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EXECUTIVE OFFICE OF THE PRESIDENT
COUNCIL ON ENVIRONMENTAL QUALITY
OFFICE OF ENVIRONMENTAL QUALITY

# REPORT ON LONG-TERM ENVIRONMENTAL RESEARCH AND DEVELOPMENT

MARCH 1985

EXECUTIVE OFFICE OF THE PRESIDENT

COUNCIL ON ENVIRONMENTAL QUALITY

# REPORT ON LONG-TERM ENVIRONMENTAL RESEARCH AND DEVELOPMENT

MARCH 1985

EXECUTIVE OFFICE OF THE PRESIDENT
COUNCIL ON ENVIRONMENTAL QUALITY
722 JACKSON PLACE, N. W.
WASHINGTON, D. C 20006

March 18, 1985

Memorandum

To: CEQ Interagency Subcabinet Committee on Long-Term Environmental Research

From: A. Alan Hill, Chairman

Subject: Report on Long-Term Environmental Research and Development (R&D)

I am pleased to transmit the Report on Long-Term Environmental Research and Development (R&D) in which the findings and recommendations of four expert meetings held over the past year are summarized. Panelists at each meetings concentrated on a particular research area: human health impacts and their mitigation; geochemical and hydrologic processes and their protection; environmental impacts and their mitigation; and monitoring, assessment, and environmental management. A final meeting was convened at which panel chairmen and rapporteurs and Council on Environmental Quality (CEQ) and National Science Foundation (NSF) staff met to summarize topical conclusions of the respective panels and to identify overriding or cross-cutting long-term R&D issues.

This Report is the output of the chairmen's meeting. The overall effort was requested by the Environmental Protection Agency (EPA), and CEQ's preparation of the Report was substantially assisted by the Division of Policy Research and Analysis, NSF. Additional financial assistance was provided by the Department of Energy, the National Institute of Environmental Health Sciences, and the Nuclear Regulatory Commission. The Report contains an overview of the process, the chairmen's findings and recommendations, a list of participants and discussion topics, and summaries of each panel's respective discussions and recommendations.

# COUNCIL ON ENVIRONMENTAL QUALITY
# CONFERENCE ON LONG-TERM
# ENVIRONMENTAL RESEARCH AND DEVELOPMENT

CHAIRMEN'S REPORT

March 18, 1985

## EXECUTIVE SUMMARY

Scientists knowledgeable about human health and the quality of the natural environment recognize that manmade pressures placed upon natural resources, both living and nonliving, are more severe than previously suspected. Significant gaps in the existing scientific knowledge base cause or exacerbate acknowledged problems of environmental management. Long-term environmental and health research and development (R&D) is needed to resolve these scientific uncertainties, to establish baseline health and environmental parameters, to overcome lack of understanding of short-term variations in natural systems, and to identify long-term trends and relate them to their causes.

The Council on Environmental Quality (CEQ) convened a series of four two-day scientific and technical panel meetings to address specific clusters of long-term environmental R&D topics and to discern major scientific issues warranting enhanced government or private sector attention during the remainder of the century. This effort was requested by the Environmental Protection Agency (EPA) and was substantially assisted by the National Science Foundation (NSF). Additional financial assistance was provided by the Department of Energy, the National Institute of Environmental Health Sciences, and the Nuclear Regulatory Commission.

As panel chairmen, we caution that the long-term environmental and health R&D recommendations presented herein should not be viewed collectively as the comprehensive national R&D agenda for the remainder of the century; rather, they reflect those R&D issues to which substantial increased attention should be devoted. We recommend that the Federal government accord long-term environmental research heightened priority and a level of support sufficient that research findings will contribute to the attainment of broad National goals. This level of support need not require additional resources; it will require, however, a continuity in many existing research programs, a continuing commitment to scientific excellence, and an adequate institutional framework.

Principal long-term environmental R&D issues identified as warranting particular emphasis include the following:

- o Improving the quality and cost-effectiveness of physical, chemical, and biological monitoring programs to test scientific hypotheses about how environmental systems operate and interact;

- Continued progress in the use of molecular epidemiology techniques to detect and measure the interaction of foreign chemicals with easily accessible normal human constituents;

- Expanded research on geohydrological processes data at the soil/water/air/hazardous waste interface;

- Identification of genetic and other factors that account for individual differences in susceptibility to environmental agents;

- Development of underlying scientific principles for evaluating the toxicity of mixtures of chemicals;

- Evaluation of consequences of human or ecological exposure to chemical substances used in, or organisms that are chemical byproducts of emerging technologies;

- Expanded collection of 10-year observations of the background physical, chemical, and ecological variations in fresh waters, oceans, and the atmosphere;

- Determination of intermedia pollutant transfer rates for description and prediction of the subsequent fate of toxic chemicals in the environment;

- Examination of the behavior and biological effects of chemicals in environmental media;

- Characterization of the role of biological and physical/chemical linkages and processes in specific biogeochemical cycles;

- Identification of the biological interactions and ecosystem processes most sensitive to global pollutants and environmental stresses;

- Development of biological inventories and baseline studies of ecosystem structures, functioning, and linkages; and

- Improvement of the scientific basis for quantitative risk assessments, and of procedures for extending them to human health effects other than cancer, such as systemic toxicity.

# COUNCIL ON ENVIRONMENTAL QUALITY
# CONFERENCE ON LONG-TERM
# ENVIRONMENTAL RESEARCH AND DEVELOPMENT

## CHAIRMEN'S REPORT

March 18, 1985

## BACKGROUND

CEQ, at the request of the Environmental Protection Agency (EPA) and with assistance from the National Science Foundation (NSF), convened a series of four two-day scientific and technical panel meetings to address specific clusters of long-term environmental research and development (R&D) topics[1] and to discern major scientific issues warranting enhanced government or private sector attention during the remainder of the century. Overseeing this activity was an interagency subcabinet Committee on Long-Term Environmental Research (the Committee), chaired by CEQ Chairman A. Alan Hill. Support for the effort was provided by NSF, EPA, the Department of Energy, the National Institute of Environmental Health Sciences, and the Nuclear Regulatory Commission.

At each meeting, a panel of experts discussed a set of four substantively interrelated research topics and associated topic groupings, each of which was originated through consultation with Federal agencies. Panelists included public and private sector representatives, among whom was a rapporteur whose function was to provide a report summarizing the discussion. Background papers were prepared for each panel, and during the panel meetings the authors responded to questions. (Appendix I is a detailed listing of panel topics and subtopics.)

After all panel meetings had taken place, a fifth meeting was convened at which the panel chairmen, rapporteurs, and CEQ and NSF staff met to summarize the conclusions and recommendations of the respective panels and to identify overriding or cross-cutting long-term environmental R&D issues. This draft report is the output of that meeting. CEQ expects, following Committee review of this draft and appendices, to disseminate a report on long-term environmental R&D.

When published in final form, the CEQ report will include the background papers for the four panel meetings, rapporteur reports summarizing the discussions of each of the four panels, and this report, together with any agency comments received on the circulated draft.

## THE CONTEXT

Scientists knowledgeable about health and the environment recognize that manmade pressures placed upon natural resources, both living and nonliving, are more severe than previously suspected. There is mounting evidence, for instance, that potentially health-threatening groundwater contamination is a problem of increasing concern in the United States, and that toxic chemicals

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[1] As used here, long-term environmental R&D includes three kinds of activities: (1) anticipatory research, designed to identify potential environmental problems before they occur; (2) investigations of a continuing nature, such as ecological baseline studies or epidemiological studies, which may require a period of up to several decades to complete; and (3) fundamental research, the output of which may advance basic understanding of health- or environment-related processes.

in hazardous waste dumps and underground storage may pose serious health and environmental threats and create public anxiety. Analogous problems with toxic chemicals have been experienced with air and water and in the workplace.

During the past 40 years a wide variety of new synthetic chemicals have been introduced, some of which appear to pose serious acute and chronic health effects. Of similar concern is evidence of potential and perhaps irreversible damage to such important natural processes or properties as biogeochemical cycling or biologic diversity. Examples of possible changes in natural ambient levels that may be precursors of damage to health or ecosystems include the observed gradual increase in atmospheric carbon dioxide concentrations, suspected increases in atmospheric methane concentrations, possible increases in non-urban concentrations of ozone and carbon monoxide (current monitoring efforts detect primarily urban changes), observed increases in rates of species extinction in various parts of the world, and substantial ecosystem degradation in estuarine environments.

Ignorance about many scientific questions has resulted in acknowledged problems of environmental management, such as inappropriate regulation in the face of data uncertainties or heightened public anxieties. Long-term environmental and health research is needed to resolve scientific uncertainties, to establish baseline health and environmental parameters, to overcome lack of understanding of the short-term variations in natural systems, and to identify long-term trends and relate them to their causes.

For a variety of reasons, current incentives for private sector and governmental support of environmental and health R&D favor short-term approaches. Government agency research programs are generally designed to support mission goals of the agencies sponsoring them, resulting in relatively short-term research planning horizons that do not extend beyond immediate regulatory or programmatic require-ments. Similarly, corporate research efforts frequently support near-term product development strategies and are necessarily reflective of annual (or shorter) profit and loss statements. Current government and public concern over such environmental problems as acid deposition phenomena (acid rain) illustrates the fallacy in continued reliance on short-term research design. Although the potential environmental and health effects of acid particulates were pointed out years ago, relatively little research attention was devoted to following up on early studies noting these effects, and commitment of resources could not be justified on the basis of then-current regulatory strategies. Accordingly, long-term acid rain research programs were deferred; had they been undertaken a decade ago they might by now have been yielding information and predictive models of use to current regulators and policymakers.

We believe there is need for a greater resource commitment to and better direction, coördination, and interdisciplinary integration of long-term environmental and health R&D. Improvements in environmental management will flow from better characterization of environmental phenomena, increased understanding of basic mechanisms, and the development of more meaningful measures of hazard or harm assessment. Our panelists noted the need for good long-term monitoring data and accompanying quality assurance to evaluate models used for understanding processes and environmental trends. They believe that lack of validated monitoring time series data, based upon even crude health and environmental measures, has impeded the expansion of fundamental research programs. Further, they believe, the piggybacking of monitoring onto ongoing research activities may well provide useful information at little marginal cost.

We believe that modeling can be an integrating force for the environmental research community, in that the imposition of modeling requirements yields helpful insights in identifying needs and opportunities for new research. However, the use of models must be accompanied by continuing efforts to validate them. For example, in constructing quantitative risk assessment (QRA) models, it becomes clear that the unknowns in the QRA can only be satisfied by continued basic research on biochemical mechanisms. Efforts to fill modeling gaps can, in turn, provide help in determining the need for and priority of data collection activities.

General acceptance of these observations led to a consensus that the current institutional framework and organization of government resources may not assure that needed long-term research projects can be started, continued, and completed with adequate funding on a multiyear basis. Only with research continuing over many years, and for projects that extend over a substantial period of time and that are focused on fundamental issues, will the Nation be able to develop the credible and necessary expertise to better rationalize environmental management policies. Environmental science needs a critical mass of talent and resources to effectively approach the challenge of understanding complex environmental and health phenomena. The need to integrate data gathering, modeling, and environmental impact assessment and QRA efforts may require the establishment of centers of excellence, at which this critical mass of talent and resources can be assembled.

Over the long term, environmental science may provide answers to such unresolved questions as the effects of subtle changes on ecosystems, the effects of new technologies such as biotechnology and microelectronics on human health, and the effects of global contaminants on ecological processes.

## CHAIRMEN'S RECOMMENDATIONS

### Monitoring

Monitoring yields essential current and time series information on the status of environmental systems, information used both for environmental management and for regulatory compliance. Information gained from monitoring forms the basis for the testing of scientific hypotheses about how environmental systems operate and interact. Panelists believe that the lack of coordinated scientific resources providing institutional sophistication in environmental monitoring, modeling, biostatistics, and ecostatistics account for such problems as nonstandardization of monitoring practices and failure to monitor for parameters of greatest importance or relevance.

In the absence of validated data, modeling is frequently the only source of guidance to environmental managers for describing or predicting complex environmental events. Invariably one or more elements of necessary environmental data are missing, either because we lack the understanding or tools to measure the phenomena or because the cost of measurement is prohibitive. To the extent that monitoring data necessary for model-building are not validated or not reliable, models lack predictive value. Environmental models also frequently suffer from over complexity or from lack of standardization.

In spite of significant efforts expended on the collection of environmental data by a variety of unrelated and uncoordinated state and Federal agencies, no adequate system exists for the centralized collection, storage, maintenance, and quality control of such data. We recommend that CEQ foster an evaluation by an appropriate organization of existing physical, chemical, and biological monitoring programs (and extant data associated with them) to identify and stimulate research and development on improving the quality and cost-effectiveness of monitoring programs. Particular emphasis should be placed on determining requirements for biological and environmental monitoring, on identifying pollutants such as toxic chemicals for sampling, and on determining information and statistical requirements for environmental models.

### Institutional Capability

There is an evident disparity between the current mix of institutional arrangements and resource availability to design, conduct, and manage long-term environmental research, on the one hand, and the long-term research needs identified herein, on the other. The establishment of well-funded "centers of excellence" at existing universities, such as those now supported by the National Institutes of Health and by EPA, was discussed as a possible means to redress this imbalance. The National Institute of Environmental Health Sciences represents one example of a governmental institution that has successfully supported integrated research over a long term. Another example is the Long-Term Ecological Research Program supported by NSF's Division of Biotic Systems and Resources. Laboratories owned and operated by EPA, where the research staff are government employees, provide another model. Research laboratories operated by the Federal government on contract to universities, university-based consortia, or industry-based research organizations--such as the National Center for Atmospheric Research, funded by NSF, or the national laboratories supported by the Department of Energy--provide still a third model, one at which government-sponsored research is funded on a multiyear basis.

To supplement government support, we recommend that attention be given to seeking sources of environmental and health research funding and ideas from the private sector to the extent possible, and that effort be given to coordinating government-sponsored research with research performed or supported by industry-specific organizations such as the Electric Power Research Institute, the Health Effects Institute, the Chemical Industry Institute of Toxicology, and the Gas Research Institute.

Molecular Epidemiology and Exposure Estimation

Molecular epidemiology is based upon measurement in the exposed individual of the interaction of a toxic chemical (or its derivative) with a tissue constituent or a tissue alteration resulting from exposure to the chemical. It thus provides an indirect measure of individual exposure.

There has been significant recent progress in refining means for detecting and measuring the interaction of foreign chemicals with easily accessible normal human constituents, such as chemical carcinogen interactions with deoxyribonucleic acid (DNA). It is now sometimes possible to detect a few altered DNA molecules out of the millions in a cell. Along with other developments for detection of altered chromosomes in human cells, these refinements mark important advances in our ability to detect and quantitate human exposure to foreign chemicals. Potential benefits from molecular epidemiology are so great as to justify an expansion of the national molecular epidemiology research effort. This research may eventually yield accurate markers of exposure, and information on the relationship between exposure and adverse health outcomes.

Hazardous Waste Sites

Panelists indicate that substantial geohydrological processes data gaps exist at the soil/water/air/hazardous waste interface, and that very little is known or fully understood about underlying processes. Current hazardous waste site cleanup actions are routinely commenced in the absence of knowledge about how chemicals move through the soil, how they are transformed, and how modeling can successfully be used to predict air and water contamination. We recommend that government agencies, particularly EPA and DoD, integrate well-defined research studies into plans for measurement and remedial action programs at selected hazardous waste sites.

Genetic Diversity/Susceptibility and Biological Mechanisms

Susceptibility to chemical toxicants varies widely in human host populations. Such host factors as genetic diversity, current or prior disease, sex, and age contribute to this variation. There are approximately 2,000 genetically identifiable human diseases, and there are firm theoretical foundations for the hypothesis that genetic conditions are likely to enhance (sometimes dramatically) the risk to affected individuals of developing environmentally or occupationally associated adverse health effects; however, the extent of risk enhancement is currently unknown.

It is recognized that identification of different susceptibilities as a result of genetic differences may generate or exacerbate social problems. Although this variation creates difficulties for the research establishment, it should not be allowed to inhibit scientific inquiry. Many of the differences in susceptibility derive from major differences in the way individuals alter (intensify or decrease) chemical toxicity through their different enzymes (active body constituents that alter foreign chemicals). We recommend that a research effort be mounted to identify genetic and other factors important for individual differences in susceptibility to environmental agents, to assess the health impacts of such differences, and to examine the means (such as differences in enzymatic activity) by which these differences become manifest.

Mixtures

Exposures of humans, other animals, and the natural environment to pollutants rarely involve single substances. Human and nonhuman organisms are generally exposed to mixtures of chemicals, whether from waste dumps, contaminated water, or ambient or workplace air. When single agents can be identified as of predominant importance, the problem is simplified. Often, however, this is not the case. Regrettably, there are no valid general rules for determining the presence of synergism in inhibitory action, and any study requiring a mixture by mixture analysis would be impossibly complex. We recommend, therefore, a major effort to define underlying general scientific principles for evaluating the toxicity of mixtures of chemicals.

Anticipating the Impacts of Emerging Technologies

Two emerging technologies--biotechnology and microelectronics--were examined. The first deals with the new field of genetic engineering, which promises to yield more efficient or new medicinal products, agricultural products, chemicals, and other products. The second involves the production of microelectronic chips with the use of a variety of virtually unstudied (exotic) chemicals, such as gallium arsenide, silicon, and halogenated hydrocarbon solvents. We recognize that there may be health and environmental consequences associated with emerging technologies, and we therefore recommend that these possible consequences be evaluated.

Fundamental Research in Fresh Water, Ocean, and Atmospheric Cycles

We lack 10-year observations of the background physical, chemical, and ecological variations in fresh waters, oceans, and the atmosphere that are adequate to distinguish natural changes from perturbations attributable to human activities. We lack continental-scale background information on historic chemicals, or even on currently important chemicals in the environmental, particularly coastal waters and the atmosphere. We are also ignorant of the normal range of variations through which ecosystems and their components pass. In addition to obtaining better information on background concentrations or biological variation, we also need to examine the processes that account for ecological variations and how pollutants interfere with those processes.

We recommend expanded collection of 10-year observations of these phenomena to better enable us to distinguish natural fluctuations from those caused by anthropogenic activities, at study sites selected to permit simultaneous observation of contaminated and close-by or ecologically similar uncontaminated areas. For study of coastal waters, several estuaries have been identified as receiving or having received significant amounts of pollutants; the fates of these pollutants should be continuously monitored. We recommend that long-term continental-scale and global-scale baseline atmospheric measurements be gathered for polluting gases such as ozone, nitrous oxides, sulfur dioxide, and hydrocarbons, and for particulates. These pollutants should be studied to determine their effects on climate, visibility, and variables that affect human health and the quality of life. We also recommend an expanded research effort to more effectively describe the transport and transformation of toxic organic compounds with respect to their physical, chemical, and biological effects in the subsurface soil and groundwater environments. Subsurface and groundwater research activities should be focused on better understanding of the interactions of groundwater with the solid phases of rocks and soils; on developing a quantitative understanding of the variation of the sorption and vapor pressure of organic compounds with soil moisture, particularly below 10-percent soil water content; and on the fundamentals of hydraulics and geochemistry for low-permeability materials. Given the expense of groundwater monitoring, research could most efficiently be coordinated with existing compliance monitoring activities.

Intermedia Transfer

Pollutants move from one receiving medium to another, and their concentrations in any given reservoir depend on the rates of these transfers. Predictions of pollutant distribution within the environment are based on quantitative assessment of these movements. At present, quantitative rate measurements of these intermedia transfers are still rudimentary.

We recommend studies to determine intermedia transfer rates for both description and prediction of the subsequent fate of toxic chemicals in the environment. Of particular interest are aerosol formation rates of chemically reactive organic pollutants and the effects of surface active agents on the air/water interface, precipitation scavenging of aerosols in the field, dry deposition as a function of topological (surface) roughness, volatilization and adsorption as a function of temperature and moisture content, and the deposition and resuspension of sediments. Determining the chemical composition of aerosol particles and the size distribution of suspended solids in the liquid phase will also help in understanding these transfer rates. We further recommend that the results of intermedia monitoring and modeling studies be used to develop screening procedures to determine under what environmental conditions and with which chemical substance characteristics it is necessary to consider intermedia transfers. We also recommend linking intermedia transport processes to the biological effects observed in receptor organisms affected by exposure to the transferred chemicals; monitored receptor organisms should include humans, commercially important species, and whole ecosystems. Finally, we recommend that anticipatory research on the intermedia nature of pollutant transfer be used in the consideration of the respective cost efficiencies of alternative multimedia control strategies. For instance, the Toxic Substances Control Act, with proposed or hypothesized amendments, should be evaluated as a basis for regulating toxic and hazardous wastes by means of multimedia control strategies.

Assimilative Capacity

In the United States billions of tons of solid waste (of which a small percentage can be designated as hazardous) are released to the land, air, surface waters, and groundwaters. Wastes also move from one medium to another and ultimately reside or degrade in various media. Assessments must be made of the cross media risks associated with waste treatment and disposal practices. Each environmental medium, and the biological communities that reside within it, is assumed to have a site-specific set of responses to different levels of waste loadings. Various wastes differ on many characteristics including toxicity, mobility, degradation rates, and their ability to be recycled or otherwise treated. Presumably some amount of certain wastes may be assimilated by the different media without adverse impacts on human health and environmental quality. Identification of the lower limits at which public health, ecosystem integrity, or the most sensitive species populations are affected is crucial to a determination of environmental quality.

Although we recognize that considerable research has been done in some of these subject areas, we recommend that research be undertaken to determine the behavior and biolgical effects of chemicals in environmental media as a basis for setting levels of release for protection of human health and environmental quality. In addition, the determination of these levels should include a consideration of such changes in the physical environment as global warming, decreases in atmospheric visibility, and their impact during extremes of temperature and moisture encountered after one- to five-decade levels of variations.

Global Biogeochemical Cycles

Global biogeochemical cycles are essential to the maintenance of the biosphere. Continuous interaction between biotic and abiotic components of the Earth distinguishes it from other planets and, thus, the term biogeochemical cycling. We recommend the conduct of long-term fundamental studies to characterize the role of biological and physical/chemical linkages and processes in specific biogeochemical cycles (for example, in carbon, nitrogen, phosphorus, sulfur, and oxygen cycles), as well as in their interactions with each other and with climate to build on the results of previous research.

Global Pollutants and Impacts on Ecological Processes

Significant gaps in the environmental sciences knowledge base reflect our ignorance of cause/effect relationships between global pollutants and ecological processes. We recommend the conduct of long-term studies to identify the biological interactions and ecosystem processes that are most sensitive to specific past, present, and anticipated global pollutants and environmental stresses. The outputs of such studies might be helpful in clarifying the impact of pollutants on the total biosphere, and on global chemical processes.

Fundamentals of Ecosystem Structures and Processes

In order to identify those ecological phenomena that are related to the sustainability of ecosystem resources, and especially to the conservation of biological diversity, there is a need for long-term biological inventories and baseline studies of ecosystem structures and functioning. Especially needed are studies of processes that link different ecosystem types (for example, suburban/urban, agricultural/aquatic, industrial/agricultural linkages). We recommend the identification of ecosystem sites that can be studied and the establishment and maintenance of a national network of representative ecosystem sites, both those that are relatively undisturbed and those that are intensively managed.

## Quantitative Risk Assessment

Quantitative risk assessment (QRA) has come to play a major role in regulatory processes and in risk management generally. Current QRA techniques are generally acknowledged to be crude and to lack the precision commensurate with the importance attached to the regulatory decisions frequently based on them, decisions that may be of great social and economic consequence.

Scientists now believe that improvements in QRA techniques are more likely to come from better understanding of biological processes than from improvements in mathematics. Far too little is now known, for instance, about basic pharmacodynamic and environmental mechanisms of toxic effects, about virtually any health (other than cancer) or ecological effects, and about actual exposure patterns; and about the potential of short-term biological screening to provide early prediction of effects. Further studies are needed on the independent validation of risk assessments, better understanding of decision processes, identification of scientific principles used in selection of assumptions and criteria, determination of the criteria used as a basis for risk assessment priorities, and improving the understanding of risks. We recommend that particular research attention be given to improving the scientific basis for risk assessments, and to procedures for valid extension to human health effects other than cancer, such as systemic toxicity.

COUNCIL ON ENVIRONMENTAL QUALITY

CONFERENCE ON LONG-TERM
ENVIRONMENTAL RESEARCH AND DEVELOPMENT

CHAIRMEN'S REPORT

# APPENDIX I

## PANEL TOPICS AND PANELISTS

| Panel Subject | Topic Subjects |
|---|---|
| HUMAN HEALTH IMPACTS AND THEIR MITIGATION | Human Physiologic/Genetic Diversity<br>Molecular Epidemiology<br>Monitoring of Effects of Exposure to Health Hazards<br>Human Health Impacts of Current and Emerging Technologies |
| GEOCHEMICAL AND HYDROLOGIC PROCESSES AND THEIR PROTECTION | Surface Water/Groundwater Processes and Pollution<br>Land/Soil Processes and Pollution<br>Atmospheric/Oceanic Processes and Pollution<br>Multimedia Toxic Substance/Hazardous Waste Research |
| ENVIRONMENTAL IMPACTS AND THEIR MITIGATION | Global/Biosphere Impacts<br>Local Ecological Impacts<br>Ecological Diversity<br>Environmental Impacts of Current and Emerging Technologies |
| MONITORING, ASSESSMENT, AND ENVIRONMENTAL MANAGEMENT | Data Generation, Collection, Analysis, and Interpretation<br>Risk/Impact Assessment Techniques<br>Modeling and Forecasting Techniques<br>Environmental Management Approaches |

## HUMAN HEALTH IMPACTS AND THEIR MITIGATION

### Human Physiologic/Genetic Diversity

Topics

"Priority Needs in the Development of Epidemiological Techniques That Account for Human Physiologic and Genetic Variability"
Author: Dr. Mark Skolnick, University of Utah Medical Center

"The Roles of Human Physiologic and Genetic Variability in the Development and Expression of Pollutant-Related Diseases"
Author: Dr. Elliot S. Vesell, Pennsylvania State University

### Molecular Epidemiology

Topics

"Novel Approaches to the Investigation of Pollutant-Related Disease: Molecular Epidemiology"
Author: Dr. Frederica P. Perera, Columbia University

"The Value of Molecular Epidemiological Techniques in Quantitative Health Risk Assessment"
Author: Dr. Dale Hattis, Massachusetts Institute of Technology

### Monitoring of Effects of Exposure to Health Hazards

Topics

"Problems in Demonstrating Disease Causation Following Multiple Exposures to Toxic or Hazardous Substances"
Author: Dr. Robert Dixon, U.S. Environmental Protection Agency

"The Value of Health Registries in Monitoring Pollutant-Related Disease"
Author: Dr. Paul Schulte, National Institute for Occupational Safety and Health

### Human Health Impacts of Current and Emerging Technologies

Topics

"Improved Methods for Discerning Health Impacts of Current Technologies"
Author: Dr. James M. Robins, Harvard University

"Anticipating the Potential Health Impacts of Exotic Chemicals Associated with Emerging Technologies"
Author: Dr. Daniel T. Teitelbaum, Denver Clinic Medical Centers

"Anticipating the Potential Health Impacts of Emerging Technologies: Biotechnology"
Author: Dr. Daniel Liberman, Massachusetts Institute of Technology

## HUMAN HEALTH IMPACTS AND THEIR MITIGATION


Chairman

| | |
|---|---|
| Dr. Norton Nelson | New York University Medical Center |

Rapporteur

| | |
|---|---|
| Dr. Edward J. Calabrese | University of Massachusetts |

Panelists

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| Dr. Manning Feinleib | National Center for Health Statistics |
| Dr. Perry J. Gehring | Dow Chemical Company |
| Dr. Bernard Goldstein | U.S. Environmental Protection Agency |
| Dr. Irving Johnson | Eli Lilly and Company |
| Dr. James L. Liverman | Litton Bionetics |
| Dr. Brian MacMahon | Harvard School of Public Health |
| Dr. Mortimer L. Mendelsohn | Lawrence Livermore Laboratory |
| Dr. David P. Rall | Nat'l. Inst. Environ. Health Sciences |
| Dr. Ellen K. Silbergeld | The Environmental Defense Fund |

# GEOCHEMICAL AND HYDROLOGIC PROCESSES AND THEIR PROTECTION

## Surface Water and Groundwater Processes and Pollution

Topics

"Improving the Knowledge Base on Surface Water and Groundwater"
Author: Dr. Wayne A. Pettyjohn, Oklahoma State University

"Protection of Surface Water and Groundwater Resources"
Author: Dr. Nathan Buras, University of Arizona

## Land/Soil Processes and Pollution

Topics

"Identifying Knowledge Gaps on Land/Soil Processes: Hazardous Substances and the Land/Soil Resource"
Author: Dr. Louis J. Thibodeaux, Louisiana State University

"Advancing Knowledge on Protection of the Land/Soil Resource: Assimilative Capacity for Pollutants"
Author: Dr. Raymond C. Loehr, University of Texas

## Atmospheric/Oceanic Processes and Pollution

Topics

"Priority Needs for Improving the Knowledge Base on Atmospheric Processes"
Author: Dr. Stephen Wofsy, Harvard University

"Advancing Knowledge of Pollutant Effects on Coastal Ecosystems"
Author: Dr. John Teal, Woods Hole Oceanographic Institution

## Multimedia Toxic Substance/Hazardous Waste Research

Topics

"Scientific, Legislative, and Administrative Constraints to Multimedia Control of Toxic Substances and Hazardous Wastes"
Author: Leslie Sue Ritts, Esq., Morgan, Lewis and Bockius

"Modeling of Pollutant Transport in a Multimedia Environment"
Author: Dr. Yoram Cohen, University of California

## GEOCHEMICAL AND HYDROLOGIC PROCESSES AND THEIR PROTECTION

### Chairman

| | |
|---|---|
| Dr. Edward D. Goldberg | Scripps Institution of Oceanography |

### Rapporteur

| | |
|---|---|
| Dr. Michael S. Connor | U.S. Environmental Protection Agency |

### Panelists

| | |
|---|---|
| Dr. Keros Cartwright | Illinois Geological Survey |
| Professor Henry P. Caulfield, Jr. | Colorado State University |
| Dr. Michael A. Champ | U.S. Environmental Protection Agency |
| Dr. Robert A. Duce | University of Rhode Island |
| Dr. Bruce A. Egan | Environmental Research and Technology, Inc. |
| Ms. Frances H. Irwin | The Conservation Foundation |
| Dr. Charles L. Osterberg | U.S. Department of Energy |
| Dr. Frederick G. Pohland | Georgia Institute of Technology |

## ENVIRONMENTAL IMPACTS AND THEIR MITIGATION

### Global/Biosphere Impacts

Topics

"Maintaining the Integrity of Global Cycles: Nutrient Elements, Water, and Energy"
Author: Dr. William R. Emanuel, Oak Ridge National Laboratory

"On Toxins and Toxic Effects: Guarding Life in a Small World"
Author: Dr. George M. Woodwell, The Ecosystems Center

### Local Ecological Impacts

Topics

"The Role of Basic Ecological Knowledge in Environmental Assessment"
Author: Dr. Stephen G. Hildebrand, Oak Ridge National Laboratory

"The Role of Basic Ecological Knowledge in the Mitigation of Impacts from Complex Technological Systems: Agricultural, Transportation, and Urban Systems"
Author: Dr. Orie L. Loucks, Holcomb Research Institute

### Biological Diversity

Topics

"Habitat Diversity and Genetic Variability: Are They Necessary Ecosystem Properties?"
Author: Dr. Elliott A. Norse, The Ecological Society of America

"Increasing the Precision of Indicators of Change in Natural and Man-Impacted Ecosystems"
Author: Dr. Frieda B. Taub, University of Washington

### Environmental Impacts of Current and Emerging Technologies

Topics

"Improved Methods for Discerning the Environmental Impacts of Current Technologies"
Author: Dr. Nicholas Clesceri, Rensselaer Polytechnic Institute

"Anticipating the Potential Environmental Impacts of Emerging Technologies"
Author: Dr. Martin Alexander, Cornell University

## ENVIRONMENTAL IMPACTS AND THEIR MITIGATION


**Chairman**

| | |
|---|---|
| Dr. John E. Cantlon | Michigan State University |

**Rapporteur**

| | |
|---|---|
| Dr. Lev R. Ginzburg | State University of New York |

**Panelists**

| | |
|---|---|
| Dr. John W. Firor | National Center for Atmospheric Research |
| Dr. Marvin B. Glaser | Exxon Research and Engineering Company |
| Dr. Robert C. Harriss | Nat'l. Aeronautics and Space Admin. |
| Dr. Dean L. Haynes | Michigan State University |
| Dr. Francis C. McMichael | Carnegie-Mellon University |
| Dr. Joseph F. Malina, Jr. | University of Texas |
| Dr. John M. Neuhold | Utah State University |
| Dr. J. C. Randolph | Indiana University |

# MONITORING, ASSESSMENT, AND ENVIRONMENTAL MANAGEMENT

## Data Generation, Collection, Analysis, and Interpretation

Topics

"Development of Environmental Data Bases and Inventories"
Author: Dr. Don W. Hayne, North Carolina State University

"Improving Methods of Data Analysis and Interpretation for Environmental Management Programs"
Author: Dr. Richard E. Sparks, Illinois Natural History Survey

## Risk/Impact Assessment Techniques

Topics

"Improving Quantitative Health Risk Assessment Techniques"
Author: Dr. Lester B. Lave, Carnegie-Mellon University

"Improving Qualitative and Quantitative Environmental Assessment Techniques to Support Environmental Management"
Author: Dr. Peter E. Black, State University of New York at Syracuse

## Modeling and Forecasting Techniques

Topics

"Long-Term Research on Simulation Models Applied to Environmental Management"
Author: Dr. Gordon L. Swartzman, University of Washington

"The Role of Forecasting Methodologies in Approaches to Environmental Management"
Author: Dr. John F. Ficke, The IT Corporation

## Environmental Management Approaches

Topics

"The Role of Interdisciplinary Environmental Research in Natural Resource Management"
Author: Dr. David Pimentel, Cornell University

"Innovative Environmental Management Approaches for Cost-Effective Control of Industrial Residuals"
Author: Richard A. Ferguson, Esq., The Skylonda Group, Inc.

## MONITORING, ASSESSMENT, AND ENVIRONMENTAL MANAGEMENT

### Chairman

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| Dr. Richard M. Dowd | R. M. Dowd & Co. |

### Rapporteur

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|---|---|
| Dr. Richard N. L. Andrews | University of North Carolina |

### Panelists

| | |
|---|---|
| Dr. A. Karim Ahmed | Natural Resources Defense Council |
| Dr. Bruce N. Bastian | Shell Oil Company |
| Dr. Edward J. Burger | Georgetown University Medical Center |
| Dr. James W. Gillett | Cornell University |
| Dr. Raphael G. Kasper | National Research Council |
| Dr. Franklin E. Mirer | United Auto Workers |
| Dr. Glenn W. Suter | Oak Ridge National Laboratory |
| Dr. Milton Russell | U.S. Environmental Protection Agency |
| Dr. Jaroslav J. Vostal | General Motors Research Laboratories |

APPENDIX II

# Summary Report
# of the
# Expert Panel Meeting
# on
# Human Health Impacts and Their Mitigation

## INTRODUCTION

The meeting on Human Health Impacts and Their Mitigation was included in the series of expert panel meetings because the preservation and protection of public health and well-being are the ultimate goals of environmental management strategies practiced by governments and by the private sector. The four topic areas examined in this meeting were:

- Human Physiologic and Genetic Diversity;
- Molecular Epidemiology;
- Monitoring of Effects of Exposure to Health Hazards; and
- Human Health Impacts of Current and Emerging Technologies.

## THE CONTEXT

The intent of this meeting was the development of consensus judgments on long-term environmental R&D needs in the topic areas mentioned above. The expert panelists agreed that development of such consensus judgments depended on clear statement of the problem at hand. We stated our sense of the problem in the form of four critical questions:

- What are the major knowledge gaps related to the maintenance of good health and prevention of disease in the topic areas examined?
- What research can be identified as promising to fill the identified gaps and reduce the likelihood of future disease and injury?
- What precedent steps may be required in order to make possible research more directly relevant to the knowledge sought? and
- What are the priorities among the R&D recommendations so assembled?

Specific suggestions with respect to these four questions were contained in the background papers for each topic area examined. In addition, we were asked to address the generic question of how the necessary R&D might be done. We considered the processes of funding support and institutional mechanisms, namely, intramural and extramural funding, grants, contracts, cooperative agreements, and other special support mechanisms. We stress the importance of interdisciplinary collaboration and the desirability of incentives for better connections among investigators in laboratory sciences, clinical research, and epidemiology. Such collaborations are especially desirable if scientists are to fully benefit from new and emerging basic research in genetics and molecular epidemiology.

We were also asked to comment on where the responsibilities lie between the public and private sectors for long-term research (both environmental health research and environmental research) and to comment on the institutional framework for the R&D activities proposed in this review. Our response to these issues is general and rarely prescriptive. Research in pursuit of long-term goals must of necessity be concerned with fundamental questions of science. As an example, the role of oncogenes is an important current topic in the evaluation of human carcinogenesis, but it would have been impossible to approach without the preceding extensive basic research leading to an understanding of deoxyribonucleic acid (DNA) structure and function.

Exposure, Dose, Effects, and Endpoints

Throughout our discussion, we searched repeatedly for a suitable schematic to outline our notions of mechanism. The starting point rests on the four rough stages at which environmental toxicological processes can be observed:

- Exposure--the ambient condition bringing the organism into contact with the hazard (for example, toxicant concentrations in air or water);

- Dose--the internalized quantity of the hazard (for example, serum concentration, body burden, or amount of a toxicant in a particular organ or cell);

- Effects (markers)--intermediate biological effects that are either a step in the toxicologic process or a parallel manifestation of effect; or effects that are useful as indicators of exposure or dose, as predictors of toxicity, and for studies of mechanism (for example, chromosome aberrations, mutation, cell killing, enzyme activity); and

- Endpoint--the ultimate toxicologic effect (for example, a disease such as cancer, heart disease, or early mortality or morbidity).

Between each two of these stages there are intermediate stages or processes. Thus between exposure and dose there is entrance of the agent into the organism, absorption, distribution, and--depending on the circumstances--metabolic activation and deactivation, and excretion. Between dose and intermediate biologic effect there may again be the various pharmacologic processes that bring the agent to the effector site and thus activate it into its effective form. Also there are various mechanisms that may cause lesions, such as DNA or protein adducts, that repair lesions, and that cause lesions to progress to an observable biological response. Between the intermediate or surrogate biological response and the ultimate disease there are numerous and generally unknown steps in the toxicological process.

At each stage and for many of the transition processes there may be markers of what is going on; that is, observable chemical or biological properties that can serve as markers. Examples are numerous and include, using cancer for illustration,

- Monitors of ambient concentrations;
- Personal exposure devices;
- Tracer, experimental, or trial measurement of uptake, activation, excretion, or transport;
- Measurement of adducts in cells or organs, or of free chemical or chemical products in body fluids;
- Measurement of actual DNA repair or of repair capacity;
- Characterization of the metabolic profile of an individual or group;
- Assay of gene mutation, chromosome aberration or sister-chromatid exchange;
- Assay of cell transformation;
- Precancerous markers, and the analogous markers for other disease processes;
- The full battery of clinical laboratory tests for early disease or abnormality; and
- Screening methods for disease detection.

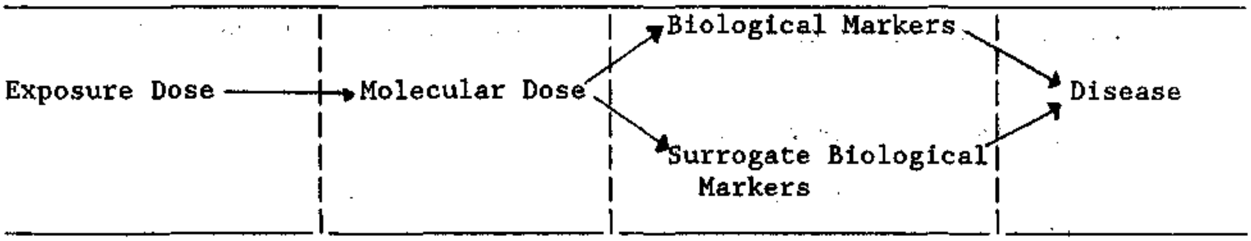
The Panelists are in general agreement about these concepts, and more detail for this kind of reasoning can be found in the background papers by Perera, Hattis, and Dixon. We cannot, however, reach consensus on a common terminology, in part because of a priori preferences for particular terms, and in part because several of the stages are conceptually broader than any of the terms on which the Panel could agree. Some choices of terms are summarized in Figure 1. Panelists are agreed that detection capability is greatest at the "exposure" end of the continuum and weakest at the "clinical" (disease) end.

FIGURE 1

Continuum From Exposure to Endpoint

| Exposure → | Dose → | Effects/Response → | Endpoint |
|---|---|---|---|
| Exposure Dose | Internal Dose<br>Biologically Effective Dose<br>Molecular Dose | Preclinical Response<br>Biological Dose<br>Surrogate Biological Dose<br>Biological Marker<br>Exposure Indicator | Disease<br>Clinical Effects |

The preferred (but not unanimous) version of our schematic is as follows:




## Physiologic and Genetic Diversity

Susceptibility to chemical toxicants varies widely in human populations. Such differences have not always been adequately taken into account in the study of environmentally related health impacts. Many host factors, such as current or prior disease, sex, age, and genetic diversity, contribute to differential susceptibility to chemical toxicants.

There are approximately 2000 genetically identifiable human diseases, as well as numerous genetic modulators of biochemical, physiological, and immunological human responses. Some genes are known to enhance individual response to environmental agents, but for the most part, whether and to what extent genetic factors predispose persons to, or protect them from, environmentally-induced disease is not known. Developing research methodologies in the field of genetic epidemiology offer considerable promise in assessing the associations posed above.

Techniques such as the use of DNA probes as genetic markers can lead to the development of an accurate genealogical assessment of how a gene is distributed in a population. This has the advantage of identifying persons who could serve as part of a cohort for a subsequent epidemiologic study to quantify the extent to which particular genetic constitutions may be at increased risk to environmental agents such as mutagens and carcinogens. This technique offers a clear advance over present approaches, which are limited to using genetic probabilities to estimate the relationships between genes and delayed effects.

There are excellent theoretical foundations to support the hypothesis that genetic factors are likely to enhance the risk of affected individuals developing environmentally or occupationally induced adverse health effects. However, the extent to which supporting data are available from studies with appropriate predictive animal models or human epidemiologic studies is highly variable. Examples of likely mechanisms are heritable DNA repair deficiencies, such as ataxia telangiectasia and xeroderma pigmentosa and metabolic variants such as glucose-6-phosphate dehydrogenase (G-6-PD) deficiency, acetylation capacity, and spectra of mixed function oxidase capacities.

Of the human genetic conditions identified as potentially enhancing susceptibility to environmental and occupational toxins, only G-6-PD deficiency leading to hemolysis has a demonstrable causal history of enhancing susceptibility to certain industrial pollutants. Even in this instance, there is a lack of precise dose-response relationships. No adverse effects have ever been adequately related causally to the statistical association between levels of pollutant exposures and the extent of hemolysis occurrence. In addition, the role of potential confounding variables, such as nutritional and overall health status, has never been adequately assessed. The lack of predictive animal models for many human hereditary disorders indicates a research need in what is a new and promising area of investigation.

## Molecular Epidemiology

The use of integrated chemical or biomolecular measurements of individuals within the context of properly designed epidemiological studies (that is, molecular epidemiology) provides a powerful and sensitive means for linking individual exposure and response with outcomes (See Figure 1). It should be noted here that the distinction between injury and "change" is not always sharp or readily made. Although most research identified as being "molecular epidemiology" has dealt with cancer, this concept could apply to the study of many other disease entities as well.

A principal research thrust associated with the field of molecular epidemiology is that molecular biologists/toxicologists and epidemiologists should identify subfields in which complementary skills could be effectively integrated and applied to solving complex environmental health problems. What is important here is the development of institutional mechanisms that assist basic scientists and toxicologists in working collaboratively with epidemiologists.

Research needs include the development of better characterizations of sensitivity, specificity, time-course, predictive value, reproducibility, and dose-sensitivity of molecular markers. Research in molecular epidemiology requires an aggressive development of a biochemical molecular approach to assess chemical exposure and chemical-induced injury; establishment of the relationship between these measurements and effects (molecular interactions, biological markers, and subsequent disease) is an essential element of such research.

The use of quantitative risk assessment (QRA) of health effects depends on development of models that are hypothetically consistent with present understandings of the process of carcinogenesis within an individual (that is, a multistage process), but such models may have the current limitation that, despite being aimed--to the extent possible--at the most sensitive group, they do not deal explicitly with variation in host susceptibility.

Molecular epidemiology may be useful in the development of more biologically plausible models for use in quantitative risk assessments. At the present time, molecular epidemiology would be helpful in providing a better quantification of variation in response to exposure to environmental contaminants. This type of quantitative characterization of the responses of the human incorporated into quantitative risk assessment models could play a significant role in improving their reliability.

Although there is a need to establish the most biologically plausible predictive models for low-dose cancer risk predictions, both from the perspective of the process of carcinogensis in the individual and from the perspective of the variation in response within the highly heterogenous human population, there is also a need to validate these predictions whenever possible. Expanded use of pharmacokinetic data can also improve cross-species extrapolation.

Research on alternate and adjunct approaches to methods for QRA also merits extension; this should include QRA for endpoints other than cancer. In addition, the use of molecular epidemiology can provide measurements of effective dose and early preclinical response to toxicants that may be predictive of health outcomes. This approach should ultimately provide more precise estimates of human risk of disease associated with low level exposures than are now possible with existing quantitative risk methods.

Banks of Biological Materials

The Panel discussed the usefulness of various tissue banks. The resolution of several questions was considered crucial before a bank is established. Examples of these questions are the following:

- What specific hypotheses are to be addressed by the study of these tissues?
- How should the samples be stored and access to them assigned?
- What type of materials (for example, whole blood, plasma, red cells, DNA, fibroblasts, lymphoblastoid lines, placentas, tumors) should be maintained? and
- What types of populations are to be studied?

The primary task is to evaluate the value of banking tissues from various ongoing studies or projects. A second task is to decide if specific studies should be undertaken to establish additional cell banks. We believe that a thorough review of existing tissue and cell banks should be undertaken, and that maximum use should be made of these resources before developing and supporting further efforts in this area.

Multiple Exposure

One of the most important environmental health questions of continuous concern has been the issue of predicting the effects of mixtures of toxic agents on biological systems. Human exposure to environmental agents is almost always multiple, and the potential for interactions among the multiple exposures is great. The biological activity of these agents must be defined in the laboratory and monitored in human populations. Dose-response relationships must be developed, species comparisons attempted, and thresholds for effects (when they exist) established. Use of biological dosimeters of exposure will greatly facilitate this effort.

It was pointed out that major gaps in our knowledge of the effects of multiple exposure include the lack of models that enable us to predict interactions, short-term tests to identify hazardous complex mixtures, and epidemiological studies on the effects of mixtures. Because most known biological interactions between chemicals have only been investigated at relatively high exposures, particular attention needs to be given the dose dependency of these interactions.

The Panel felt that a pragmatic evaluation of numerous combinations of chemicals is not likely to be useful for establishing the foundation upon which accurate predictions of untested combinations could be made. The best avenue for developing predictive models is thought to be best derived from the elucidation of mechanisms of toxicity.

Registries

Registries may be an important resource in the assessment of the potential effects of environmental agents on human health. They provide a useful source of information for analysts to tap in evaluating specific hypotheses concerning exposure/outcome associations, and in identifying high-risk populations, exposures, and specific outcomes. Exposure-type registries should not be permanent fixtures, as is sometimes the case, but should have a defined term based on fulfillment (or nonfulfillment) of purpose.

Most emphasis on registries has rightly been placed on the development of outcome and exposure registries. However, there is a need to develop registries of genetic groups potentially at high risk, for research purposes only. For example, the availability of a registry of persons carrying one or more defective repair genes would allow the health consequence of these deficiencies to be determined, and would provide a resource of subjects for incorporation in other studies of dosimetry, markers, and occupational and environmental effects. It was also suggested that a registry of biochemical receptors would be a useful tool for anticipating agents likely to have high-level biological effects.

The operation and administration of registries can be performed by private or public institutions. This has been effectively demonstrated with outcome registries, such as cancer registries. Generally, for outcome registries, those who use the information should have a primary role in the data collection or maintenance of the registry. The situation is less clear with exposure registries. These are newer and may not initially have designated users. For these types of registries, governmental entities might be the best choice for overseeing or maintaining them. However, major users should be actively involved in their advisory bodies.

The legislation that established the Agency for Toxic Substances and Disease Registry designated it as a Federal Government responsibility. Individually identifiable information, in Federally-operated registries, is subject to the protection of the Privacy Act and hence generally not available under Freedom of Information Act requests. Thus, governmental operation imposes certain limitations.

Exposure registries need not be exclusively confined to governmental operation, but since they may be used to circumscribe populations with potential risk prior to the formulation of specific hypotheses, the impetus for maintaining them might be weaker without governmental support.

Clinical Epidemiology

In addition to the strong emphasis placed on the need for research in the subject area of molecular epidemiology, it was recognized that the practice of clinical epidemiology can also make significant contributions in elucidating the etiology of occupationally- and environmentally-enhanced disease in a cost- and time-effective manner. This is particularly true when the risk of disease is high and a relatively small sample size can suffice. Such research could make use of special techniques (See, Robins et al.) to enable researchers to establish the occurrence of clinical changes. Use of this type of clinical epidemiological study could indicate whether a serious public health concern exists and whether more extensive cohort studies are necessary. An example of the utility of such a research methodology was presented in which a clinical study of lithographers revealed that six of seven displayed aplastic anemia as a result of exposure to glycol ethers. The existence of such a study established the link between glycol ethers and aplastic anemia, but it would require data to extend this linkage to a quantitative risk assessment.

Emerging Technologies

Assessing health hazards of emerging chemical and biological technologies presents a major challenge to environmental health professionals.

Microelectronics: The problem is exemplified by the semiconductor industry. Although traditionally viewed as nonpolluting, the microelectronics industry uses several thousand chemical agents, including a number of exotic chemicals with limited toxicological data bases. Process advances in microelectronics technology are in a state of continuous change and new processes (such as laser etching and focused ion implantation) bring new toxicological concerns. These and other forthcoming advances in the microelectronics industry provide an opportunity to initiate human health studies from the very first use in industry.

The need to evaluate the toxicological hazards associated with novel advances within the microelectronics industry should serve as a model for the close cooperation between the emerging chemical technologies and the toxicological community.

<u>Biotechnology</u>: The field of biotechnology represents another set of unique and important health-related research challenges. By definition, biotechnology is the application of a biological system to technical and industrial processes; as such it represents the integration of molecular biology with chemical and process engineering. The principal health-related concerns have centered on the potential for microorganisms, as altered by genetic engineering, to prove harmful. Assessing these concerns has been hindered by the difficulty not only of quantifying the probability of a novel occurrence, but also by uncertainty as to the type of damage that may occur.

Even when the organisms employed in an industrial process are thoroughly characterized, the concern about possible health risks will continue. A recombinant DNA sequence may be characterized in great detail, but its entrance into the human ecosystem may provide unforeseen and important assessment questions.

It should be emphasized that natural organisms highly modified by genetic selection have been used safely for a long time in industrial applications. The issue, then, is whether artificial insertion of genes by genetic engineering is sufficiently different to bring a new scale and spectrum of risks. The present reality is that there is a lack of data to support or deny concerns about genetically-engineered organisms in the open environment. A major health-related challenge in the area of technology as applied to genetic engineering is the need to address this lack of a data base from which reasonable policy judgments on the course of biotechnology could emerge.

<u>Research Funding Considerations</u>

Within the context of limited governmental research budgets, there is the natural inclination for peer reviewers and agencies to shy away from funding labor-intensive studies, especially as they pertain to long-term studies. However, given the chronic exposure and long latency periods of environmentally induced diseases, long-term studies in this field are often the most likely to yield outcomes of a quantitative and relevant nature. Because such studies have the possibility of providing a large return on investment, they should not be discriminated against in the funding process. As with all studies, the long-term, labor-intensive protocols must be able to pass the peer review process as established by granting agencies.

Both contracts and cooperative agreements have tended to become so awkward administratively as to discourage many investigators. In addition, there is little incentive on the part of investigators to bring novel ideas to Federal agencies in the context of contracts, given the necessity of issuing a Request For Proposals that requires the investigator to release an idea to the competitive, open research market. The reopening of the sole source mechanism in appropriate cases could remedy this dilemma.

The Panel believes that the Federal funding agencies should develop a flexible program for funding certain studies, in a very short time frame, for which relatively unique and transient opportunities present themselves and in which the research benefits are potentially high. Although the provision of initial funding could help the investigator acquire the baseline data, he would then be obligated to submit a full proposal for continued funding of the project. This process would allow the supporting agency to take advantage of a unique circumstance with a modest investment of money, while requiring the investigator to perform in an acceptable and peer-reviewed way for continued support.

## RECOMMENDATIONS

We elected not to assign priorities to our recommendations. In addition, we adopted an underlying principle, related to informing the public, that is critical to effective implementation of our recommendations.

This principle states, "Society should be informed of the rapid technological changes in the biomedical sciences and their implications for human health and safety to provide a sound basis of understanding and to allay needless anxiety."

Specific Panel recommendations are as follows:

### Human Physiologic and Genetic Diversity

We have adopted the following recommendations relative to the great heterogeneity that exists in physiologic and genetic makeup of human populations:

- Seek better understanding of how human genes interact with environmental factors to produce or modify disease;

- Initiate a major Federally-funded effort to study the basis and significance of differential susceptibility. The effort should be concerned with genetic factors affecting toxicity, as well as with development, aging, nutrition, lifestyle, and prior disease. The knowledge generated would be of considerable value to many sectors of society, including the various regulatory agencies in their standard-setting methodologies;

- Extend occupational and environmental health studies beyond Caucasian males to include women and non-Caucasian racial groups, given that most current health-based criteria used to support exposure standards are derived from studies with Caucasian males. This is needed because the workforce composition is changing;

- Extend the use of DNA probe/genealogical studies to identify useful cohorts in environmental and occupational health studies;

- Develop *in vitro* assays for the phenotypic identification of heterozygotes, which are possibly at increased risk to agents causing environmentally induced disease;

- Combine the use of DNA probe/genealogical studies and *in vitro* assay techniques to identify high-risk and susceptible groups within the population; and

- Continue review of test protocols for the Environmental Protection Agency, the Food and Drug Administration, and other appropriate agencies with special considerations for groups at potentially increased risk.

Biochemical and Molecular Epidemiology

The wide range of opportunities available in the fields of biochemical and molecular epidemiology leads us to make the following recommendations:

- Monitor by molecular epidemiological methods specific human populations (such as workers, smokers, or chemotherapy patients) exposed to potential carcinogens. Monitor both for intrinsic information and for methods development;

- Refine measures of functional impairment and cumulative damage and correlate these with markers;

- Explore utility of tissue banks in biochemical epidemiology in order to permit retrospective studies; and

- Seek ways to facilitate collaboration between epidemiologists and molecular biologists.

Clinical Epidemiology

We have adopted the following recommendation as a means of capturing the potential contributions of epidemiologic approaches:

- Actively pursue clinical epidemiological studies as an initial phase in the establishment of a data base to support subsequent risk assessments.

Health Registries

Recognition of the value of health registries in documenting evidence of health dysfunction leads us to make the following recommendations:

- Support research for the mapping of genes and create repositories. This follows from the observation that large-scale genealogical studies and data bases have a unique opportunity to determine gene/environment interrelationships in the development of numerous human diseases;

- Increase availability of medical and Federal financial records, such as Social Security and Internal Revenue Service records, for research studies in ethical ways;

- Improve access to health and safety data in industry;

- Encourage and extend the Surveillance Epidemiology and End Results (SEER) program;

- o Evaluate possibility of self-selection bias in exposure registeries and its influence on epidemiological study outcomes; and

- o Explore utility of establishing a registry of receptors, including their occurrence and characteristics.

Exposure

Reliable exposure assessment is a vital component in quantitatively relating exposure to disease outcome. Consequently, we make the following recommendation:

- o Develop better methods for quantification of exposure. Two of the most important limitations affecting the statistical strength of epidemiologic studies are the quality and quantity of exposure data. There is a need to direct research toward the development of better biological indicators of exposure. These include markers of molecular dose and early response to toxic substances at the cellular or molecular level. Environmental monitoring also needs improvement; the correlation of biological markers with environmental measurement (exposure dose) should be tested.

Mechanisms of Action

Comprehensive knowledge of mechanisms of action is invaluable information. Therefore we recommend that the following actions be taken:

- o Extend study of cell membrane and cytosolic receptors as aids to understanding of toxic action of chemicals; and

- o Support parallel pharmacokinetic laboratory studies in animal models and persons already occupationally exposed, in order to attempt to validate animal model predictions of human responses, and understand mechanisms of toxicity.

Quantitative Risk Assessment (QRA)

QRA approaches are increasingly being incorporated in regulatory and other environmental management strategies. Because of the need to improve and clarify the methodology for QRAs, we make the following recommendations:

- o Intensify efforts to improve and validate QRAs, in particular to improve methods for extrapolating data from laboratory animals and other test systems to determine QRA in humans;

- o Seek ways to include role of genetic diversity in QRAs; and

- o Explore development of QRAs for endpoints other than cancer and genetic effects.

Mixtures

Individuals or populations are rarely exposed to agents singly; therefore, we make the following recommendations:

- o Intensify study of the mechanisms of known chemical interactions in and on biological systems; and
- o Downgrade the utility of pragmatic testing of chemicals studied in combination.

New and Emerging Technologies

Although sometimes characterized as having few or benign impacts, the existence of novel technologies poses the potential for serious health impacts. Thus, we have adopted the following recommendations:

- o Monitor genetically engineered organisms that have been designed to survive in the environment; and
- o Exploit the opportunities for the study of new and exotic potentially toxic chemicals by initiating studies on first usage.

## APPENDIX III

### Summary Report of the Expert Panel Meeting on Geochemical and Hydrologic Processes and Their Protection

### INTRODUCTION

The meeting on Geochemical and Hydrologic Processes and Their Protection was included in the series of expert panel meetings because the observed changes in the environmental media of air, soil, and water are key indicators of the existence and ultimate effects of hazards to environmental quality and human health. Many connections with the long-term environmental research and development (R&D) topics addressed in the other panel meetings were evident. The Panel was charged with addressing long-term environmental R&D needs in four topic areas:

- o Surface Water and Groundwater Processes and Pollution;
- o Land/Soil Processes and Pollution;
- o Atmospheric/Oceanic Processes and Pollution; and
- o Multimedia Toxic Substance/Hazardous Waste Research.

### THE CONTEXT

Knowledge of geochemical, atmospheric, and hydrologic processes enables us to understand how chemical substances we have released to the environment, or that we encounter from natural sources, are transported or transformed and thus inadvertently become part of the air we breathe, the food we eat, or the water we drink. These processes also affect the survival of plants, animals, and humans living at sites distant from where the substances were initially discharged, or initially existed. Environmental risk assessment requires an evaluation of exposure to chemical substances and of the nature and magnitude of effects associated with that exposure. Both categories of knowledge are essential to our capability to predict the consequences of the release of chemical substances to the environment. In order to predict exposure to chemical substances, environmental managers must determine quantitatively where these chemicals are discharged and how they are transported through and transformed by the environment. Long-term research is needed to define the relationships between geochemical, atmospheric, and hydrologic processes and the biological receptors of the transported chemical substances. We cannot preserve the health of biological communities or humans without knowledge of these transport and transformation processes.

Each year billions of tons of solid waste material, of which a small percent can be designated as "hazardous," are produced in the United States. Whether via landfills, surface impoundments, direct surface applications to land, incineration, fugitive emissions, or direct or indirect release to air, surface waters and groundwaters, these wastes are discharged to and ultimately reside or degrade in the three environmental media. Many wastes degrade rapidly. Some, however, persist or are transformed into substances that can cause damage.

Waste management strategies have assumed that soils can be used for interment of toxic and hazardous substances. But we still lack rudimentary knowledge of the quantitative importance of fundamental soil processes. We need to make an investment now in increasing our knowledge of these processes to prevent future waste disposal problems, problems that are serious in consequence. Wastes discharged improperly can increase our daily exposure to chemical substances that may be acutely or chronically toxic, or these substances may cause long-term environmental change to living or nonliving resources. Furthermore, these are long-term problems. Even with waste reduction processes, we will continue to generate some amount of residuals that require disposal.

Each medium has a specific capacity to assimilate some amount of the wastes it receives without unacceptable adverse impacts to human health or environmental quality. There are numerous examples, however, in which each of the environmental media has been overloaded in certain areas. In addition, there are transfers between the environmental media that may cause unanticipated environmental degradation. In some cases, the transfer from one medium to another has been the measure of concern; an example would be the movement of material from the soil to groundwater. This and other examples are described below.

Soil/Groundwater Manifestations

Large quantities of wastes are discharged to the soil from homes, cities, and industries. Researchers surveying hazardous wastes managed in 1981 under the authority of the Resource Conservation and Recovery Act estimated that of the total hazardous wastes disposed, 3 million wet tons were placed in landfills, 19 million tons were released to surface impoundments, and 32 million tons were discharged into deep injection wells. In addition, several million tons of hazardous wastes removed from hazardous waste sites over the next decade will need to be treated. Other less-visible threats to our soil/water resources include approximately 1.9 trillion gallons annually from septic tanks, 4.8 million dry tons/year of municipal sludge, and an unquantified amount of leakage from underground chemical storage tanks, particularly from the large number of gasoline storage tanks dotting our landscape. Although we have some information regarding the thousands of drinking water wells that have been forced to close, we have very little information concerning the magnitude of risks facing our groundwater supplies, both in terms of information on the status of whole aquifers and comprehensive data on concentrations of all organic compounds of concern.

Atmospheric Manifestations

The behavior of the atmosphere itself is affected by trace contaminants. Among these are methane, nitrogen oxides, and ozone. The atmosphere receives emissions from a number of different sources while providing rapid transport and dilution. Emissions have been increasing at a rapid rate from the surface of the Earth such that each year $10^8$ tons of methane and $10^7$ tons of nitrogen oxides are released to the atmosphere, a large percentage attributable to human activities. The emissions to the atmosphere of nitrous oxide and methane exceed known removal processes, and atmospheric concentrations of these pollutants are expected to increase during the next 10-50 years. Analysts examining data from polar ice cores have indicated that methane concentrations have doubled in the last 400 years. We are just beginning to understand the effects of these emissions, but it is expected that increased concentrations of nitrous oxide, methane, and other radiatively active gases will contribute to the warming of the Earth's climate. Ozone concentrations in rural U.S. locations are also thought to have increased, doubling since the 1950s, and episodes of high concentrations that can blanket the eastern United States are sufficient to cause damage to agricultural crops in laboratory experiments.

Atmospheric manifestations have terrestrial consequences as well. Ozone, nitrogen oxides, and sulfate deposition have all been hypothesized to be responsible for the decline of forests in Central Europe and the eastern United States. Environmental exposure to airborne lead, benzene, and trichloroethylene, as well as to ozone and other substances, may pose significant health risks.

<u>Coastal Ocean Manifestations</u>

The open ocean can provide an enormous amount of dilution to minimize the risks presented by waste disposal, particularly those wastes dominated by high concentrations of metals or nutrients. Coastal estuaries, however, act as containment basins, collecting sediments and contaminants from rivers, direct discharges from coastal municipalities and industries, and atmospheric deposition. Several billion gallons of sewage effluent are discharged to our coastal waters each day, and millions of tons of solid wastes are dumped into the ocean each year.

The potential for estuaries to retain wastes can be seen in those estuaries that have become hazardous waste sites such as Commencement Bay, WA, and New Bedford Harbor, MA. In other estuaries, contamination of fish with kepone, PCBs, or dioxins has forced the closure of commercial and recreational fisheries, and bacterial contamination forces the closure of about one-quarter of the Nation's shellfish beds each year. Eutrophication in the Chesapeake Bay has been blamed for the decline of sea grass communities that serve as nursery areas for fish and help protect the coastline from erosion.

<u>General Principles of Geochemical and Hydrologic Processes Research</u>

The Panel's major findings fell into four general categories:

- o The importance of an integrated approach to environmental problems;
- o The need for long-term environmental research;
- o The necessity of developing predictive models for determining the stresses placed on geochemical and hydrologic processes by different waste loadings; and

- The importance of determining the efficacy of different institutional strategies for environmental management.

Integrated Approaches: The Panelists endorse an integrated approach to the environmental problem of waste management, and feel that the approach must be

- integrated across the environmental media of air, soil, and water, and subcompartments within those media;

- integrated across substances present in the environment, both in how interactions between chemicals may affect the way these substances are transported and transformed in the different media, and in how chemical interactions influence their effects on health and environmental quality; and

- integrated across disciplines addressing these problems, including disciplines in the natural sciences and social sciences concerned with the natural and institutional processes for controlling risks associated with alternative environmental management strategies.

The importance of the transfer of contaminants between environmental media has been documented repeatedly: volatilization of chlorinated organics from hazardous waste sites, sewage treatment plants, or contaminated shallow aquifers; the transfer of substances from air to soils and surface waters through precipitation and dry deposition; and the removal of contaminants from surface waters and groundwaters by sorption to soils or sediments and their subsequent rerelease during resuspension and desorption.

Although the results of intermedia transfer studies have often been qualitatively demonstrated, quantitative rate measurements of these processes are still rudimentary. Transfer rates of particular interest are aerosol formation of chemically reactive organic pollutants and the effect of surface active agents on the air/water interface, on precipitation scavenging of aerosols in the field, on dry deposition as a function of surface roughness, on volatilization and adsorption rates as a function of temperature and moisture content, and on the rates of deposition and resuspension of sediments. Determining the chemical composition of aerosol particles of different sizes and the size distribution of suspended solids in the liquid phase will also help in understanding these transfer rates.

The other CEQ Panels have recommended the study of long-term effects attributable to chemical interactions in complex mixtures of effluents. We endorse those recommendations. From preliminary results of ongoing studies, analysts believe that the movement of high concentrations of complex mixtures of wastes through soil differs markedly from laboratory models, in which the movement of dilute concentrations of single compounds is measured. Leaking containers at hazardous waste sites or accidental spills are prime examples of the occurrence of such high concentrations of complex chemical mixtures.

The study of geochemical and hydrologic process across different media obviously requires knowledge from a broad range of disciplines from soil science to atmospheric chemistry. The Panel also recognizes that natural science research of geochemical and hydrologic processes must be supplemented by social science research to facilitate interpretation of natural science results into laws and regulations that can be implemented and enforced.

Long-Term Environmental Research: In addition to the need for greater emphasis on integrated approaches, there are long-term needs for the study of transport and transfer processes and their biological consequences within each medium. We use the phrase "long-term environmental research" in all three definitional senses considered by CEQ: research that is fundamental in nature, that provides a continuing baseline, or that anticipates future research needs.

Fundamental Processes: The Panel finds that long-term research of fundamental processes is particularly necessary in the subsurface soil/groundwater environment. Our knowledge base is least advanced for this medium and currently inadequate to protect public health and the environment in all cases. Specific research is needed to better describe the transport and transformation of organic compounds considering hydraulic, biotic, and abiotic aspects in both homogenous and heterogenous soil matrices. For instance, water plays a dominant role in the soil environment, but we do not have a quantitative understanding of the variation of the sorption and vapor pressure of organic compounds with soil moisture, particularly below 10 percent soil water content. In addition, we know very little about the fundamentals of hydraulics and geochemistry for low-permeability materials, yet low-permeability sites are selected as optimal locations for the disposal of hazardous wastes. In addition, we need more information about the hydrologic properties of crystalline rocks and the importance of networks of fractures because most deep injection wells are located in these types of formations.

Continuing-Baseline Studies: The Panel finds that the short-term intensive studies at waste disposal sites can be linked with some longer term studies conducted concomitantly. The Panel emphasizes deficiencies in "continuing-baseline," long-term research, especially continental-scale studies in coastal waters and the atmosphere. In these media, there is more knowledge of environmental processes, but insufficient background information on historic or even present concentrations of many important chemicals.

We are also ignorant of the normal range of variations through which ecosystems and their components pass in coastal waters. Without knowledge of background concentrations or biological variation, we have a very difficult time assigning causes to small pollution impacts, because we cannot distinguish them from natural variations. Once we are certain that a measurement is outside the normal range for the system, we will also need to study the processes that have caused such effects and how pollutants interfere with those processes. Without knowledge of the normal range of variation, we are unable to determine unambiguously that something has happened to the system. Long-term observations, on the scale of decades, are necessary for the oceans and the atmosphere.

Anticipatory Studies: Long-term coastal ocean and rural atmosphere monitoring stations could serve as "anticipatory long-term research" vehicles for identifying actual environmental problems at an early stage. Past experience is an indication that surprises are likely, and it is our intent that monitoring programs exist side by side with research programs to explain the monitoring observations.

Data Quality: Because of the expense associated with collecting long-term environmental data, the Panel notes that compliance and monitoring data at existing hazardous waste sites and other industrial waste sites, such as Superfund sites, could be used for research if scientists were involved in the initial design of data collection programs. Data gathered for environmental management, whether for litigation, for determination of environmental trends, or for policy development, must be valid for its proposed uses. In addition to our concern about the precision and accuracy of measurements, we must assure that the data collected are appropriate to the needs the data must serve. Monitoring data could be used in answering research questions by careful initial design of sampling programs. Research questions concerningthe transport of chemicals away from the site are, in turn, germane to regulatory concerns about risks posed by waste sites.

Assimilative Capacity Predictive Models: The Panel deliberated extensively over the usefulness and meaning of the concept of "assimilative capacity" of the environmental media to process waste loadings. We recognize that recycling and re-use, pretreatment, and process and product substitutions are the cornerstones of any waste management strategy. We note that the term "assimilative capacity" can have different meanings among disciplines. Many of these differences can be attributed to media characteristics. For instance, in containment media such as soils and some coastal estuaries, assimilative capacity refers to the amount of material added to a system before the system begins to leak, releasing the wastes to the outside environment. In dispersive environments, such as the atmosphere and open ocean, this physical sense of the term would refer to a rate of waste loading that is sufficient to allow waste concentrations to be diluted to the range of background variation outside of a mixing zone. Especially for soils, we know relatively little about assimilative capacity, with the exception of the assimilative behavior of soils with respect to certain metals.

Both of these transport-based definitions also may include, although they have not usually in the past, consideration of the extent to which these waste loadings are transformed by physical, chemical, or biological processes to other substances, the rates of transformation, and the degree to which these transformed substances accumulate in the environment.

Transport-based definitions of assimilative capacity require knowledge of geochemical, hydrologic, and transformation processes. Assimilative capacity has a further biological definition in engineering design of "the amount or rate of waste addition that does not cause unacceptable adverse impacts to the public health or to the environment." This effects-based definition of assimilative capacity requires additional information on long-term biological effects of pollutants on receiving media.

The determination of assimilative capacity in environmental management requires the development of predictive models. We believe that a useful first step for model formulation is to use simple models coupled with monitoring to permit model validation. Initial case studies should emphasize pollutants that exist primarily in one environmental state, whether gaseous, liquid, or particulate. In addition, compounds should be chosen that have similar residence times in each medium.

Institutional Arrangement Needs

Although we believe that an intermedia approach is necessary for proper waste management, we note that extant laws and regulations relating to environmental protection largely focus upon single media. Changing institutional arrangements to allow more of an intermedia perspective will present problems requiring long-term political and administrative research as well as research in the technical areas discussed above. Multimedia regulation may be possible by only slight modifications of existing institutions or may require extensive changes. In appraising alternative institutional means for achieving multimedia control, the feasibility of legislative and regulatory adoption and implementation must be determined. Better efforts are required to understand the political science and policy of environmental legislation. One method to improve our understanding of the efficacy and feasibility of multimedia control is to explore our historical experience with environmental protection at the legislative and administrative levels.

Institutional Research Needs

We believe that the sorts of intermedia, interdisciplinary, long-term research that we have discussed above are not adequately addressed by the current institutional structure. For instance, no academic institution has a curriculum that is adequate for the preparation of its students for tackling intermedia issues. Additionally, the measurement of substances in any medium over long time periods will be quite difficult, as evidenced by case histories of past attempts. Example are the long-term measurements of carbon dioxide in the atmosphere.

Improved institutional arrangements are needed to assure adequate funding, conduct research, and apply the research results to the solution of environmental problems. We have not identified what sort of institutional structure would be most appropriate. We have agreed that important attributes of the institutions conducting research are that they be interdisciplinary, nonpolitical, and capable of taking a broad, integrative view. Several panelists cite the EPA's Centers of Excellence or the National Center for Atmospheric Research as possible models.

## RECOMMENDATIONS

In developing our recommendations, the Panel concurs with a seminal principle for research on media processes and their protection: "our environmental media--air, soil, and water resources--are vital to life on this planet and inextricably linked; our goal is to preserve our soil, air, and water resources and their productivity; these resources will always serve as an ultimate sink for wastes, though the amount and hazard associated with current waste inputs can be reduced; and protection of these resources is economically more reasonable than allowing contamination and then attempting restoration." We also believe strongly that the private sector should play an increased role in the improvement of existing monitoring techniques and protocols. We emphasize the importance of information transfer to environmental managers in both government and industry.

Integrated Approaches

We recommend the following research activities:

- intermedia transfer rates should be determined in an intermedia modeling/monitoring program that is focused on intermedia transport as a function of different physical parameters;
- results of intermedia modeling/monitoring programs should be used to develop screening procedures to determine for which environmental conditions and chemical characteristics it is necessary to consider intermedia transfer;
- effects of chemical interactions on transport processes should be studied; and
- there should be a linking of these transport processes to the biological effects seen in receptor organisms affected by exposure to the chemicals, whether they be humans, commercially important species, or indicators of the health of whole ecosystems.

Fundamental Processes

We recommend the following research activities:

- Research on transport and transformation processes should be conducted as part of site-specific field studies. Extrapolation to field situations from laboratory results has been extraordinarily difficult to date, and until more general principles are revealed, site specificity will remain a perplexing problem for soil/groundwater studies. We recommend that existing hazardous waste sites and other industrial waste sites and well-functioning Resource Conservation and Recovery Act (RCRA) sites be investigated as candidates for study. Given the expense of groundwater monitoring, we believe that research can most efficiently be coordinated with compliance monitoring at existing sites at an early stage of development of the sampling plan. Besides exiting sites, older sites can also be explored similarly to the way archaeologists exhume ancient civilizations, reconstructing important processes occurring years earlier through the analysis of chemical residues. We recognize that there may be legal constraints on such a research approach because of such questions as liability, but we believe the idea is worth pursuing; and
- Atmospheric research should be aimed at understanding fundamental processes controlling tropospheric biogeochemical cycles on a global and continental scale. These processes include studies of source and sink processes for atmospheric gases, particles, and research on photochemical reactions.

Continuing Baseline Studies

We recommend the following research activities:

- For coastal waters, several small estuaries (for example, Buzzards Bay, MA, South San Francisco Bay, CA, Mobile Bay, AL, or Trinity Bay, TX) should be selected to determine the mass balance of contaminants while establishing the natural biological communities residing in these estuaries and the effects of contaminants on those communities. Mass balances should also be conducted on coastal areas that have historically received large waste loads. Improved collection of data about chemical release is necessary to do this. As yet, we have not been able to construct satisfactory contaminant mass balances for any estuary. This problem should be most tractable in small estuaries, where it is only necessary to consider relatively few processes; and

- For the atmosphere, long-term baseline measurements (at least 10 years) should be collected for the most important pollutants. These would include ozone, oxides of nitrogen and sulfur, and methane--on both a continental and global scale. Baseline rural atmospheric measurements, in particular, are emphasized because they currently lie outside the range of responsibility of the present institutional structure. These data are needed to define the current state of the atmosphere, to shed light on important source mechanisms, and to allow determination of long-term trends.

Assimilative Capacity

We recommend the following research activities:

- Information should be developed regarding the amount of waste generated and disposed into each of the environmental media, the amount of reduction available under the above options, and whether exposure could be reduced by handling the waste in another medium. We also recommend that new methods be evaluated for their potential to detoxify municipal and industrial wastes. For example, wet-air oxidation, an aerobic thermophilic process, may be technically and economically feasible for treatment of concentrated organic wastes; and

- Regional evaluation methods and techniques should be developed for predictive purposes, including the development of single-media and intermedia models. There is an immediate need for methods to screen out inappropriate or outmoded waste sites and to verify pollutant transport models. Models appropriate for one set of uses or conditions may be totally inappropriate in a slightly different context. It is important to ensure that the model is valid for the uses that are required of it. We are concerned that the models not require unrealistically extensive data collection. The data collection requirements for many existing models quickly become impractical under normal field settings.

Institutional Arrangement Needs

We recommend the following research activities:

- Respective costs and benefits of multimedia control as a regulatory strategy should be characterized and appraised in comparison to single- or dual-media alternatives. Pollutants that can be more effectively controlled through multimedia control strategies should be identified. It may be that a multimedia approach is only necessary for a small number of cases;

- Alternative strategies of multimedia control should be characterized and appraised, with such strategies defined by (a) alternative goals and objectives; (b) alternative institutional means to achieve goals and objectives; and, (c) requirements for scientific and other technical inputs. In particular, there should be an evaluation of the usefulness of approaches such as that embraced in the Toxic Substances Control Act as a basis for regulating toxic and hazardous wastes by means of multimedia control. For instance, case studies of EPA's Office of Groundwater and the evolution of the Groundwater Strategy, or of EPA's current acid rain activities, could yield valuable insights into administrative constraints to toxics integration efforts, because each of these potential subject areas involves toxic problems with multimedia dimensions.

# APPENDIX IV

## Summary Report
## of the
## Expert Panel Meeting
## on
## Environmental Impacts and Their Mitigation

### INTRODUCTION

The meeting on Environmental Impacts and Their Mitigation was included in the series of expert panel meetings both because of growing public concern over the compatibility of economic growth and environmental quality and because of the need for greater predictive capability concerning the role of environmental resources (physical and biotic) in influencing human well-being. The topic areas that served as foci for background papers for and discussion at the meeting were

- Global and Biosphere Impacts;
- Local Ecological Impacts;
- Biological Diversity; and
- Environmental Impacts of Current and Emerging Technologies.

### THE CONTEXT

Environmental concerns about the abandonment, use, and expansion of existing technologies, and about the deployment of new technologies, will continue to influence both governmental and private decisionmaking. These concerns have both national and international dimensions and are expected to continue well into the foreseeable future. Our capacity to estimate ecological and environmental risks is not sufficient to insure against either costly, and possibly irreversible, damage to essential biogeochemical cycles or preventable extinctions of endangered species and ecosystems.

This panel is convinced that serious deficiencies in risk assessment capabilities can only be alleviated by a substantial increase in the amount, quality and diversity of long-term ecological research. Our discussions have been focused on the kinds of long-term environmental research that we believe should be given high priority. We also note structural and personnel dimensions of long-term environmental research that need attention.

A major point of consensus among Panelists was the need to evaluate institutional options for providing stable support for interdisciplinary long-term environmental research. Stability in the funding for this interdisciplinary research was viewed as crucial in attaining the goals and recommendations set forth by the Panel.

Global and Biosphere Impacts

The Earth's biogeochemical cycles are essential to the maintenance of the biosphere. The continual transfer of chemical elements between biotic and abiotic components of the biosphere distinguishes the Earth from other planets and motivates use of the term biogeochemical element cycling.

Earth's element cycles involve major reservoirs, including the atmosphere, oceans, and terrestrial and aquatic ecosystems, that are coupled by physical, chemical, and biological processes. Earth's climate is strongly influenced by the physical, chemical, and biological composition of these reservoirs, and climate, in turn, influences element cycling in the biosphere. These element cycles are also linked to each other by ecosystem processes--the pool size and flux of one element depends on the pool sizes and fluxes of other elements. These two feedback loops play significant roles in maintaining habitability of the planet.

Equilibrium (or steady-state) budgets for global cycling of the major elements (carbon, nitrogen, phosphorus, and sulfur) have been widely published and recently well reviewed. Missing values are frequently estimated based on the assumption of equilibrium or as a fixed ratio to values for other elements. Thus, such numerical values are largely judgmental. It is, therefore, important to realize that the published data on equilibrium global budgets for elements may be misleading in suggesting responses to perturbations. The relative importance of various cycling processes and elemental fluxes may be quite different in projecting responses to a perturbation than they are in describing budgets under natural circumstances.

The carbon cycle is the only cycle whose global scale response to a large-scale perturbation, fossil fuel use, has been analyzed in any detail. It makes a great deal of difference, however, what assumptions are made concerning the biotic reservoir during the period of enhanced release of fossil $CO_2$ into the atmosphere. Even in this case, model-based estimates of carbon uptake by the oceans are, in general, too low to balance with observed changes in atmospheric $CO_2$ concentrations.

Because of the strong interactions between biotic and abiotic components, there are at least four general characteristics of global cycles that may constrain efforts at long-term element cycling research:

- Variability across landscapes and between ecosystem types;
- Linkages between the cycles of different elements;
- Potential feedback of climate change on element cycling; and
- Potential nonlinear responses to perturbations.

A hierarchical approach with respect to the geographical scale of analysis is needed in order to contend with these characteristics. Scales ranging from local ecosystems,[1] and their specific processes, to the biosphere must be considered. Concepts, data, and models are strongest at the local ecosystem level, and impacts are often first perceived locally. The development of satisfactory methods to extrapolate results based on site-specific studies to larger spatial scales is a key issue for long-term ecological research.

Core data for understanding ecological processes are derived from intensive monitoring and experimental programs at representative field sites. Eventually, ecosystems that are representative of each element of the hierarchy must be included to arrive at a total picture. In choosing sites for long-term monitoring, emphasis needs to be placed on representing broad categories of ecosystems. At the ecosystem level, models can be sophisticated and explicitly incorporate fundamental system processes. Testing and verification of such models have often been more rigorous than of those that depend on the use of complex ecosystem component data. Translation of observed changes in variables that can be measured into changes in other variables that are difficult to measure, but which may be critical in understanding impacts at larger spatial scales, is an important application of accurate ecosystem models and their underlying data sets.

Linkages between element cycles and climate require increased attention to intensive site specific studies. Using such studies, researchers can identify basic mechanisms and measure them in such a way that they can be reliably handled in studies at larger spatial scales. Moreover, results based on work at specific sites cannot be extended easily to the global scale. Research is required to develop an understanding of variations that can be derived by analyzing large data sets. This research should lead to more general descriptions that are applicable to all important variables and parameters.

The natural distribution of ecosystem complexes is the basic level of organization suitable for translation across spatial scales. Explicit treatment of the geographic distribution of these complexes in data collection, analysis, and modeling then becomes an important element of a hierarchical approach in which data on local ecosystems are synthesized into useful models of entire landscapes, biomes, and the planet's biosphere. For example, the substantial prior investment in carbon cycle research is an indication of the level of effort required to reliably project future responses of global cycles to disturbance by human activities or of changes (such as climatic change or major volcanic activity) attributable to natural variations. The carbon cycle is but one biogeochemical cycle that exerts an impact on climate. The interactions of other key element cycles (for example, N, P, S, and O) must also be studied within the framework of a long-term research strategy.

Two major uncertainties exist in understanding past changes in the carbon cycle: the history of atmospheric $CO_2$ concentration prior to the beginning of

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[1] Ecologists generally consider the local ecosystem the lowest (geographical) level of generality for purposes of analysis. Thus, although environmental impact analysts would generally view the project, or site-specific level as that at which data and models are best developed and most accessible, that is a level not normally characterized in ecological analysis.

accurate measurements in 1958; and, the history of changes in carbon storage in vegetation and soil. These two uncertainties should receive long-term research priority.

Our lack of knowledge about changes in atmospheric $CO_2$ concentration prior to 1958 shows the potential impact of inadequate or inappropriate monitoring of global environmental variables. The costs of attempting to reconstruct pre-measurement $CO_2$ records from proxy data such as $CO_2$ concentrations in air trapped in ice cores, or by modeling studies, have proven to be substantial. Work to reconstruct the history of carbon storage in the planet's terrestrial ecosystems is also proving extremely tedious. Satisfactory analysis of the impacts of land-use change on terrestrial carbon storage requires detailed knowledge of the ecological processes responsible for carbon cycling and the response of these processes to perturbations.

The general framework for long-term element cycling research incorporates two basic needs: world-scale monitoring and hierarchical approaches for synthesizing understanding of processes at the ecosystem level. Fulfillment of these needs can provide a means of translating this knowledge to larger spatial scales. Shortfalls in either aspect of the described framework can be expected to result in gaps in knowledge (or in the reliability of conclusions) that will limit applicability of research to policy development.

Toxins and Toxic Effects

Toxic materials are regulated not only to protect humans from poisoning but also to protect the human habitat, apart from man. The most important and valuable segment of the habitat is the biota, that is, other living organisms essential to maintaining the habitability of the planet. A program of toxin regulation that is effective in protecting the biosphere's organisms other than man will go a long way in protecting man as well. Programs focused solely on protecting man over the short term do not adequately protect other living organisms, and therefore cannot protect man in the longer term.

Toxic materials have now become global contaminants. The fact of the release of a toxin, whatever its purpose or source, constitutes a hazard. Pesticides, for instance, are commonly released into the air, where some fraction is dissipated, a part of which may move widely in the biosphere. Certain pesticides become hemispheric or even global contaminants that permeate the planet's oceans. Other toxins are wastes, deposited accidentally, by permit, or improperly into air and water, or onto land.

The system that the industrialized nations have developed for controlling toxins and wastes is imperfect. The scientific rationale for such regulation has been borrowed from the field of human and mammalian toxicology, but applied to the environment in general. It is based on three assumptions:

- o There is a threshold for every toxin below which there are no effects warranting regulation. This threshold concept is applied to individuals, species, populations, and ecosystems;

- o Given that there is a threshold for toxic effects, there must be an assimilative capacity for any toxin. Toxins are innocuous if concentrations remain low; and

- The primary objective of toxic substances control regulation is protection of people from toxic effects; protection of other organisms is either a secondary objective or is ignored.

A regulatory system based on the three assumptions noted above will work in a world in which the sum of human influences remains small in proportion to natural processes. It will generate a growing risk in a world in which toxin-generating human influences are continuously expanding. What regulatory approach will work? Reviewers of some of the recent ecological data suggest approaches that warrant consideration. For example, human interests can also be served well by a system of management that places a high priority on protection of the planet's biota. Thought must be given to how to reach an appropriate degree of isolation of harmful anthropogenic activities from the unmanaged environment. Long-term research is needed to assess the feasibility of such an approach. In particular, needed information includes

- Information on patterns of biotic impoverishment (at local, regional, and global scales);
- Long-term, detailed studies on the impacts of particular global toxins on the biological diversity of well-defined ecosystems that can be manipulated experimentally;
- Long-term research on man-dominated ecosystems (especially agricultural, urban, and industrialized areas) to learn how such systems can be made more benign, especially with respect to releases of widely circulating toxins; and
- Long-term research on how to better manage the earth's biota in ways that will allow both its use and its preservation.

Basic Ecological Knowledge for Environmental Assessment

Environmental assessments of Federal actions, initiated as a result of passage of the National Environmental Policy Act of 1969 (NEPA), are common elements of most (if not all) policy decisions. The NEPA process challenged ecologists to predict impacts on ecological systems from a wide array of stresses. To examine the role of ecological knowledge in environmental assessment, analysts consider case studies that illustrate how population biology, ecosystem studies, and regional analysis contribute to environmental assessment needs. Based on these analyses, it is possible to identify the need for and recommend support for long-term ecological research topics whose outputs would enable ecologists to improve the environmental assessment process.

The 17-year controversy surrounding electric power generation on the Hudson River provides a unique study of the usefulness and limitations of population-level studies in the environmental assessments. Biological resources potentially at risk because of entrainment and impingement of striped bass and other fish at power-generating facilities were highly valued, and measures to mitigate effects were very expensive. Central to resolution of the concerns was an understanding of the distribution and abundance of the Hudson River striped bass population. Massive field studies in the entire tidal Hudson were undertaken to quantitate adverse impacts. Compensatory mechanisms and density-dependent mortality regulating the population became a central issue. Mathematical models of the striped bass life cycle proliferated, but quantitative resolution of technical disputes was elusive. An out-of-court settlement was reached; it was focused on mitigation of existing impacts and long-term research. One lesson learned is that problems involving long-term dynamics of complex ecological systems cannot be solved through "crash" research efforts of a few years' duration.

Studies at the ecosystem level of organization have always been a major component of basic ecology. However, the present status of this subfield of biology is inadequate to enable analysts to perform ecosystem-level assessments. Assessment of toxic effects at the ecosystem level has been approached in laboratory model ecosystem studies (microcosms) or through mathematical modeling; in both of these approaches, analysts endeavor to reliably simulate ecosystem structure and functioning. Model ecosystem studies generally cannot be addressed directly to species of interest, and simulation models can be prohibitively unrealistic. The potential for the contribution of ecosystem ecology to impact assessment is great. Basic ecological studies of disturbed as well as pristine ecosystems are needed to develop ecosystem-level impact assessment methods.

The National Acid Precipitation Assessment Program (NAPAP) is an example of focused environmental research designed to address regional- and national-scale problems. The assessment framework is complex and emphasizes the need to develop linkages between emissions, transport/transformation, deposition, and effects at appropriate geographic and temporal scales. The NAPAP experience (degree of success) in producing a national assessment will define long-term research needs for national and regional assessments.

Basic ecological research required to support environmental impact assessment falls into three categories:

- <u>Anticipatory Research</u>. Basic information is needed concerning the dynamics of important biotic resources that are vulnerable to anthropogenic disturbance, including exploited population such as striped bass and aesthetically valued populations such as bald eagles;

- <u>Participatory Research</u>. Basic ecological research is needed to support ongoing assessments of major, long-term problems such as acid rain, the decline of the Chesapeake Bay, and photochemical oxidant effects on Southern California forests; and

- <u>Retrospective Research</u>. Basic research is needed to determine the validity of assessment methods and models. Long-term studies are needed to distinguish effects from background variance and to track the processes of adaptation and recovery.

Environmental impact assessment practitioners, who will make use of this research, must make specific predictions about clearly defined endpoints so as to generate testable hypotheses. In addition, uncertainty must be explicitly addressed. Specific research efforts can be rank ordered on the basis of the relative uncertainty associated with each of the assessment components. Estimated uncertainties are also part of the set of hypotheses to be tested in validation studies. From validation studies, assessors can unearth areas of ignorance of which they have been previously unaware (the unknowns such as improper boundaries or unknown process linkages). Assessments with these characteristics have been termed "environmental risk analyses" because of their similarity to engineering and human health risk analyses. The development of methods for environmental risk analysis is only beginning and should itself be a subject for long-term research.

Mitigating Impacts From Complex Technological Systems

Anticipating and mitigating effects on natural resources, as well as on human resources, from the residuals of complex technologies requires well-defined, forward-looking research, rather than case-by-case reactive studies of problems that could have been avoided. Major questions remain, however, as to how to explore and guide the expanded ecological knowledge base needed to minimize unanticipated impacts from changes in technology.

The three subject areas in which expanded ecological knowledge is likely to be needed to mitigate impacts from emerging technologies are the following:

- Agriculture (forestry, range management, freshwater fisheries, and the intensive cultivation of cereals, fruits, and oil crops);
- Transportation (including waterborne, pipeline, air, and interurban corridor developments); and
- Urban systems, ranging from aggregates of small cities in otherwise rural areas to suburban complexes and developing megalopolitan corridors.

In agriculture, there has been a slowing down of the rate of increase in yields of major food crops. Wheat, sorghum, soybean, and potato yields in the United States have not increased at their historic rates since the late 1960s. Several factors have been suggested as contributing to slower rates of improvement in agricultural productivity. Agriculture is the chief user of land, water, and air resources, and all of these must be of a quality consistent with the high productivity and high quality expected of agricultural production. New technologies in urban or industrial regions need to facilitate stable production in the agricultural sector.

Transportation is already a complex technology with profound primary and secondary impacts on land (corridor impacts), water (due to runoff), and the atmosphere (due to emissions). As land transportation corridors change in the years ahead, as river, lake, and coastal shipping patterns change, and as air transport changes, new patterns of residual releases to air and water will emerge and new materials in these residuals will almost certainly be encountered. For the transportation sector, models are the most widely used means of describing trends in the complexity of present and future technologies. Models have been developed to facilitate planning and operation of almost all forms of transportation, and have been used frequently in land-use planning and resource impact analysis. Problems with transportation systems analysis and prediction of the impacts of related technologies stem largely from a lack of defined or consistent urban and regional environmental goals, as well as from a less than fully developed set of risk definitions.

Urban systems, together with their built-up suburbs, can be regarded as ecosystems importing food, water, fossil fuels, and raw materials, while exporting sewage, combustion products, and solid waste. Although ecosystem waste products exit from the city in highly modified forms, and ecosystem processes operate under highly stressed conditions, many of the principles governing material exchanges in normal ecosystems have been found to hold in the city. Human activities within urban systems cause increased production of organic materials as well as release of residuals to the air and water. All of these can operate locally to change the soil, hydrology, and climate, in turn influencing the productivity of both the managed (agricultural, park, and commercial forest) and the unmanaged natural ecosystems that surround the city.

The impacts of these technologies that may need mitigation in the future can be understood best by tracking changes in residuals from early technology. Future releases of residuals to the air, for instance, will depend primarily on three factors:

(1) Growth in energy requirements, particularly for electricity, industrial processes, and automobiles;

(2) The types of industrial or economic expansion taking place in urban areas and the energy form used to meet future demand (for example, the extent of nonfossil energy sources such as solar and hydropower); and

(3) The rates at which existing pollution sources (for example, both point and nonpoint sources) are replaced with newer sources more tightly controlled under the Clean Air Act.

Predicting the influence of each of these factors is difficult. We can make rough projections for the quantities of regulated pollutant emissions, assuming any of several plausible future courses in legislation, compliance, and enforcement. Much more difficult, however, and requiring intensive anticipatory research, are projections of the trends in the emissions of presently unregulated substances (polynuclear aromatic hydrocarbons, aldehydes, dioxins, and their possible transformation products) that are expected to increase with the introduction of new technologies in agriculture, transportation, and urban systems, including urban incineration of waste.

Residual releases from urban and agricultural systems to water environments should be viewed as a sequence from their origin in homes, industry, and on land, to a fate in the total urban or rural air and water system. Environmental impacts through disposal of residuals to water from urban and transportation technologies occur not only at the site of final disposition (sludge spreading, landfill, or incineration sites), but also at other stages in the waste treatment cycle.

Among the important "residuals" of agricultural systems are soil and its associated chemicals. Soil erosion is a serious national problem, compounded now by the high level of related pollutants carried with soil from agricultural systems to the nonagricultural environment. Animal wastes from feedlots and from other intensive animal production facilities also constitute important sources of residuals. Increasingly, these wastes may be put to good use, but their use or disposal may also engender new risks.

Almost any human activity produces some effect on surrounding resources--air, water, or the biota. Large-scale, complex technologies produce diverse impacts and great risk of consequences for heath, productivity, or other amenities crucial to our society. The essential questions here are: (1) What is a significant effect? (2) What is the certainty of the effect (as opposed to a natural background trend)? (3) If the effect is only a possibility (that is, a risk), at what probability level should the risk be judged as significant? and (4) Most fundamentally, are we developing the baseline information and ecological understanding to determine what constitutes a significant impact or risk?'

The Nation already is making an immense investment in monitoring of air and water quality. Most of these measurements, however, focus on the effects of residuals on ambient values of receiving media (air, water, and soil), rather than on ultimate effects of pollutants on humans and other living organisms. Measurements of effects of complex technologies on natural resource systems (for example, the effects of acidic deposition) are exceptionally difficult, and will require intensive programmatic research, ideally with replication at numerous locations throughout the country.

In addition to the need for long-term measurements as a means of understanding or validating ecological assessment tools, and the need for all agencies who may use such data to contribute to their development, problems also exist with the reliability of, and access to, existing long-term monitoring of physical and biological systems. Most of the needs cited above rest with Federal agencies, whereas most of the reliability and access problems rest with state agencies. The impetus for improvement in data quality and data access arose from two parallel developments: first, the increasing importance of reliable data for determining the consequences of technological applications; and second, the unnecessary cost of having each investigator or decision-maker find, read, and appraise all original information relevant to a problem area or decision. Based on these examples,equal priority must be attached to the accessing of examples, equal priority must be attached to the accessing of existing data and to the development of new ecological data bases.

## Habitat Diversity and Genetic Variability

In coming decades, the global diversity of ecosystems, species, and genotypes within species is likely to decline at a rate without precedent in the last 65 million years. Although highly industrialized nations are significant contributors to this decline, most of the loss will occur in less industrialized nations, where biological diversity, human population growth rates, and pressures for development are highest, but where infrastructure is least equipped to deal with this problem.

Direct effects of this loss will include sharp declines in quality of life, economic growth, and survivorship of large numbers of humans. These effects are expected to result from loss of aesthetic and psychological benefits, from loss of intellectual benefits, and from losses of ecosystem goods and services.

People, in fact, may have an innate psychological need to be surrounded by nonhuman life. Among other things, we depend on animals as surrogates for humans in many kinds of research. Wild plants, animals, and microorganisms serve as food, medicine, energy, and raw materials for humankind. The structure of, and interactions within ecosystems result in indispensable free services, including maintaining the chemical composition of the atmosphere, moderating the climate on at least local and regional levels, minimizing the effects of flood and drought, creating and enhancing the fertility of soils and preventing their erosion, providing food for free-ranging livestock and fish populations, pollinating crops, controlling pests and diseases, and degrading natural organic detritus and anthropogenic chemical pollutants. By driving and modulating biogeochemical cycles, the biota provide for the habitability of our planet.

Loss of genetic diversity within species diminishes their evolutionary capacity to withstand environmental change. Extinctions of populations are even more frequent than extinctions of species.

Loss of some species can cause marked changes in community structure and ecosystem function, and possibly, cascading extinctions among dependent species. Historical examples are the extinction of dodos and sea otters (throughout part of their range). Nonetheless, some ecosystem measures, such as primary productivity, may not change substantially when some species are eliminated, because some ecological processes are performed by multispecies "guilds" that provide varying degrees of functional redundancy. Losses of entire guilds--for example deforestation--cause far greater physicochemical and biological changes to the ecosystem than losses of single species. Loss of ecosystem diversity eliminates both species endemic within those ecosystems and animal species that must migrate among particular ecosystems.

Long-term research on biological diversity is needed because

- o Processes underlying health and decline of biological diversity are complex;
- o Many processes unfold slowly, over periods of decades or more; and
- o Rare natural events may be far more important in determining community composition, structure, and ecosystem functioning than usual conditions.

Indicators of Change in Natural and Human-Impacted Ecosystems

Although ecologists measure numerous variables as indicators of change, many measurements are based on inadequately tested assumptions, and most have relatively poor precision with limited potential for improvement. The lack of precision and occasional controversy over the validity of an approach have lessened the effectiveness of ecological inputs to decisionmaking. The vast majority of decisions concerning the release of chemicals are based on estimated risk to human health, rather than on ecological concerns. Even in those situations when ecological aspects cannot be ignored, a projected cause-effect relationship has often been challenged with arguments that other causes were responsible or that the change was part of natural variation.

Data exist that could be used to effectively test the assumptions, accuracy, and precision of measures of ecological change. Some of these data exist in post-impact monitoring of projects for which environmental baseline data and predicted effects were documented in environmental impact statements (EISs). Other data exist in academic or scientific agency studies. Currently there are no organized plans to accumulate and interpret the data, to provide well-documented case histories of cause-effect relationships, or to test various measures of changes. These goals could be accomplished through appropriately organized and funded efforts; the harvesting of these efforts, especially of the EIS work, should be cost effective.

There are many proposed indicators of change, but few have any measure of precision, and even fewer have a track record of having been useful to a variety of scientists in different environments. Many of the indicators of change are based on ecological assumptions that have not been critically tested. Because there is more prestige in proposing new indices than in testing existing ones, large numbers of potential indicators--each with a narrow data base and a lack of confidence--tend to accumulate within the discipline of environmental impact analysis. There is uncertainty about which variables should be included in environmental monitoring.

Many of the ecological indicators of change are based on single species data, either of individual health or population characteristics. These indicators of change include yields of economically important species, changes in abundance or reproductive success of obvious species (for example, birds, and turtles), growth rates or estimates of scope for growth, bioaccumulation of xenobiotic chemicals or metals, tumors or cellular abnormalities, enzyme levels (that is, excessive or depleted), energy state, and behavior.

All of the indicators of change are prone to local and annual variations. One serious shortcoming of bioaccumulation data is the lack of appropriate information from which to infer biological effects of the accumulated toxicant; unfortunately, most toxicity data are correlated with the exposure concentration, not internal concentration. Specific chemical causes have not been associated with cases of cellular or enzymatic abnormalities.

Indicators of change based on community structure include changes in species dominance, abundances of indicator species, similarities or difference indices to a reference (control) community, species diversity or evenness, vulnerability to invasion by exotic species, abundance of biomass or chlorophyll (especially for eutrophication), and patterns shown by remote sensing.

Most research has concentrated on species diversity because of its presumed relationship to stability of ecosystems, their properties, or their processes; this relationship is now under question. In many cases, the existence of stress is likely to eliminate sensitive species and increase the abundance of resistant species. However, under certain conditions, stress may also increase diversity. In practice it has not been possible to cite a specific value above which an ecological community might be considered as healthy, and below which its health is compromised.

Indicators based on community function have been hypothesized, and many have been demonstrated in specific cases; however, the class of indices has not been systematically tested to see if the generalizations are valid. Many of the indicators such as respiration, heat output, production/respiration or respiration/biomass ratios, nutrient retention or cycling, and physicochemical changes are based on the general assumption that natural succession causes predictable changes in ecosystems and that disturbance tends to force ecosystems to less mature states.

New research should be focused on community structure and function indicators. It is critical that known effects of stress be distinguished from, or related to, natural variations associated with climate or other cycles. Much of the new research should be experimental, such that replicated pairs of control and treated communities can be compared. These studies should involve natural communities, enclosures, mesocosms, microcosms, or any unit that is appropriate to the questions being asked. Single impact-control paired sites, or use of gradients associated with a stress, are the methodologies of choice in some cases, especially when long-term studies have been initiated. Studies must be conducted over a long enough period to assure that seasonal and annual variations are included.

Mitigating the Impacts of Current Technologies

Despite great improvements in pollution control and management, some technologies still pose potential threats to the environment. These technologies include industrial manufacturing, resource development, energy production, and agriculture.

Placement of sludges in landfills and soil application of liquids or sludges are the major sources of discharge to the terrestrial environment. If not managed properly, both may result in soil and groundwater contamination. Because a great deal of intermedia transport occurs, it is necessary to develop a multimedia focus for environmental research and regulation. This necessitates long-term research to enable us to determine impacts on all segments of the ecosystem.

Land application techniques are in need of such long-term research. Included in this category are the techniques of irrigation, rapid infiltration, overland flow, land disposal of sludge, and wetlands application of wastewater. The number of such techniques in use is steadily increasing, and post-construction performance evaluations as well as long-term follow-up studies are necessary to assess the potential hazard of long-term usage. In particular, additional information on pathogen survival and transport and on heavy metals build-up is desirable. Additional studies on the use of forest land for irrigation systems, especially under freezing conditions, are needed. Studies of process kinetics of overland flow systems are needed to aid in the design of such systems. The fate, transport, and transformation of contaminants in the soil need to be investigated. This information is necessary not only for prevention of impacts, but also for cleanup of contaminated aquifers. Studies on biodegradation, adsorption, and hydraulic transport are necessary.

Extensive research is needed on decontamination of groundwater. In particular, the fate of organic pollutants in an aquifer may depend upon the type of microorganisms present, and their ability to transform or degrade the pollutants. Information is needed on how these organisms interact and grow to determine their potential for aquifer decontamination. Studies are necessary to determine the extent and effect of nonpoint source pollution at a national level. To ensure adequate control of such pollution, followup studies to determine the effects of the controls employed need to be done.

Research on dechlorination techniques has been stimulated by findings of recent studies that chlorine combines with humic substances to produce significant amounts of potentially hazardous halocarbons. Researchers now believe that sulfur compounds may be best for dechlorination, but research on process kinetics is still necessary to determine proper operating conditions. Resulting effects of the dechlorination agents on the aquatic environment also need to be determined.

Research is needed on the fate of sediment contaminants. Factors that can affect the persistence and transport of toxic chemicals on sediments, and the release of toxic chemicals to the water, are not well understood. It has been suggested that such releases are an important source of exposure for aquatic organisms.

The long-range transport of chemicals in the atmosphere is a problem of considerable importance. Methods to monitor and control such pollutants must be developed. Of particular concern are the products of incomplete combustion that may be released during the incineration of hazardous wastes. Also of concern is the release of volatile compounds from industrial waste landfills.

The problem of acid deposition is still a major one. Studies on the atmospheric transport of sulfur dioxide and nitrogen oxides are needed. Increased research efforts should be focused on the importance of nitrogen oxides and other air emissions on acid deposition. Increased research will be needed to determine the mechanisms by which waterways may become acidified by acid deposition. Also, not enough is known about the effects of acid deposition to forests and croplands from acid deposition.

Insufficient attention has been given to developing methods of waste recycling and reuse, emphasizing more efficient and effective techniques than are now commonly used. In addition, not enough is yet understood about the problem of hazard evaluation and risk assessment. In particular, there has not yet been developed a comprehensive chemical monitoring program that enables analysts to assess long-term trends.

We believe that future environmental research on mitigating the environmental impacts of current technologies should have a multimedia focus, because extensive intermedia transport of pollutants can occur.

Anticipating Potential Impacts of Emerging Technologies

Genetic engineering is a specific case that illustrates some of the issues that arise in defining approaches and research needs for predicting potential environmental impacts of emerging technologies.

Environmental problems have often arisen in the past following the introduction of major new technologies. There are no compelling a priori reasons to believe that genetic engineering is any different in this regard. A number of hypotheses have been advanced in support of the view that the risk of genetic engineering is negligible, but these arguments, though having merit, are far from definitive. Of particular concern is the fact that the growth of this new technology will likely involve the application of gene-transfer techniques to an array of organisms, and that the consequences of this application cannot be predicted. Moreover, although major long-term ecological disturbances are not common in nature, they do take place, either when the natural community is upset, as commonly occurs in agriculture, or when a species is newly introduced into an environment. Although some of the upsets following the introduction of exotic species have been catastrophic, few well-documented explanations exist to account for the success of some introduced species and the failure of others.

The potential environmental consequences and risks of genetic engineering have attracted considerable attention, but they remain virtually unstudied. It is generally believed that the probability of a significant environmental change or upset is small, but no consensus exists on how small is small. A substantive base of scientific information and meaningful predictive tests is, therefore, required. This will allow development of a predictive capacity, from suitable testing protocols, to evaluate potential environmental impacts. Information is particularly needed on the following potential hazards:

- The likelihood that an organism will be deliberately or inadvertently introduced into a natural or human-altered environment;
- The probability that it will survive in that environment;
- The probability that it will multiply at that site;
- The likelihood that genetic information will be transferred from the released organism to other species and that the latter will survive and multiply;
- The probability that the organism containing genetic information of ecological importance will be dispersed to make contact with species that it can injure; and
- The possibility that some deleterious alteration will take place.

Research designed to detect released organisms is of great importance. Particularly among microorganisms, attention has been given to the development of such methods, but the techniques have rarely been applied to genetically engineered microbes. Information is needed on the effectiveness of containment procedures, routes of release from the laboratory or fermentation facility, the probability of release via each route, and on factors affecting the use of organisms debilitated to prevent their establishment in natural environments. Information is also needed on the potential for survival of organisms produced through genetic engineering, the environmental conditions allowing for the survival of organisms that usually do not persist, and the traits of species able or unable to endure in natural environments.

Scientists now believe that gene exchange takes place between microorganisms. This belief needs to be verified, and the factors governing the exchange need to be defined. To this end, the types of genetic exchange that take place, the organisms and conditions most likely to be involved, and the outcome of these transfers of genes need to be determined.

It is generally not possible to predict accurately whether a particular introduced organism will, or will not, be disseminated to a site where it may have an impact; hence, research is needed to establish the types of organisms that are susceptible to dispersal, the mechanisms of this dispersal, environmental conditions that favor or are detrimental to dispersal, and morphological and physiological characteristics of the organisms that make them more or less susceptible to dispersal.

Of critical importance is the potential of the genetically engineered organisms to affect natural populations and communities. Inasmuch as information is limited on the mechanisms by which harm is done to natural populations and communities, exploratory research on species interactions is needed. In performing such research, scientists should consider the basis for species interactions and whether experiments can be devised to predict the possible harm done by genetically altered organisms to established natural communities. Taxonomic categorizations of organisms (for example, pathogens, parasites, or predators) that affect ecologically important species interactions need to be established. Attention needs to be given to the design of suitable and low-cost model ecosystems to predict how genetically altered organisms might affect ecosystem structure and function. The utility of these model ecosystems for field predictions, however, must be validated. Research is also needed to establish the ecosystem processes particularly susceptible to disruption by pathogens, parasites, and predators.

A credible testing program to be used both by industry and by regulatory agencies cannot be devised until some of these information gaps are filled. Although much can be learned from the existing scientific literature, that literature is still too sparse and is often devoted to issues not relevant to species likely to be employed in genetic engineering. In addition, the extent of financial support for research in this area is extremely limited, and no major increase in funding has been publicly announced.

## RECOMMENDATIONS

We have adopted recommendations within five categories of research focused on the structure and functioning of ecological systems. In addition, we have added a sixth recommendation for shoring up the Nation's institutional resources and professional training programs. We emphasize that the conduct of research contained in our recommendations will require substantial increase in the amount, quality, and diversity and continuity of support for long-term environmental research. Specific recommendations by research category are as follows:

### Fundamental Studies of the Ecological Dimensions of Biospheric and Global Processes

- Illuminate the role of biological linkages and processes in specific biogeochemical cycles (for example, C, N, P, S, and O); and

- Characterize the interactions among these biogeochemical cycles and the relations between these and climate.

Applied Cause/Effect and Dose/Response Studies of the Impacts of Global Pollutants on the Ecological Dimensions of Biospheric and Global Processes

- o Search for biological interactions and ecosystem processes that are most sensitive to specific present, past, and anticipated global pollutants and environmental stresses; and

- o Characterize the interactions among global pollutants in producing impacts on the ecological dimensions of biospheric and global processes.

Fundamental Studies of Ecosystem Structure and Processes

- o Establish and sustain a national network of representative ecosystem sites at which long-term ecological studies can be pursued. This network should include both relatively natural and intensively managed (that is, agricultural, fishery, and urban) ecosystem study areas;

- o Sustain long-term biological inventories and records of baseline ecosystem structures and processes;

- o Identify those ecological dimensions and processes that are related to the sustainability of ecosystem resources and amenities, especially conservation of biological diversity. Initiate a new, comprehensive program (probably within the National Science Foundation) in conservation biology, including conservation biogeography, ecology, genetics, and behavior. It is currently impossible to determine priorities for conserving biological diversity because we have major gaps in our knowledge of geographic patterns of diversity. Substantially increased funding for systematics is also needed. Because most of the losses are occurring in the tropics, basic studies in tropical ecology are needed, particularly in freshwaters, nearshore marine ecosystems and lowland forests. One component of this research strengthening should be the extension of the National Science Foundation's Long Term Ecological Research program to include terrestrial, freshwater, and marine sites in the Neotropics;

- o Establish a research program to study the effect of global climatic change on isolated habitat "islands." Without accounting for the eventuality of climate change, the strategy of conserving biological diversity in parks and other reserves is unlikely to succeed; and

- o Identify processes that link different ecosystem types (for example, suburban-urban, agricultural-fisheries, industrial-agricultural systems).

Applied Cause/Effect and Dose/Response Studies of Technological Impacts on Ecosystem Structure and Processes

- o Initiate model ecosystem and field experimental studies of the impact of specific technology stresses on ecosystem structures and processes;

- o Search for early signals of ecological impacts and study their reliability for environmental forecasting;

- Search for attributes of ecosystems that offer opportunities for mitigation of environmental impacts;

- Search for safe means of simulating possible environmental impacts of emerging technologies (for example, genetically engineered new organisms, synfuels, and communication technologies waste streams and resource demands);

- Study interactions of various technological impacts on ecosystem processes and structures;

- Conduct research on how man-dominated ecosystems such as cities and industries can be moved towards closure, especially with respect to toxins; and

- Identify representative sites at which significant man-made or natural changes have already had an impact on or are about to have an impact on ecosystems. Document changes that have occurred based on published or unpublished pre-disturbance studies and on reconstructions from fossil and paleobiogeochemical studies. Specifically search for ecological changes that operate on very different time scales.

Studies of the Tools and Methods of Ecological Research, Risk Analysis and Management, and Long-Term Environmental Management

- Evaluate existing physical, chemical, and biological monitoring programs and records with the objective of identifying and stimulating R&D for improving their quality and cost-effectiveness as well as their utility in predicting ecological impacts and efficacy of mitigative measures;

- Evaluate the major national and international sources of long-term ecological and environmental management R&D with the objective of improving interaction among government, university and industry research groups; identifying the most cost-effective ways of strengthening long-term ecological research on high-priority national and international environmental challenges; and assessing the supply of trained professionals at work and in the training pipelines and identifying cost-effective means for ensuring a continued adequate supply; and

- Search for and evaluate strategies for environmental and ecological impact mitigation with the intent of facilitating their adoption when cost effective.

Institutional and Professional Training Needs

We feel that the long-term environmental research needed in order to address the problems raised above is not adequately reflected within the existing funding and management structures. Extramural environmental research sponsored by the U.S. Environmental Protection Agency (EPA), as well as EPA's Centers of Excellence Programs, are judged as steps in the right direction although both programs are inadequate to meet the research needs. Long-term research cannot be managed and funded piece-meal and in a temporarily unpredictable fashion. We discussed extensively the possibility of creating a National Center or Institute for Long-Term Environmental Research. The

National Center for Atmospheric Research was suggested as a possible model for the establishment of such a Center. At the same time, some of our Panelists feel that such a Center should be a step removed from the government and managed like a national laboratory by a consortium of universities. The inability of universities to manage long-term interdisciplinary research was mentioned as a deterrent to the success of basic environmental sciences and an argument in favor of a National Center for Environmental Sciences.

We also discussed the possibility of involving the private sector in funding long-term environmental research. The positive example of the Electric Power Research Institute, which has successfully managed a number of long-term multidisciplinary programs, was mentioned as a model for such involvement. Panelists feel that a general evolution from adversarial to collaborative relationships has been taking place between industry and government on environmental issues; this needs to be encouraged and used in the long-term development of environmental sciences.

The necessity of professional training in the field of environmental sciences and engineering was stressed by a number of Panelists. The establishment of training grants modeled after the practice of the National Institutes of Health was proposed. Such grants are needed to foster interdisciplinary programs in the leading universities of the nation. They should serve as a stimulus for overcoming traditional departmental structures that tend to be extremely stable in spite of all the efforts over the past decade.

## APPENDIX V

# Summary Report of the Expert Panel Meeting on Monitoring, Assessment, and Environmental Management

## INTRODUCTION

The meeting on Monitoring, Assessment, and Environmental Management was included in the series of expert panle meetings in recognition of the fact that protection of public health and management of environmental quality can be assured only through the use of reliable environmental data and analyses. The topic areas that served as foci for background papers for and discussion at the meeting were

- Data Generation, Collection, Analysis, and Interpretation;
- Health Risk Assessment and Environmental Impact Assessment Techniques;
- Modeling and Forecasting Techniques; and
- Environmental Management Approaches.

## THE CONTEXT

It is important from the outset to agree on the meaning and boundaries of our subject. Long-term environmental research and development (R&D) is taken to refer to a time period of approximately 3 to 15 years from the present, but even within that period it can be given several meanings: for instance, research whose outputs can be used in anticipating future environmental problems, research that requires multiyear time series data collection (such as prospective epidemiological and ecological studies), or "basic" research undertaken to illuminate fundamental properties of human and ecological systems that determine health and environmental effects.

A strong consensus emerged among Panelists to focus on future needs that are at least identifiable today, and not to attempt to identify wholly new concerns by speculation alone. In addition, the Panel decided not to limit its attention only to environmental concerns clearly within the authority of existing regulatory statutes. Because of the Panel's own mix of expertise, it may have unavoidably omitted some topics that other scientists may view as equally important as the ones considered; many of the examples cited, for instance, reflect the greater familiarity of the Panelists with pollution and toxic substances issues, rather than with environmental health, soil erosion and farmland protection, fishery and wildlife habitats, wetlands, and other topics on which long-term environmental R&D may be equally important.

In identifying long-term environmental R&D needs, a useful first step is to distinguish between research and monitoring: that is, between studies that attempt to improve our understanding of how environmental systems work, and studies that provide data on the status of those systems. Each is essential to the other in the sense that basic understanding must guide the choice of what parameters to monitor, and monitoring may in turn provide data from which postulates and theories are derived and assessed. The issue of reliability in the interpretation of data from research and monitoring studies, especially in the many situations of high uncertainty is an important one.

Research

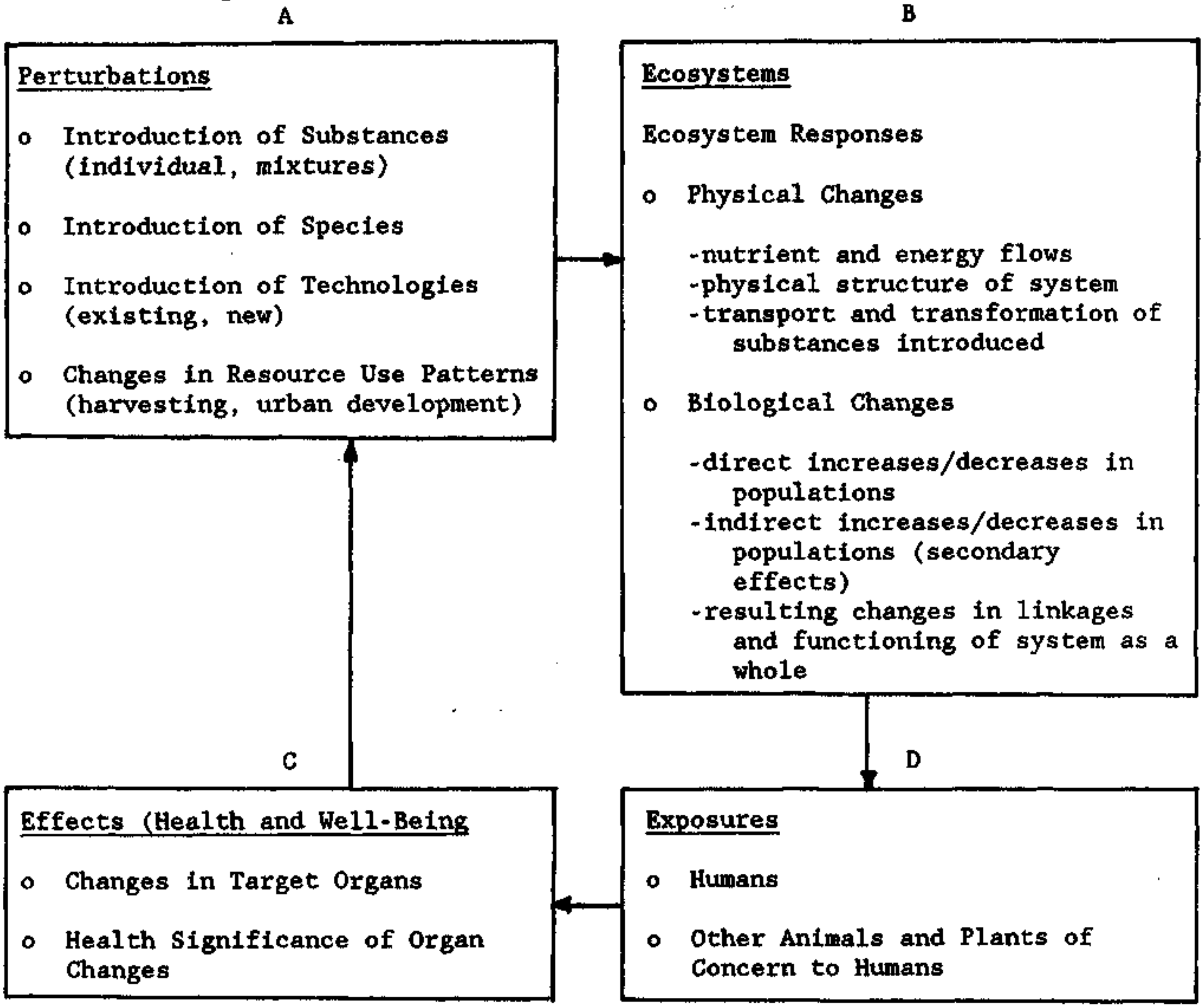
Figure 1 below is a simple conceptual model of how human actions may cause impacts on environmental quality and human health. The introduction of some human perturbation causes changes in the dynamics of an ecological system, these changes affect the exposure of humans and other living organisms, and such exposures in turn cause changes in those organisms that may have significance for their health and well-being. Human responses to these effects may also introduce additional perturbations that further affect the process.

Each of the four boxes, although greatly oversimplified, represents a cluster of questions that require answers from environmental research in order to permit sound management decisions. To understand the health effects of a toxic pollutant on humans, for instance, one must understand how much of it gets into the environment in the first place, how it is transferred within the ecosystem, to how much of it people are actually exposed (and by what pathways), and the acute and long-term effects of the biologically effective dose both on specific organs and on their overall health and well-being.

Typically, perturbations consist of complex mixtures of substances or impacts and equally complex patterns of human activities that influence exposure. Examples would include hazardous waste dumps and related groundwater problems. The dynamics of ecosystems are constantly affected by both manmade and natural perturbations; these ecosystems may transform substances into forms that are either more or less toxic.

Exposures occur via multiple pathways, and toxic pollutants enter humans via ingestion, inhalation, or contact. People are often exposed to complex mixtures of substances whose net effect may be synergistic, additive, or antagonistic, in multiple environments (at home or at work), and in amounts varying from single acute to chronic low doses.

Figure 1. Environmental Changes and Their Consequences




The effects of such exposure in turn may vary both by species and among individuals in how much of the toxic pollutant actually reaches target organs in the individual, how the pollutant changes the functioning of those organs, and how those changes in turn affect overall health.

Each of these aspects of environmental change gives rise to long-term research needs. In addition, there is a need for research on the linkages among them, especially between ecological systems and on exposures and health effects.

Ecosystems Research: We know relatively little about the functioning and maintenance of ecosystems and, indeed, it is important to address the basic question of what information we need to collect to determine if an ecosystem has been seriously disrupted. Major information needs related to this subject include the identification of critical and sensitive components of ecosystems, generalizations among ecosystems and types of stress, determinations of mechanisms of adaptation and/or damage, and valid methods for monitoring of normal ecosystem fluctuations.

Exposures: Much of the research on human exposure to environmental pollutants has been based more on modeling assumptions than on direct measurement, and has been fragmented among the various regulatory agencies and programs responsible for ambient environmental quality (and within that, air, water, and other programs), occupational health, food, and consumer products. Relatively little information exists on patterns of total exposure of human populations even to common individual substances, let alone to mixtures or interactive substances, despite the fact that assumptions about such exposures and biological dose are a key link in the chain between environmental pollution and human health effects.

It is clear that multiple exposures to multiple chemicals are the rule , not the exception, and it is important to determine total environmental exposure and total uptake by a wide variety of techniques in combination. The use of advanced detection techniques such as biological markers of exposure and animal and mathematical and models, is therefore a major research need.

Health Effects: How do exposures actually affect human hosts and their disease burdens? Much research so far has been focused on cancer and mutagenicity as endpoints, and on epidemiology and animal toxicology as major sources of evidence of health effects. The long-term effects of substances on the neurobehavioral system, on the reproductive system, and on the immune system remain largely unknown. The same is true of the contributions of pollutants to other chronic diseases such as those of the liver, kidney, heart, blood, and musculoskeletal system.

For the short term, substance-by-substance studies of cancer toxicology continue to be a valuable and necessary starting point. For the long term, however, greater emphasis is needed on a broader range of possible effects: on the basic mechanisms by which substances enter, interact with, and change the human body; and on the health significance of these changes.

Future Perturbations: Will patterns of human perturbations of the environment be substantially the same over the next 15 years, or will important changes occur that should affect research priorities? A credible long-term research agenda must be so designed such that researchers can take account both of trends and events that are now visible (either agreed upon or the subject of important controversy) and of long-term consequences that might be serious.

Monitoring

Good environmental research, monitoring, and management are iterative and interdependent. Research yields hypotheses about how environmental systems work; the outputs of monitoring are data about the status of those systems that permit both testing of those hypotheses and management responses. Monitoring thus ooought to be designed to serve as an essential complement to research, and must be closely linked to it, yet it raises such additional and difficult questions of long-term priorities as the following:

Purposes: No monitoring system can be designed to serve all purposes equally well, and most existing monitoring systems were designed to facilitate enforcement of existing pollution control regulations rather than to track basic ecological variables or to anticipate new problems. Few substances are monitored at all in the environment or in humans, and even fewer biological variables are systematically observed. What purpose(s) should guide the development of each monitoring system, and what compromises can and cannot be made to incorporate additional purposes without loss of scientific usefulness?

Design and Quality Control: What variables are appropriate indicators for the evaluation of monitoring strategies? What sensing elements, instruments, locations, frequencies of observation, data transmission, and analytical protocols will provide valid and useful measurements of environmental change? What analogous health indicators and tests are appropriate? How can data series already gathered for one purpose ("passive data sets") be used for other purposes and inferences?

Cost-Effectiveness and Continuity: Long-term monitoring is expensive and often little used; in addition, it frequently competes with research for the funds that support both activities. What biological and ecological variables are worth long-term monitoring, and what long-term research purposes do they serve? How can quality control and usefulness of the outputs be assured? What are the advantages of the continuity provided by long-term data sets, and how should these be weighed both against other data needs and against the benefits of altering measurement methods so as to incorporate more recent scientific advances?

Long-term monitoring programs may be an integral element of environmental R&D needs, if designed to provide valid and useful data to meet the most important R&D needs. It is thus important to reassess current efforts, to carry out quality control studies, to implement consistent time-series data on additional variables, and to create better linkages among monitoring programs.

Quality Control: The validity as well as the usefulness of some large environmental data systems should be periodically examined. It may be important to question the basic design of the systems, such as the locations of monitoring stations for particular variables, the range of variables measured, and the frequencies and time periods over which they are recorded. Other questions pertain to the quality of operation of the system, such as the training and reliability of the personnel who record the measurements and analyze the samples; still others concern the inferences drawn from the data, as, for instance, attempting to estimate ozone damage to crops when most ozone monitoring stations are in urban areas. Comparable problems of validity and quality arise in health exposure modeling.

Additional Considerations: Certain variables are so basic to a broad range of fundamental questions in environmental research that there would be broad scientific consensus on the need to monitor them on a long-term basis. Among the most obvious are data on climatic conditions such as rainfall and atmospheric temperature; on marine and estuarine conditions such as tidal fluctuations; and on such fundamental indicators of human health as mortality rates.

For many other similarly important indicators of environmental quality, however, we now have little or no monitoring data and often not even baselines against which to measure current conditions. Research is most often limited to short-term experiments that may or may not adequately reflect real systematic responses to stressors under field conditions, and models cannot be calibrated.

Interpretation of Contested Data Sets: A central problem in the application of environmental data to environmental management is the presence of uncertainties and conflicts over interpretation. Some of these conflicts may be rooted in legitimate differences of scientific opinion as to particular assumptions (for instance, the use of cell culture or choice of animal extrapolation models to predict human toxicology). It is important to be explicit regarding these assumptions.

Health Risk Assessment and Environmental Impact Assessment Techniques

Risk and impact assessments refer to two forms of applied analyses introduced into environmental management over the past 14 years, each intended to assure that managers have adequate estimates of the possible consequences of their actions to make well-reasoned decisions. A health risk assessment normally applies to a regulatory decision concerning the release of a particular substance into the environment, and involves the review and analysis of relevant scientific information about the substance's possible hazards and about the extent of likely public exposure to it. It is also sometimes applied to alterations of the environment, such as modifications of streams and wetlands.

An environmental impact assessment normally applies to any Federal action that may significantly affect human or environmental quality; it is most often prepared for actions by which Federal agencies undertake, permit, or support construction projects, and involves the inventory of environmental baseline conditions and the prediction of impacts on those conditions that would be likely to result from the proposed action or its alternatives, as well as likely measures to mitigate those impacts.

Both types of assessment are attempts to estimate possible consequences of future actions based upon available scientific information, in order to improve environmental management decisions, and both in practice usually require going beyond scientific estimates into the comparative evaluation and "balancing" of risks and benefits by the decisionmaker.

Health Risk Assessment: In concept, the rationale for health risk assessment is incontrovertibly simple. So long as government is expected to control some risks and can never have the knowledge or the resources to control all, some process of risk assessment is inevitable: the nature of environmental and health decisions implies it. The requirement of formalized and quantitative risk assessment (QRA) is intended to be an instrument of both scientific rigor and democratic accountability, ensuring that the inevitable process of assessing risk is documented in a reviewable public record and supported insofar as possible by scientific data and methods. A QRA for human health effects, for instance, in principle includes four primary elements:

- Hazard identification (the causal relationship between substance and effects);

- Dose-response assessment (the relationship between dose and incidence in humans);

- o Exposure assessment (the actual human exposures expected under plausible conditions); and
- o Risk characterization (the estimated incidence of adverse effect in a given population).

These elements of the risk assessment are the basis for management decisions balancing the social costs and benefits of reducing or prohibiting the risks.

QRA is in fact only one of several risk assessment approaches presently available, however. For instance, EPA now uses risk assessments in six of its major programs, including hazardous waste cleanup, and toxic substances; it conducts QRAs for carcinogenic effects, though often not for other types of health or ecological risks.

Scientific uncertainties often plague many of the assumptions of QRAs, including the extrapolations from particular laboratory animals as analogs to humans, from high to low doses, from small samples and single studies to large populations, from qualitative bioassays to QRA estimates, and even from intensively exposed or well-studied human groups to other populations. These uncertainties may be compounded by adding or multiplying them together in a QRA, a process which can lead to risk estimates that differ by up to 6 orders of magnitude. As a result, QRAs represent an imperfect analysis and cannot always be used; some experts prefer traditional professional judgment to a QRA procedure that depends on quantitative estimation.

There is substantial agreement among experts on the Panel and elsewhere, therefore, that risk assessment can benefit from long-term research.

Scientific Basis. The central scientific problem in risk assessments is not so much the risk assessment procedure itself but the absence of good understanding of the underlying systems and processes, as discussed in the earlier section on research. Too little is now known, for instance, about the basic pharmacodynamic and ecological mechanisms of toxic effects, about virtually any health (other than cancer) or ecological effects, about actual exposure patterns, and about the potentials of short-term biological screening to provide early predictions of effects. The primary long-term research needs therefore are also largely identical to those discussed earlier. The choice of assumptions, models, and significance criteria under conditions of genuine uncertainty may reflect differences among scientific perspectives.

Environmental Impact Assessment: Environmental impact assessment (EIA) has been well established as an analytical procedure for over 14 years; since 1978 it has been refined and codified in Federal regulations by the Council on Environmental Quality, including explicit procedures for establishing the scope of analysis in consultation with all interested parties. Although less controversial now than in their earlier years, however, EIA studies are still limited in their quality by certain fundamental gaps in long-term research. As QRAs would benefit from increased knowledge of health effects mechanisms, so EIAs could benefit from increased understanding of ecosystem dynamics. Most EIAs are focused on fish and wildlife, plant conservation, and endangered species, rather than on small species--such as microorganisms and pollinators--that may perform crucial ecological roles.

Modeling and Forecasting Techniques

Scientific models are commonly defined as quantitative expressions of hypotheses, ideas, concepts, or observations that permit pertinent information and data to be assembled, processed, and evaluated. In this context, environmental modeling includes a large number of instruments of several different types: data collection guides, assessment frameworks, evaluation techniques, and tools for predicting environmental effects over a wide range of situations and conditions. Over the past five years, an increasing number of Federal agencies and programs have turned to simulation and optimization models, as well as other kinds, as aids to environmental management. Some of these models have been developed within the agencies; most, however, depend heavily on modeling expertise in universities, national laboratories, and consulting firms.

At their best, models may improve understanding of ecosystems, reveal logical inconsistencies and gaps in knowledge, and focus and communicate information in a clear and concise fashion. They remain one of the few quantitative tools available for exploring the hypothesized effects of human perturbations on natural systems. Some models have only a few or a single user; and the complexity of some models reduces their usefulness as tools for understanding or prediction. A large proportion of models lack adequate documentation and verification. Models also reflect, of course, the quality of the assumptions and data that go into them.

Environmental Management Approaches

Environmental management depends on data and monitoring, assessment, and modeling, but also it is a subject of long-term R&D needs in its own right, involving two subject areas. The first of these is habitat management, the purposeful manipulation of ecological conditions. The second is the management of environmental programs, including the uses of research, monitoring, assessment, modeling, and the design and evaluation of alternative regulations, incentives, and other types of government interventions.

Our environment is sustained by a basic structure composed of biota, land, water, air, and energy resources, which interact in a dynamic manner. In the past scientists were seldom asked about the full consequences of human intervention. There is a need for more holistic, interdisciplinary approaches to the biophysical environment.

Federal environmental programs are conducted under a series of several dozen laws, covering different media, enacted at different times, and administered by different offices and programs. Long-term research is needed to improve the scientific basis for integrating these environmental programs into a single environmental policy.

## RECOMMENDATIONS

The Panel believes that its discussions and recommendations must be focused on two critical components in the use of data for environmental management purposes: research and monitoring.

### Research

For the research component, the Panel recommends attention to the following types of studies:

The Dynamics of Stressed Ecosystems: We recommend that attention be given to the collection of better long-term information on (1) the changes that occur in ecosystems in response to manmade stresses, with the goal of understanding what changes occur in response to what elements of the stress, and by what processes; (2) whether such changes are transient, adaptive, or pathological; (3) how such changes are avoided, amplified or mitigated by natural (that is, nonhuman) sources of fluctuation and stress in the system; and (4) what actions to provide additional avoidance or mitigation might be practical and effective. Stresses in this context refer not only to discharges of chemical pollutants, but to the full range of human development and use patterns, with priorities set according to which stresses appear most significant. This research is especially needed for ecosystem types in which changes are irreversibly damaging, and for those that have great value to human populations and are under great pressure from manmade stresses, such as wetlands and estuaries.

Ecologically Significant Endpoints: We recommend that as the dynamics of stressed ecosystems are better understood, studies are undertaken to identify specific changes in measurable system process parameters and species populations that define thresholds of ecological significance--for instance, those that foreshadow qualitative and perhaps irreversible changes in the functioning of the system. These parameters and organisms should therefore become potential endpoints for regulatory attention. (Some parameters may not have any thresholds). Simple physical and chemical variables (such as pH and dissolved oxygen) are already used as crude endpoints for some purposes; additional endpoints may be needed for other physical and chemical variables (such as groundwater quantity and quality, and nutrients and toxins) and for biological variables (such as biocides and species composition).

Ecological Markers and Sentinel Events: We recommend that research be undertaken to identify early indicators of potential ecological change, such as especially sensitive species or interactions between species whose condition if monitored provides a precursor or sentinel change to warn of impending adverse effects on the system. Mussels have already been identified for this purpose in some marine systems, and ceriodaphnia, fathead minnows, and algal changes in streams; we recommend that research be done on analogous markers both for other types of ecosystems and for additional types of stresses. Bumblebees, earthworms, and fungi have been suggested as indicators of change for terrestrial systems, for instance, and trout, salmon, and other fish species for aquatic systems. We recommend that particular attention be given not only to the usefulness of individual species, but also to the probable need for multispecies monitoring protocols in many systems.

Total Toxicity of Complex Mixtures: We recommend that research be devoted not only to the effects of individual substances, but to the ecological toxicity of complex but common mixtures, such as solvents, agricultural runoff, and wastewater sludges, as well as to airborne chemical mixtures subject to downwind deposition. Mixed types of perturbations such as those involving soil colloids and industrial chemicals, or acid deposition coupled with forest harvesting, also require study.

Movement of Pollutants Out of Sinks: Large quantities of toxic pollutants from past disposal are already present in the environment, stored for unknown time periods in ecological "sinks" such as wetlands, other underwater sediments, landfills, and others. Many pollutants may move across media, however. We recommend long-term research to identify the conditions under which these materials and their conversion products could move out of such sinks and reenter ecosystems and human environments, and rates at which such movement could occur. Research is also needed on the potential for in-place detoxification and immobilization of such hazardous sinks, and on the possible ecological and human health risks of such alternatives.

Human Exposure: We recommend that particular attention be given to developing actual data on human exposures from all environmental sources: for inhalation in the ambient air, in the workplace and other indoor environments; from smoking and other consumer products for ingestion of food and water; and for absorption of some substances by skin contact. In setting priorities, we recommend that consideration be given to what substances and mixtures pose the most significant and widespread hazards and to what variables to monitor and what monitoring methods to use. This line of research clearly deserves increased long-term attention. Little is also known about the relative importance of small numbers of acute exposures versus low-dose, chronic exposures; better understanding of these issues can only occur with increased research on actual exposure patterns.

Multiagency Studies. The research needs detailed above cut across not only programmatic lines within agencies like EPA (such as with EPA's air and water quality and toxic substances programs), but also the boundaries dividing EPA's regulatory responsibilities from those of the Occupational Safety and Health Administration, the Consumer Product Safety Commission, the National Centers for Disease Control, and others. The Panel therefore encourages the initiation of coordinated research and monitoring studies of exposures, involving both the relevant regulatory and research agencies.

Effects on Multiple Organs and Physiological Systems. We recommend the expansion of health effects studies to cover a broader range of possible health effects than are conventionally included in current research efforts, which are focused on studies of cancer and genetic mutations. Logical extensions of research efforts would be expanded studies of neurobehavioral and immune systems.

Climate Perturbations. Most analysts estimate that fossil fuel combustion will increase worldwide, and that the magnitude of possible effects on global temperature, carbon cycles, and other parameters will similarly increase. Other manmade impacts for which some scientific data suggest potentially significant climate effects include tropical deforestation, long distance transport of atmospheric pollutants, and large numbers of nuclear explosions. We recommend that continuing and intensified research attention be devoted to these potential perturbations and their effects.

Biotechnologies. The current knowledge explosion permitting genetic manipulation of biological organisms makes possible potentially vast changes in natural ecosystems and human health, and in humanly managed agricultural, forest, and aquatic ecosystems. Although many of these changes may be beneficial to human well-being, the potential also exists for serious unintended adverse effects. Given the self-reproducing nature of some new biological organisms, we recommend that research on possible consequences be at least as thorough as that on product development.

Linkages Among Ecological Systems. We recommend the expansion of research on the transport and fate of materials between heterogeneous ecosystem units, such as the movement of soils and their residual chemicals from agricultural lands into streams and lakes, and the movement of chemical pollutants both downward through the unsaturated soil zone into groundwater and laterally in groundwater.

Monitoring

For the monitoring component of data collection for environmental management purposes, we recommend that attention be given to the following types of studies:

Reassessment: We recommend as a short-range research need that a systematic reassessment be conducted of the breadth of major environmental monitoring systems that are now in operation, the uses, values, and limitations of these systems, and the extent to which these systems serve or fail to serve the expected future needs of long-term environmental research with special attention to what are broadly called toxic chemicals. This reassessment should include substantial involvement of research scientists who are the ultimate users of monitoring data. The most successful monitoring programs in terms of both viability and usefulness appear to be those that have direct and accountable ties to user communities.

We recommend the establishment of a separate professional environmental data or monitoring unit to serve the needs of researchers and managers for accurate data, to generate more effective involvement and oversight on the part of the scientific community, and to stimulate more productive discussions between theorists, instrumentation specialists, and data collectors.

We recommend that all new environmental monitoring programs be peer-reviewed initially and audited at regular intervals by independent statisticians to assess the validity and reliability of their design and operation. Such a review could involve specialists in such fields as remote sensing and data transmission and integration, to ensure that the most cost-effective and dependable data collection and analytical technologies are used.

Biological Indicators of Ecosystem Health: We recommend the expansion of research efforts on both ecologically significant endpoints for regulatory attention and on species that provide early precursors of possible ecological harm.

Sensitive Populations: We recommend research to monitor populations at risk that have a likelihood of serious exposure and health effects by virtue of their biological susceptibility or their high probability of significant exposure. Examples are occupationally exposed workers, pregnant women, and others at particular risk. We recommend that research studies be undertaken on both the health of such subgroups and on the significance of their responses as precursors of effects on others.

Integrated Data Bases: We recommend as a long-term research goal the development of integrated data bases providing better data than are currently available on the flows of combinations of hazardous materials both into and among ecosystems, and through multiple exposure pathways to human health effects. As initial steps, we recommend that particular attention be given to linking quantitatively the monitoring data sets on discharges, environmental transport, exposures, and human health effects, for such common substances and mixtures as lead and benzene. We also recommend design studies in which environmental monitoring is combined with more detailed medical surveillance of small groups. In some cases environmental monitoring could be added to experiments where small groups are already being studied on a long-term basis.

Health Risk Assessment and Environmental Impact Assessment Techniques

The Panel has adopted the following recommendations relative to human health risk assessment and environmental impact assessment techniques:

Risk Assessment: For health risk assessment, we recommend emphasis on the the following research needs:

Basic Mechanisms. A central need is for better understanding of the dynamics of ecological and physiological systems under stress, and of the mechanisms by which particular stresses cause adverse health effects.

Noncarcinogenic Effects. In particular, more research is needed on mechanisms and effects other than cancer, since attention so far has been devoted almost exclusively to cancer rather than to other effects that could be equally serious: effects, for instance, on reproduction and genetic mutation, on neurobehavioral development, and on the immune system.

Exposures. Exposure data are often weak in QRA, deriving from inferences from only one or a few limited studies (if any) rather than systematic evidence. Monitoring of sensitive groups, and other studies linking environmental monitoring to medical surveillance, would help to fill this gap.

Short-Term Tests. Short-term bioassay techniques are now being developed and tested as possible alternatives or adjuncts to the long-term bioassays now used to test chemical toxicity. These techniques, if successful, appear to be the best hope for screening enough substances quickly and cheaply enough to match the rate at which new substances are synthesized and introduced into commerce. Additional long-term research will be needed to develop and refine these techniques, especially for noncarcinogenic effects as well as cancer.

Social Sciences Research. Current social sciences research on decision processes and on the bases for QRA inference and assessment guidelines can provide a useful context for incorporating in QRAs the influences of publically held values and attitudes, criteria for setting risk priorities, and those factors that influence public understanding of risks.

Validation Studies. Risk assessments should be subjected to independent validation studies both to identify points of weakness and to evaluate their sensitivity to alternative assumptions and models.

Environmental Impact Assessment Techniques: We recommend that research attention be devoted to the following issues relating to possible improvements in environmental impact assessment techniques:

Ecosystem Dynamics. Research attention is needed to basic flows of materials and energy, natural fluctuations in these processes and in representative species populations and interspecies interactions, responses to both manmade and natural stresses, the transport and fate of toxic substances in ecological systems, and linkages between media and ecosystems. A primary focus would be on long-term studies of stressed ecosystems and long-polluted sites, where baseline data and some long-term monitoring already exist and potentials for recovery and rehabilitation can also be investigated.

Consistent Baselines and Monitoring. Continued research attention is needed to the establishment of reliable monitoring networks for basic parameters across a wide geographic range of sites and ecosystem types. Much of this could probably be done by better coordination among existing sites and organizations, as is now beginning to occur for acidic deposition phenomena.

Retrospective Validation. Thousands of EIA studies have been completed, but rarely, and never on a regular basis, has their validity been evaluated. Additional retrospective evaluations should be conducted of the predictive validity of EIAs, and recommendations developed for improving their methodology where appropriate.

Modeling and Forecasting Techniques

Such techniques are viewed by the Panel as critical tools to assist environmental management. We recommend particular attention to the following long-term research needs for these tools:

Verification: We recommend that existing models be verified before development of new ones proceeds. Verification studies should include clear documentation of each model, analysis of the model's sensitivity to particular assumptions and design criteria, discussion of why particular hypotheses or theories were chosen rather than competing ones, testing where possible against actual time series data (requiring links to the improvement of monitoring, discussed above), and verification of the mechanisms postulated by the model as well as the outcomes.

Simplification: As simulation models become very complex, verification and comprehension become difficult. Some large models may be too stable, and thus they will not accurately represent stresses that could in reality cause sudden qualitative change in the system. A research effort should be made to simplify environmental models as an aid to understanding.

Interactive Modeling: One of the important potential benefits of quantitative modeling is as a direct aid to management problem analysis and decisionmaking. To achieve this end requires modeling capability that is relatively simple and transparent and highly interactive with the users. We recommend long-term research on interactive techniques for management modeling at a widely usable level, such as with current microcomputer technology, and training efforts to encourage testing and refinement of such techniques in actual management situations.

Adaptive Phenomena: We recommend long-term modeling research on the dynamics of ecological system adaptation or non-adaptation to stressors, including especially the conditions under which qualitative changes might occur in the system that would be either adaptive or pathological.

Environmental Management Approaches

Discussions at the panel meeting on extant and alternative environmental management strategies were focused on three issues requiring substantial research attention. These issues were (1) environmental management research, (2) the administration of environmental programs, and (3) the institutional framework for environmental management.

Environmental Management: We recommend the following long-term research emphases to foster achievement of better integration of both the understanding and the goals of environmental management strategies:

Interdisciplinary Studies in Ecosystem Management. Studies are needed to link research and monitoring of the dynamics of stressed ecosystems, especially in important types such as wetlands, estuaries, and agricultural ecosystems, with experimental evaluation of alternative management strategies. Such studies require substantial periods of time for completion, as well as interdisciplinary cooperation among a substantial range of natural and social science and engineering disciplines. One particular need is for studies of rehabilitation strategies for degraded ecosystems.

Cross-Media Management Strategies. When pollutants are released into the environment, they may be discharged either to the air, the water, or the land. When they are treated, they may in part be rendered less harmful, but in fact they may simply be transformed: for instance, a decrease in water pollution may create an increase in sludge. Pollution control programs are often focused by media, a factor that limits the development of multimedia solutions to pollution. Long-term research is needed to develop and test strategies for monitoring cross-media effects, and then for optimizing pollution control strategies both across the several media and across multiple routes of human exposure.

New Technology Management. Long-term research is needed on the possible ecological effects of new technologies such as biotechnology, and on scientific resources available to anticipate particular consequences.

Interagency Coordination. Many of the needs identified by this Panel cut widely across existing agencies and programs. We believe that CEQ could foster the implementation of this research agenda. Particular attention is needed to issues such as coordination of data collection and monitoring activities, and establishment of a clearinghouse for environmental modeling; to further development and explication of general methodologies for impact and risk assessment; and to generating multi-agency research and management initiatives on key topics, such as ecosystem management studies and regional integrated regulatory experiments.

## Related Social Science Research

We believe that attention should also be given to social science research on topics relating to environmental management decisionmaking, the utilization of environmental science, and innovative environmental management strategies.