated nationwide program of monitoring of air quality and of the composition of precipitation in agricultural and forested areas should be undertaken. A review of monitoring needs has been conducted concurrently with this project by the Study Group on Environmental Monitoring of the NRC, and their report should be consulted for additional recommendations and discussion of monitoring requirements and methods (NRC 1977).

LABORATORY STUDIES ON PHYTOTOXICITY

Efforts should be expanded to develop quantitative dose-response data for the effects of pollutants on plants, through indoor experiments under controlled conditions.

Basic dose-response data are still needed on the effects of most environmental contaminants on plants, especially for long-term, low-level exposures (Larsen and Heck 1976). Indoor studies that attempt to simulate outdoor growing conditions provide models of effects of pollutants, which can then be tested in the field. Plant populations in the field are influenced by many natural variables, including light, temperature, air and soil moisture, soil nutrients, insects and pathogens, and physiological states (seasonal changes and life-cycle phases). The effects of exposure to one or more pollutants are usually superimposed on effects due to other stresses, so that it is difficult to assign a particular cause to observed chronic adverse effects. The chief advantage of laboratory testing is that the interactions among several such stresses (including mixtures of pollutants) can be studied by varying one factor at a time.

Although elaborate chamber studies have been conducted in a number of institutions for many years, significant opportunities to advance the sophistication of research methods still exist. For example, programmable or computer-controlled exposure chambers can be used to vary one or more factors and more closely simulate actual stress conditions (McLaughlin et al. 1976, Doshi 1975). The study of dynamic, multivariate systems is a promising direction for future research.

Laboratory research would best be accomplished in those government agencies, nonprofit research institutes, and agricultural experiment stations that have the necessary facilities and experience to conduct complex chamber studies. Funds should be provided by EPA, USDA and the Forest Service (USFS), ERDA, and other interested public or private sources.

FIELD STUDIES OF VEGETATION DAMAGE

Additional emphasis should be placed on field research to measure the effects of concentrations of pollutants on plant growth and productivity under actual growing conditions.

Because no laboratory study, no matter how sophisticated, can duplicate the actual conditions plants encounter in the outdoor environment, measurements in the field of injury to vegetation are essential. The soundest assessment of the actual risks pollution represents to crops, forests, and other plant communities is one based on mutually confirming results of indoor and field studies.

Some promising techniques have been developed for making quantitative measurements of the effects of air pollutants on crop productivity. Field plots can be covered with enclosures that exclude pollutants (by filtration) but make minimal changes in other environmental conditions (Mandl et al. 1973). Measured amounts of pollutants can also be added to such enclosures, or to field plots with no enclosures (Lee et al. 1975). Effects of exposing vegetation to a given pollutant concentration in the laboratory can thus be compared with effects of the same known concentrations under field conditions.

Field studies and indoor experimental research must be closely coordinated to ensure comparability of results. Unfortunately, the research teams and institutions that are best prepared to do large indoor studies may not be experienced in field research techniques. Programs should be designed that incorporate both elements. Here, too, support should be provided from EPA, USDA, USFS, ERDA, and other interested public or private sources.

MECHANISMS OF PHYTOTOXICITY

Research should be intensified on mechanisms of action of toxic environmental agents on plants.

Lack of knowledge of the metabolic and physiological mechanisms of toxic effects of pollutants on plants hinders the achievement of several important objectives. Perhaps the most significant of these is the development of practical, reliable bioassay techniques for detecting injury to vegetation in the field on the basis of physiological indicators. Present measures cannot reliably distinguish metabolic responses of plants to pollutants from responses to other stresses, such as cold or drought. By the time more reliable indicators of toxicity such as visible foliar injury or reduced growth appear, damage may be irreversible. A better understanding of the physiological mechanisms of toxicity could make it possible to detect injury at a time when corrective action might still be possible.

Research of this kind should also advance understanding in such problem areas as breeding pollution-resistant varieties of plants, assessing the effects of interactions among multiple toxic agents and other stresses, anticipating the impacts of new pollutants, extrapolating estimates of injury from one species of plant to others that metabolize a contaminant in the same way, and determining the utility of the threshold concept for damage to vegetation.

The elucidation of mechanisms of phytotoxicity is likely to be accomplished best through long-term research efforts in universities, private research foundations, and the few government laboratories that study fundamental problems of plant physiology. At present, the demand for dose-response data and other information of immediate value is great. In contrast, support for more basic investigations of mechanisms has become increasingly limited. Greater funding in this area should be encouraged; it might most reasonably be provided through NSF or other granting agencies that are not mission-oriented.

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CHAPTER 7

EFFECTS ON BIOLOGICAL COMMUNITIES AND ECOSYSTEMS

Humanity depends on the continued functioning of ecosystems for food, timber, and other renewable resources and for recreation and aesthetic amenities. Ecological processes also assimilate wastes, recycle nutrients, and regenerate clean air and water. To protect these largely irreplaceable values, it is very important to know the effects of human activities on ecosystems.

Evaluation of the potential ecological impact of a given environmental change requires information on effects on units of biological organization above the level of the population or species. We need to examine structural features of biological communities, such as diversity and abundance of species and interactions among populations, and functional characteristics of ecosystems, such as the transfer of energy, production of biomass, and cycling of oxygen, water, and mineral elements. The stability and resilience of these characteristics over time is another important property of the system. Any factor, such as pollution, that significantly alters the rates of one or more critical processes within a system can shift the system to a new equilibrium state.

Spatial heterogeneity, the mobility of organisms, dormant structures, and other mechanisms give most ecosystems a capacity to recover from many kinds of perturbations, even those that have severe immediate impacts. Some environmental impacts, however, might produce long-lasting or irreversible detrimental ecological changes. It is therefore important to know not only the kinds of effects that may occur, but also the ability of the system to recover from those impacts it may absorb.

A great deal of research on the structure and functions of ecosystems has been conducted in the last decade under the auspices of the International Biological Program (IBP), the NSF-RANN Environmental Systems Program, and other major projects. Much less has been done to study the responses of ecosystems to specific pollution stresses. Available approaches for investigating such problems include laboratory model systems, or microcosms; field studies of experimentally perturbed communities of limited dimensions; mathematical (computer) simulation models of ecosystems; and field studies to measure effects on the scale of a regional ecosystem. The predictive accuracy of each approach alone is distinctly limited; taken together, they can produce research results that may approximate measurements of ecosystem effects.

LABORATORY MICROCOSMS

Techniques based on laboratory microcosms should be refined and applied, but the limited utility of such systems for predicting ecosystem effects must be recognized.

A number of bioassay techniques have recently been developed that employ simplified microcosms of biological communities. Small populations of six or seven "typical" organisms representing different ecological roles are kept in a small aquarium or terrarium; a sample of a chemical to be tested is introduced, and its behavior and effects are observed (Metcalf 1975).

Microcosm studies are appealing because of the speed with which they can be used to test many chemicals. The systems are relatively simple and can be mass-produced. It is likely that such models will be increasingly used in evaluations of the potential environmental impacts of new chemicals and other pollutants. It is very important, therefore, to recognize the inherent limitations of the technique for simulating the processes of actual ecosystems.

Some microcosm studies have been useful for predicting the behavior of chemicals in the environment. Results showing the extent to which substances may be bioaccumulated or degraded, and the nature of the breakdown products, have been relatively accurate, as confirmed by studies of the same chemicals in the outdoor environment. Microcosm studies, therefore, can be very useful screening devices for identifying substances whose behavior may indicate potential risks of biological effects (NRC 1975).

The laboratory microcosm is not, however, an adequate model for predicting either the quality or the magnitude of effects on ecosystems. Because of their structural simplicity, microcosms lack homeostatic mechanisms present in real, complex systems. They are thus subject to random changes, and to instabilities and perturbations that might not occur in nature because of built-in resilience. Furthermore, it is generally not possible to project the probable ecological consequences of effects that may be observed in microcosms, because the mechanisms of effects on real ecosystems are simply not well enough understood yet. In short, while microcosm studies may be useful to screen substances for further study, reliable predictions of effects on ecosystems require more elaborate tests.

EXPERIMENTAL PERTURBATIONS OF COMMUNITIES

Increased emphasis in research on ecosystem effects should be placed on experimental perturbations of biological communities in the field.

Experimental perturbations of ecosystems are a very useful tool for producing important information about ecological effects of environmental changes. In such studies a number of nearly identical plots, ponds, or streams within a given system are used in statistically designed controlled experiments to examine the effects of specific perturbations on the structural integrity and stability of biological communities. The parameters measured include species diversity, predator-prey interactions, productivity, energy transfer, and other selected characteristics of the system. To control changes due to the cyclic nature of many biological processes, measurements should be made under different appropriate seasonal and environmental conditions.

Experimental manipulations of communities and ecosystems have been used successfully in recent ecological research; examples include the Hubbard Brook experimental watershed and the Brookhaven irradiated forest (Bormann et al. 1974, Dayton 1971, Hall et al. 1970, Paine 1966, Simberloff and Wilson 1969). Such studies involved selective removal of key species, introductions of new species, eradication of whole trophic levels, and intentional changes in inputs of nutrients, toxic substances, radiation, or heat. Techniques both for making experimental perturbations and for measuring biological responses are relatively well-developed.

The chief advantage of field studies of this kind is that they measure effects under actual environmental conditions; there is no other reliable way to obtain much of the needed information. However, such studies are elaborate and costly, and can look at only relatively small spatial units of ecosystems, which may not be representative of the total system in their response to stresses.

Some studies of this sort should be undertaken as controlled experiments; that is, a pollutant or other environmental stress should be introduced as part of the study design. In addition, research should continue to take advantage of subsequent effects of accidents (large oil or chemical spills, for example) and of existing pollution-stressed systems (such as the forests around a lead smelter or an aluminum refinery), where comparison studies can be conducted after the fact.

Because of seasonal and annual variability, the resilience of natural systems, and the time scales of many ecological processes, several years of observations may be required before reliable evidence of ecological effects can be obtained. The substantial costs of long-term field ecological research make it important to develop a relatively small number of sites that can be sustained until useful results are derived.

SIMULATION MODELS OF ECOSYSTEMS

Continued research is needed to improve simulation models of ecosystems and their applications to the study of effects of environmental changes.

Mathematical simulation techniques, usually computer-based, can be very useful in modeling the behavior of at least portions of ecosystems. Like laboratory microcosms, such simulation techniques cannot, by themselves, predict ecological effects with much certainty. They can, however, contribute to support for environmental management decisions when used in concert with the other approaches described here.

Computer simulations have the considerable advantages of speed and flexibility; the significance of new assumptions or data can be tested almost immediately. The chief disadvantages of theoretical models, however, are the inability of models to include all relevant variables, and of modelers to describe intricate biological relationships in precise mathematical terms. Simulation techniques are most useful for predicting the spatial and temporal distribution, movement, transformation, and accumulation of substances in the environment. To the extent that these phenomena depend on well-understood physical and biological processes, properly designed modeling studies can predict with some confidence the exposures to various contaminants that organisms or spatial units in an ecosystem will encounter. The biological mechanisms of effects on populations or ecosystems, however, involve many variables and are much less well understood; consequently, predictions of the responses of whole ecosystems to environmental stresses are beyond the capability of most existing models (Levin 1974).

A model must account for the distribution of responses in both space and time in order to predict effects on whole ecosystems. Present models can simulate with some accuracy structural and functional changes in some subunits of ecosystems through time. The greatest need now is to develop models that can also incorporate the spatial variability (heterogeneity) of ecosystems.

Refinement of ecological models is a major objective of current research, and advances in ecosystem modeling would be very valuable for environmental decision making. If simulations of the spatial and temporal distribution of toxic agents in an ecosystem could be integrated with models identifying the locations and time scales of critical structural or functional properties of the system, the result would be a "map" that could point to particularly vulnerable, as well as especially resilient, elements of the system. Such predictions would need to be verified by field measurements, but models could assist that effort immensely by indicating what, where, and when to measure.

MONITORING EFFECTS ON ECOSYSTEMS

Results of experimental perturbations and predictions based on simulation models should be verified by monitoring programs at the ecosystem level, using critical indicator organisms, processes, or system characteristics.

The only reliable way of knowing whether deleterious changes are occurring in polluted ecosystems is to measure the status of systems in the field. Carefully chosen indicators of critical structural or functional elements of ecosystems (such as species diversity, nutrient cycling, energy flows, or primary productivity) should be monitored. In addition, some parameters should be measured on a scale larger than the local ecosystem. For instance, regional impacts on agricultural productivity, changes in water yield or water quality, or changes in the heterogeneity of wildlife habitat cannot usually be detected in small experimental plots, but rather might be anticipated through the combined knowledge gained from experimental perturbation and theoretical modeling, and then detected by field observation.

A great deal of past effort has been expended in gathering data (such as measurements of pesticide residues in soils, plants, animals, and humans) with no real sense of the possible ecological significance of the information being gathered. The usefulness of collected data will depend on the soundness of the theoretical basis for choosing particular indicators. If the status of selected organisms is to be used to assess effects on ecosystems, the organisms chosen should be valid representatives of critical functional guilds, such as primary producers, decomposers, herbivores, predators, and the like. If, on the other hand, the objective is to use wildlife as sentinels for potential environmental hazards to man, a sound theoretical basis must exist for drawing analogies between specific responses in those organisisms and risks to humans. In each case, it is critical to know the time delays between exposure to an environmental agent and the appearance of a detectable effect.

An example of an important theoretical consideration in selecting parameters to measure to assess ecosystem damage is the potential significance of impacts on organisms that rank low in numerical abundance in biological communities. Monitoring has generally focused on the most common, easily sampled representatives of a given functional guild. The resilience of an ecosystem depends to a large extent, however, on the ability of less numerous species to increase their populations to fill an ecological role if the population of the primary species occupying that role should decline. Thus, if an environmental contaminant were selectively toxic to the most abundant species, the impact on the system might be less severe than if it increased mortality randomly in all species within the functional group. It is important, therefore, to obtain information on the effects of an environmental change on some of the many numerically uncommon species, as well as on the more abundant ones, and to assess the impact on the biological community of mortality in relatively rare species.

Measurement of effects on the functions of ecosystems at the regional scale should be based on monitoring of important processes, as well as observations of effects on representative organisms. Some critical processes to monitor include the hydrologic cycle, the transfer of energy and production of biomass, the natural (biological) regulation of "pest" populations, and the cycling of nutrients and other elements. Research on one of these (nutrient cycles) is discussed in detail below as an example; but potential effects on any of these functions (and others not mentioned) deserve research attention.

Effects on Nutrient Cycles

A major, coordinated effort should be made to study the effects of pollutants on the cycling of elements in ecosystems.

One of the critical functional characteristics of ecosystems is the cyclic movement of nutrients, pollutants, and other elements among the atmosphere, soil, water, and living things. In both terrestrial and aquatic ecosystems, processes carried out largely by microorganisms synthesize, transform, and decompose the microchemical species of many stages of biogeochemical cycles. It is possible that pollutants or other stresses could, through effects on the environment itself or on the organisms involved, alter either the products or the rates of some of the myriad chemical and biological processes that make up cycles. Even small changes in the rates of a few critical processes could produce, over time, substantial shifts in the pools or rates of flow of important nutrients or of undesirable by-products or pollutants. For example, a change in the rate of denitrification in soils or aquatic systems could have adverse or beneficial impacts on productivity, and on the generation of nitrous oxide, a critical factor in the depletion of stratospheric ozone (Council for Agricultural Science and Technology 1976).

Our present understanding of potential effects of environmental contaminants on biogeochemical cycles is too crude to assess the resulting risks of significant long-term impacts on ecosystems or climate. Most investigations of ecological effects of pollution have not examined impacts on biogeochemical cycles or underlying microbial processes. One exception is the extensive body of research on effects of fertilizers and pesticides on soil organisms; but even in this well-studied field, systematic understanding of impacts on complex elemental cycles has proved to be elusive.

On the other hand, recent large-scale ecological research efforts such as the IBP have produced a substantial amount of basic knowledge about the ways important nutrient cycles function in a variety of terrestrial and aquatic systems. Some general principles have emerged, based on repeated empirical observations rather than on sophisticated theoretical constructs. For instance, it has been noted that, in general, undisturbed ecosystems tend to retain nutrients, while perturbed systems tend to lose nutrients (Bormann et al. 1974, Neuhold and Ruggiero 1977). Although it is not yet clear why this is so, this knowledge may make it possible to use rates of nutrient flow from a system as rough indicators of stress or injury to the system.

It would be very valuable, therefore, to make the study of effects on nutrient cycles a major focus of field investigations of the ecological consequences of pollution. Such effects should be examined both for their utility as a measure of general stress on ecosystems and to determine the mechanisms involved and the ecological significance of any shifts that might occur in cycles.

The biogeochemical cycles of nitrogen, carbon, and sulfur are among the most critical and best understood nutrient pathways, and would be the soundest initial choices for study. Cycles of other elements known to be either essential for life or particularly toxic should also be considered where feasible. Comparably designed studies should examine nutrient cycles in a variety of polluted terrestrial and aquatic systems. Some environmental perturbations that might be investigated include acid rainfall, heavy fertilizer use, pesticide applications, fly ash fallout, fluoride and heavy metal pollution, and sludge disposal. Some unstressed ecosystems should also be examined, as controls. Field studies should be supported where feasible with laboratory and sample-plot studies.

Effective study of a problem of this magnitude will require coordinated efforts involving microbiologists, ecologists, soil scientists, limnologists, and many other specialists. Studies may require five or ten years to obtain definitive results. A unifying overview and close coordination among many projects will be essential if such a program is to fulfill its promise.

INSTITUTIONAL ARRANGEMENTS

The kinds of research needed to predict and measure effects on biological communities and ecosystems are currently being carried out by many investigators in diverse specialties, located in universities, private research institutes, national laboratories, and federal agencies. Substantial support for ecological research has come through NSF (RANN), IBP, and other sources. Federal agencies, including EPA, ERDA, DOI, NOAA, USDA, and the Army Corps of Engineers have supported investigations of ecological problems of specific interest to them. No single agency, however, has taken the effective lead or coordinating role that is essential if ecological research is to be useful in making decisions on managing the environment.

Much of the research recommended here, such as improvements in theoretical ecology, is probably best carried out in universities and national laboratories, with support from traditional sources of funds for basic research in biology and ecology. Because of the importance of basic ecosystem research for environmental protection, the adequacy of current funding levels for this field should be carefully reviewed.

The results of most of the kinds of studies described above would be valuable and useful to EPA, ERDA, USDA, and a dozen or more other federal agencies that have responsibilities for managing land and water resources, and for preventing pollution and enhancing environmental quality. Some of the recommended research is short-term and decision-oriented, and would be appropriately conducted by EPA or other agencies, intramurally or extramurally. This category includes screening chemicals with laboratory microcosms, and using the most recent data and best available modeling techniques to evaluate potential ecological impacts of particular actions. Other work, such as studies of experimental perturbations of ecosystems or monitoring of nutrient cycles, will produce information of great value to a wider group of federal and state agencies, but only after a number of years. Commitment to support several such studies to completion would be difficult for any single agency, because the costs would be a considerable fraction of any agency's research budget, and most operate on a one-year funding system. This research would probably best be done and supported with the participation of several agencies, including EPA. Close coordination of such a program would be required, and might be achieved through a mechanism such as the new White House Office of Science and Technology Policy or the Office of Management and Budget. In order to make studies of ecological damage both coherent and relevant, such an overview and coordinating mechanism is essential.

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CHAPTER 8

EFFECTS ON MATERIALS

Practically all inanimate materials are subject to some deterioration caused by both normal components of the environment and contaminants of human origin. Deterioration of materials due to pollution is extensive and costly; so, however, is the damage caused by nonpollutants such as sunlight, cold, or mildew. It is extremely difficult to assign exact dollar costs to deterioration of materials (see Waddell 1974), but some estimates of pollution-linked damages range as high as $3.8 billion per year (Council on Environmental Quality 1975). The value may be inestimable when the materials damaged are irreplaceable monuments, works of art, rare books and manuscripts, and historic documents. Many materials affected by deterioration are also critical to national defense and to important manufacturing, transportation, and communication capabilities.

The most significant pollutants in terms of deterioration of materials are the acidic sulfur and nitrogen oxides, photochemical oxidants and ozone, hydrogen sulfide, some particulate substances, and a few other acids, bases, and salts. These may act individually, in combinations, or in concert with moisture, temperature changes, and other natural environmental variables.

INSTITUTIONAL ARRANGEMENTS

Responsibility for the coordination of research on the effects of environmental pollutants on materials should be assigned to a single federal agency.

Substantial research on the deterioration of materials and on the prevention of deterioration has been conducted by the federal government and by industry, especially since 1940. Government activity in this area was stimulated largely by World War II, and later by the space program. Within the federal government, materials research has been dispersed among the various agencies whose missions are adversely influenced by deterioration of materials, such as the Departments of Defense, Agriculture, Transportation, Housing and Urban Development, the National Aeronautics and Space Administration, the National Bureau of Standards, the National Archives, the Library of Congress, and the National Science Foundation. For more than 20 years, the National Academy of Sciences maintained a Prevention of Deterioration Center that included advisory and service groups (Greathouse and Wessel 1954), but it dropped the effort in 1965 because of insufficient financial support.[1]

The division of responsibility and consequent fragmentation of materials research has inevitably led to overlap in efforts and to inefficient use of available knowledge. In our judgment, the most urgent needs in the area of effects on materials are for improved coordination of research, and for more effective dissemination of existing knowledge. These needs should take priority over research on specific deterioration problems discussed below.

Lead responsibility for research on the effects of pollutants on materials should be assigned to a single federal agency, probably the National Bureau of Standards. The role of the lead agency need not include conduct of the entire research and development effort, but it should include coordination of programs of various agencies and operation of a system for gathering and distributing information on materials damage and its prevention. Monitoring the environment to locate and identify deterioration hazards would be a very important element of such a coordinated program, and might best be performed by EPA.

CORROSION OF METALS

Research is needed on the mechanisms of corrosion of metals and the development of protective measures.

Many metals, especially steels, and also some light metals such as aluminum, are susceptible to corrosion by acidic agents. Extensive research has been carried out for many years by government, industry, and research institutes on the causes, mechanisms, and costs of metal corrosion (see for example Fink et al. 1971, Gillette 1973). Despite this research, corrosion continues to exact its toll because of the continued use of susceptible materials, the lack of suitable protective measures, and a failure to apply available knowledge to prevent corrosion. The precise dollar cost of corrosion caused by environmental pollutants is unknown; but the total cost of corrosion is enormous, and prevention of even part of the corrosion due to pollutants would be a great step forward. There is therefore a sound economic incentive for additional federal government research on corrosion, to supplement that being done by industry. The needed research includes:

- studies of physical and chemical mechanisms of different forms of corrosion;

- development of improved protective systems, such as coatings and cathodic protection; and

- wider dissemination and use of present knowledge about preventive or corrective systems within government, industry, and other domestic users of susceptible metals.

DETERIORATION OF ORGANIC MATERIALS

Additional processes should be developed to prevent deterioration of critical organic materials.

Almost all organic materials are subject to some deterioration by environmental pollutants. Among the important materials affected are cotton, wool, paper, leather, plasticizers, polymers such as natural and synthetic rubbers, plastics and synthetic fibers, and protective and decorative coatings such as paints, enamels, varnishes and lacquers. Deterioration may take the form of bleaching, stiffening, cracking, loss of strength, peeling, or any other change that leads to disintegration and loss of ability to carry out the functions for which the materials were made or chosen.

Photochemical smog is particularly important in deterioration of organic materials. Ozone, organic peroxide nitrogen compounds, and oxides of nitrogen are the photochemical pollutants that do the most damage (Wessel 1972). A considerable amount of research has been performed on deterioration of organic materials, but the problem is still far from understood or ameliorated. Many of the materials susceptible to such damage are objects or articles whose useful lives are normally expected to be short. Nevertheless, prolonging the life of some of these materials by 50 to 100 percent would save many millions of dollars per year.

Many of the materials affected are used by private industry to make short-lived products for sale to the general public (Mueller and Stickney 1970, Gillette 1974). Some of the same materials are also used, however, in such items as textiles, plastics, paper products, and coatings, that are required in large quantities by the military. The problem of deterioration of organic materials used in national defense should be critically reviewed, in our judgment, so that federal research funds may be assigned appropriately to basic research in this critical area.

EFFECTS ON IRREPLACEABLE OBJECTS

Additional research should be done to prevent deterioration of irreplaceable national treasures such as monuments, works of art, rare books, manuscripts, and historic documents.

Deterioration of the organic materials used in most works of art, books, manuscripts, and historic documents is a problem of very large dimensions. In one notable example, about ten percent of the two million volume collection of the New York Public Library Reference Department has been damaged by pollution (Wessel 1972).

Rare paintings, tapestries, other objects of art, and books and manuscripts in all parts of the world must be carefully protected to preserve them for future generations (Plenderleith and Werner 1971). Research on conservation and preservation methods has been conducted for many years; in the United States the federal effort has centered mainly in the Library of Congress, the National Archives, and the Smithsonian Institution. This laudable work is expensive, and funds have been too limited to support an adequate research program. This research should be expanded, and additional funds should be made available to the responsible federal agencies for conservation and preservation programs based on protective measures that are developed.

Statues, monuments, and buildings composed of stone or other inorganic materials also may be susceptible to deterioration by environmental pollutants (Riederer 1971). Prolonged exposure to acidic pollutants can erode, crumble, or weaken the structure of many stone objects. The most effective way to alleviate further deterioration of this type would be to reduce levels of environmental pollutants. Until this is done, research is needed on methods of protection and restoration that can prevent the loss of such irreplaceable historic objects.

NOTE

1 The Departments of the Army, Navy, and Air Force supported the program from 1945 through 1965. The program concentrated on the investigation of chemical mechanisms for prevention of deterioration. Additional information is available in the Archives of the National Academy of Sciences.

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APPENDIX

WORKING PAPERS

The following papers were prepared by Panel members to support the work of the Panel on Effects of Ambient Environmental Quality of the Environmental Research Assessment Committee:

Timothy Atkeson	Proof of Effects in Environmental Law
William E. Cooper	Research Needs on Effects on Biological Communities and Ecosystems
A. Myrick Freeman III	Research Needs on the Economic Measurement of Effects
William B. House	Efficiency Considerations for Toxicological Research
Jay S. Jacobson	Research Needs on Effects of Environmental Quality in Agriculture and Forestry
G. D. Robinson	Research Needs on Effects on Weather and Climate

Lucille F. Stickel	Research Needs on Effects of Environmental Chemicals on Wildlife Populations
H. Eldon Sutton	Research Needs in Environmental Mutagenesis
Carl J. Wessel	Research Needs on Effects on Materials

These background papers are available in limited numbers upon request from the National Research Council's Environmental Studies Board, 2101 Constitution Avenue, Washington, D.C. 20418.