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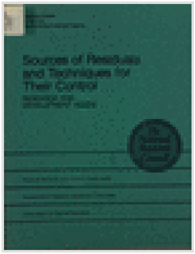

Sources of Residuals and Techniques for Their Control, Research and Development Needs: A Report (1977)

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Analytical Studies
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Environmental Protection Agency

Sources of Residuals and Techniques for Their Control

RESEARCH AND DEVELOPMENT NEEDS

A Report of the Panel on
Sources and Control Techniques
to the Environmental Research
Assessment Committee

Commission on Natural Resources
National Research Council

NATIONAL ACADEMY OF SCIENCES
Washington, D.C. 1977

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NOTICE

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

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

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

This study was supported by the Environmental Protection Agency.

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PREFACE

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

The prime objective of the ERAC study was to examine the processes by which information is acquired by EPA through research and development. Because these processes have both managerial and scientific aspects, the Committee divided its work into two parts. One part, concerned with the organization, coordination, and management of research and development to support the Agency's mission, is the subject of a separate report by ERAC itself (National Research Council 1977b). The other, which deals with the identification of opportunities for research and with strategies for guiding research planning, was divided among the four panels--on sources, fates, and effects of pollutants, and on the environmental impacts of resources management. The report of the Panel on Fates of Pollutants deals with research needed on the transport, transformation, and accumulation of pollutants in the environment. The report of the Panel on Effects of Ambient Environmental Quality deals with research needed on the effects of environmental pollution on living and nonliving things. The report of the Panel on Environmental Impacts of Resources Management deals with research needed on the environmental consequences of the development and use of natural resources, particularly. The present report, prepared by the Panel on Sources and Control Techniques, discusses research needed on the generation of residuals and strategies for their control.

There are two possible approaches to employ in a study of research needs on sources of residuals and techniques for their control. The conventional approach identifies research areas by nature of source, (e.g., municipal, industrial, radioactive, agricultural) and by medium (e.g., air, water, land). The generic approach recognizes common problems among all sources of residuals. The advantage of the conventional approach is its general familiarity; its disadvantages are its tendency to obscure many of the non-technological factors that influence the generation and distribution of residuals, and its failure to recognize critical gaps in information that may occur outside conventional areas of concern. The Panel on Sources and Control Techniques chose the generic approach to overcome these disadvantages and to seek out and emphasize recurrent themes that emerge from, and apply to, the more familiar approaches to residuals control.

At an early stage of its work, a number of critically important research needs were identified. In weighing these needs and methods of coping with them, the Panel obtained the views of individuals who have special knowledge in the areas of concern. These views were presented in a series of reports on file at the National Research Council and are available on request from the Environmental Studies Board, National Research Council, 2101 Constitution Avenue, N.W., Washington, D.C. 20418. The titles and authors of these working papers may be found in the Appendix.

Several concerns, long familiar, are of obvious importance:

a) Identification of the substances and forms of energy discharged to the environment in the course of the human activities of production, consumption, and transportation.

The Panel, in its deliberations, has conceived of these discharges as "residuals". The concept is basically an economic one: it recognizes that these discharges are "leftovers," discarded because existing market prices (and any government-administered prices or subsidies) do not make it worthwhile to recover materials, by-products, or energy for any other subsequent use. Once in the environment, the discharged substances and energy may produce results that are, from man's point of view, either good, bad, or indifferent. If the effects are bad, we call the substances "pollutants." It is necessary, however, to recognize that not all residual discharges at all places and times are pollutants. For example, waste-heat discharges in certain locations or for limited periods may produce desirable results, such as faster growth rates for fin and shell fish.

b) Measurement of discharges and the understanding of how the generation of residuals varies with such influences as input mix, production (or consumption or transportation) technology, relative prices of by-products, output specifications, etc.

In this terminology, residuals "generation" is measured before any control efforts mandated by public environmental policy, but after any processes that capture input materials or by-products in response to internal motives. This distinction is clearly most useful and relevant with respect to industrial plants (including mining and forestry operations).

c) Development and application of alternative technologies for changing the quantities, qualities, timing, or location of residuals discharged to the environment. These technologies can include input and by-product recovery units, recirculation equipment, end-of-pipe (or stack) treatment plants, storage options, and residuals transportation techniques.

d) Determination of costs and effects of technologies listed in (c), including the distribution of those costs over society.

In addition to these familiar subjects, the Panel recognizes the importance of assessing the needs for research on institutional arrangements relevant to the management of environmental quality, especially institutions that impinge on sources. This last topic is a broad and fundamental one. Existing institutions are important influences upon both research agendas and efforts to control sources. They largely determine which sectors of society have incentives (a) to search for and develop new control technologies, including those to reduce generation in the first place, (b) to investigate the causes of failure in existing control units, and to produce corrective actions or equipment improvements, and (c) to assess effects of particular residuals on environmental systems and human health.

Accordingly, this report does not take existing institutions for granted. Rather it stresses the importance of research into alternatives, and seeks to make clear the extent to which alternative institutions imply changes in research and in the more traditional areas of EPA interest--especially development of technology.

This report does not attempt to identify the many important fields of research on natural processes or the equally important development projects that might yield new control technology. There are a great many valuable existing programs that warrant continuing support, and each of the specialized scientific and professional communities is equipped to identify its own high-priority research needs, and to do so from knowledge in depth.

The purpose here is rather to highlight some general areas of research that call for special attention although significant, they have been relatively neglected in the past. This report is intended to bring these important but infrequently considered questions and issues to the attention of leadership in EPA and other federal agencies, interested Members of Congress and their staffs, and concerned citizens.

The Environmental Research Assessment Committee wishes to express its appreciation for the contributions made by the members of the Panel on Sources and Control Techniques in the preparation of this report, for the cooperation of the members of the various agencies and institutions, and for the assistance and support of the consultants. It also wishes to acknowledge the contributions made by the NRC staff, particularly the dedicated work of Karen M. Slimak who, as Staff Officer for this panel, provided invaluable support and editorial assistance.

John M. Neuhold
Chairman
Environmental Research Assessment Committee

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SUMMARY

The four sections of this report represent four major generic problem areas in research on sources of residuals and techniques for their control that should be addressed by EPA: institutional issues, economic issues, the anticipation of environmental problems, and technology. Each section presents a general background discussion followed by specific recommendations, each with a supporting and elaborating statement. The recommendations are summarized below.

INSTITUTIONAL ISSUES

A major conclusion of this study is that institutional issues are not being given sufficient attention by the environmental research community in general and by EPA in particular. Alternatives should be sought, both to current methods of decision making and to present management structures. It is becoming increasingly apparent that planners must be able to weigh control alternatives in terms of multiple combinations of institutional, economic, and technological possibilities. In addition, the choice of institutions and incentives has important implications for the formulation, support, and conduct of environmental research more broadly. Therefore, this report, which emphasizes the paucity of research effort on institutional and economic aspects of residuals control, should be viewed in the light of current institutions and possible alternatives that may influence implementation.

> Recommendation 1 - EPA should undertake empirical studies of existing state, local, and foreign environmental management systems to identify and evaluate conditions that influence efficiency of performance and to provide a basis for selecting or modifying management systems.

Recommendation 2 - EPA should support research to determine the feasibility of adapting environmental management systems to approaches that have proven successful in the management of other domestic social programs.

More narrowly, the control of hazardous materials particularly needs institutional innovation, since the threats posed by man-made toxic substances (including carcinogens and mutagens) are grave problems facing society.

Recommendation 3 - EPA should seek to identify innovative methods for use by institutions managing hazardous materials. The methods should be analyzed to identify incentives for adopting non-hazardous alternatives. Assessment of the methods should also judge how well they deal with important characteristics of hazardous materials involving low-level chronic exposure, wide environmental dispersion, and long latency.

An apparent inconsistency exists between the widespread professional enthusiasm for economic incentives as an environmental management tool and the great skepticism, even hostility, with which this class of policy instruments appears to be regarded by legislators and executive agency leaders. The reasons for this inconsistency seem worthy of careful investigation.

Recommendation 4 - Research should appraise the utility of economic incentives as an element of a residuals-management strategy. Specifically, this research should seek to: (a) provide an understanding of why such incentives appear to be ignored by policy makers, and (b) explore ways of defining and constructing incentive systems that will be applicable.

Successful public policy rests ultimately on the support and cooperation of individuals throughout the society. In an area such as environmental quality, where information is scarce, wise formulation of policy requires research into how those concerned with environmentally important decisions perceive the problems and the needed policies.

> Recommendation 5 - EPA should launch an entirely new program of research into personal perceptions and inferences regarding pollution, health risk, and value of environmental amenity to assist public agencies in developing realistic strategies for the management of residuals.

ECONOMIC ISSUES

Whereas economic issues relevant to sources of residuals discharges have not been neglected by EPA to the same extent as have institutional issues, serious gaps remain in our ability to provide relevant economic information to policymakers. We have identified three such gaps that are subjects of research recommendations. (A closely related recommendation concerning the costing of specific units of residuals control technology is included in the section of the report dealing with technology.)

> Recommendation 6 - EPA should support research to understand better the magnitudes of and the factors influencing the costs associated with the management of residuals.

> Recommendation 7 - EPA should expand and intensify its efforts to produce a practical, operational method for assessing on a regional basis the magnitudes of costs and implications for ambient environmental quality, as appropriate, of alternative residuals management policies, including ambient quality standards, technologically-defined discharge standards, and input requirements or restrictions.

Recommendations 6 and 7 deal with matters of economic efficiency, but equity is equally important in the formulation of public policy. Because the benefits created by improvements in environmental quality are widely diffused, knowledge of the distribution of the costs of those improvements is potentially of great significance. At this time, however, knowledge about the distributions of costs and benefits of specific environmental programs is very sketchy, and techniques for making estimates of distributional patterns are underdeveloped.

> Recommendation 8 - EPA should continue and, if possible, expand its research aimed at increasing knowledge of how costs of alternative policies for residuals are distributed over income classes, geographic areas, and political jurisdictions.

THE ANTICIPATION OF ENVIRONMENTAL PROBLEMS

Some serious environmental problems have appeared as surprises to regulators and public alike. Some think the frequency of such surprises is increasing, and the implied threat is that equally or more serious dangers may be lurking just outside our range of knowledge. Given the characteristics of some hazards man is creating, it is entirely plausible that very extensive human damages will already have been created by the time we discover the threat--effects that no practical public policy can reduce. Accordingly, the anticipation of environmental problems is a research area of great importance. By systematic examination of the chemical, physical, and biological characteristics of a proposed new substance, it may be possible to identify those that are more worrisome and those that may require more intensive study before release to the market place, even though it is difficult to make specific recommendations for research, particularly regarding long-term effects.

Many of our current difficulties result from the nature of human perceptions of problems and from the inadequacy of their assessment over time. The remedy lies in improved understanding among decision makers, who need better knowledge about risks and better incentives to induce error-correcting behavior. Accordingly, we wish to re-emphasize here the importance of Recommendation 3 above, which deals with the need for innovative methods for use by institutions managing hazardous materials. We also recommend some specific activities that may improve the probability that real dangers are predicted. The question of whether, among the welter of predictions, the real dangers are recognized and appropriate actions taken is, of course, the truly difficult one, the answer to which appears to hinge on institutional design.

> Recommendation 9 - EPA should extend and expand its programs aimed at identifying and measuring the chemicals and other potentially hazardous materials that are being introduced--by use, discharge, or accident--into the natural environment.

> Recommendation 10 - EPA should continue to support the development and application of modeling techniques that aim at predicting future environmental problems by combining current knowledge of hazardous materials and of products of their reaction in the natural environment with predictions about the rate and direction of economic change.

> Recommendation 11 - EPA should institutionalize the ability to conduct studies of "environmental surprises" as they occur. These studies should look at the historical background of relevant predictions and regulatory judgments as well as the characteristics of the actual events and responses to them.

TECHNOLOGY

Although our discussion treats technology research last, we do not intend to deemphasize the need for such research. Doing so would be totally unrealistic. We are, however, mindful of the historic and relatively well-developed national program of research on control technology and of continuing efforts to identify research needs by a wide range of experts in engineering and science. Authoritative descriptions of such needs have been prepared in breadth and detail by professional organizations and other advisors in the environmental fields. There is, indeed, little new that this Panel can add to these presentations.

While this report emphasizes the implications of alternative institutions for both implementing policy and conducting research, an important role for public research into control technologies would likely exist under nearly any system of residuals management. In particular, even with strong institutional incentives for and reliance on private research, EPA would probably still have a role as "developer of last resort" where capital or organizational constraints make the incentives ineffective. Finally, we recognize that there are tasks that extend beyond the reasonable area of responsibility of individual entities concerned with applying environmental control technology.

Within the current system, we have identified four generic tasks in technology research that EPA could better perform:

> <u>Recommendation 12</u> - <u>EPA should direct further research toward identifying factors that affect the efficiency of performance of installed municipal, agricultural, mining, forestry, and industrial wastewater management systems, and of air emission control technologies, for both stationary and mobile sources</u>.

> <u>Recommendation 13</u> - <u>EPA should, in its current role of developer of control technologies, make a serious effort to estimate the magnitude of the costs of installing and operating the control units developed. These costs should be reported in a form that makes them useful to prospective users as well as to those developing plantwide cost models advocated in Recommendation 6 above</u>.

> <u>Recommendation 14</u> - <u>EPA should expand its support of studies on the generation and control of sources of pollutants whose origins are in the transformations of residuals in the ambient environment</u>.

> <u>Recommendation 15</u> - <u>EPA should support research that will reduce the losses of valuable materials in wastes</u>.

CHAPTER 1

INSTITUTIONAL ISSUES

BACKGROUND

The Panel places high priority upon research directed to assessing and designing institutions that, in turn, more effectively manage environmental quality. This research should receive compensatory attention, because it has been inadequately regarded in the past.

Decisions about how good the quality of the environment should be are inherently political. However important "good science" may be in identifying alternatives, there are no scientific answers to the fundamental questions of what risks society should run or how it balances material well-being against aesthetic values and health. Much public debate in the past has tended to obscure the relations between technical questions and political ones. One result has been a concentration of the research effort, especially within EPA, on scientific and technological matters, to the virtual exclusion of exploration into the mechanisms that translate science--and economics, public perceptions, emotions, etc.--into public policy.[1] The record of the past two decades contains strong evidence of the inadequacies of this approach: a seemingly inexorable trend toward more and more centralized, cumbersome, and politically unaccountable decision making; increasingly complex regulatory methods; and an unfortunately static approach to the problem (Kneese and Schultze 1975).

There are, as implied above, two levels of institutional research that can usefully be distinguished. One concerns processes for making fundamental decisions about environmental risks and about levels of quality in relation to employment, traditional measures of economic welfare, and so forth. On this level falls research into such problems as legislative structure, the changing nature of administrative law, the role of the courts in reviewing actions of executive agencies, and the appropriate jurisdictional level at which to make particular decisions. The aim of such research is to understand how the political system--in the broadest sense--works, and how its current functioning might be improved. EPA, as a central part of the existing system, may not be in a position to do or to fund research on this level. As an alternative, a "National Environmental Institute," should one be formed, would be an appropriate locus for such research--assuming sufficient freedom of inquiry to allow questioning of any aspect of the existing system. Alternatively, a private research institute could become the central locus for this research; indeed, freedom of inquiry might be significantly greater if the research were privately financed.

A second level of institutional research relevant to the environment concerns the mechanisms for translating the larger decisions into appropriate management actions. Under this heading would come such topics as the potential for translating desired levels of ambient environmental quality into limits or incentives for dischargers, possible ways of dealing with short-run dislocations caused by toughening environmental policy, and ways of harnessing the very great innovative forces available within our economy in the search for better technologies. It is possible that research at this level could be done within or supported by EPA since the questions asked do not really involve the foundations of the system. It appears, however, that in the current climate even such issues as the relative desirability of discharge regulation and economic incentives have taken on the overtones of high political debate, and may therefore be difficult for EPA to address.

There are serious obstacles to the conduct and application of institutional research. The basic problem has already been mentioned: EPA--and other sources of research funds such as the National Science Foundation--are part of the system and thus not without a stake in things as they are. In addition, there is the very great difficulty of showing in a convincing way that some suggested institutional change represents an improvement. Short of experimentation, "demonstrations" are necessarily based on a priori assumptions and deductive analysis. Such methods may seem to be weak reeds on which to build changes that can be expected to have far-reaching effects on society. Furthermore, as we note below, experimentation is very difficult, perhaps impossible, in this field (Campbell and Stanley 1969). Therefore, the tendency toward caution and inertia in these matters is probably not misplaced, especially as we are currently so sensitive to the flaws in the reforming notions of past generations. For example, consider the widespread disenchantment with the regulatory commissions, once regarded by reformers as the answer to an array of society's problems. That experience suggests that institutional innovation is risky, but it should <u>not</u> be seen as evidence that institutional research is useless--rather the opposite.

Further encouragement for this view is offered by a current example, the Congressional budget apparatus, which has yet to prove itself, but which is off to an encouraging start. This innovation, or set of innovations, owes much to the thinking and research done over the years at the Brookings Institution, an example of a private research institute that has contributed richly to government.

We are therefore inclined to stress opportunities rather than obstacles, and in this view offer the following general observations on the possibilities for institutional research.

a. At the theoretical level, there appears to be promise in developing insights of the field known as "social choice," which exists on the borders of economics and political science (Sen 1970).

b. On a more applied level, an obvious technique is the analysis of successes and failures of existing institutions. There are limits to this technique, of course, since measures of performance are easier to devise and agree on at the management level as defined above, where the broader goals have already been stated by some higher authority. Where we are discussing legislative arrangements, for example, there exists no straightforward measure of results--at least none that does not depend on the bias of the observer.

c. Because of possibilities for gaming, or using sophisticated, misleading behavior to influence the outcomes, experiments with institutions do not generally seem a very promising road. This problem is most acute when participants are few in number, well informed, and themselves sophisticated, as many are in the environmental area where the chief actors are likely to be industrial firms and governments.

d. Understandably, not all institutional research can or should be conducted extramurally. Critical questions of statutory implementation must necessarily be analyzed by the agency staff itself. This is necessary and desirable regarding legal and political judgments about the reach of statutory programs. However, a more flexible, far-reaching search for innovative and reliable institutional devices to bring about more efficient, efficacious, and fair environmental control programs can be undertaken through intramural and extramural research in combination (Kneese and Schultze 1975).

e. EPA has been actively pursuing several problems we address below in our recommendations for institutional research. Sometimes, however, this research has been integrated into economic and technical studies in such a way as to dilute, underfund, and reduce the impact of the institutional research that has been undertaken. All too frequently, institutional research loses the influence it might otherwise have because it is diffused throughout a technical report or merely attached as an appendix.

It is important to stress one additional point that is frequently overlocked in discussions of environmental policy and research. The existing institutional framework, especially at the management level, heavily influences the content, location, conduct, presentation, and use of environmental research of all kinds. To illustrate this observation, it should be sufficient to mention three examples.

First, the existing regulatory framework for pollution control creates virtually no incentives for dischargers to fund private research on new treatment, recirculation, or materials and energy recovery processes. Because the discharge standards being written are based on specific technologies, and because future, stricter standards are to be based on "better" technologies, industrial dischargers cannot buy themselves a decrease in treatment costs through research and development. At best, a new development by one firm might give that firm a jump on others in its industry if the development is chosen as the basis for a standard. (This, unfortunately, would very likely be considered anti-social behavior by the firm's competitors, and the sociologists of business tell us that there are strong pressures against such activities.) It is important to note that institutional forms designed to obtain desired discharges <u>need</u> <u>not</u> have this dampening effect on technological development. For example, a national discharge standard set independently of specified technology would give dischargers the incentive to reduce the costs of meeting it through new technologies. It is expected that a cost-effective technology will be found in most cases. However, development of new, practical technologies capable of meeting any goal cannot be guaranteed. Witness the criticism directed to the goal of "zero" discharge in the Federal Water Pollution Control Act Amendments of 1972 (86 Stat. 845, PL 92-500) and the failure to date in finding a cost-effective solution to the emissions of nitrogen oxides by automobiles. Nonetheless, an economic incentive, such as a charge or subsidy, would provide a continuing incentive for developments that would reduce the sum of treatment costs and charges paid (or subsidy payments foregone).[2]

In the current situation, EPA has to fund research into pollution control technology if very much is to be done. Even when the results come in, there is still the problem of "selling" the new ideas to the industry, since adoption cannot lead to cost reduction as long as discharge standards are modified to meet the new technology. A particularly clear example of the operation of this perverse feature of current water pollution control legislation concerns cold rolling mills in integrated steel mills (Vaughan et al. 1976). The effluent guidelines for this industry specify three different standard levels that differ by a factor of forty: the most stringent applies to mills that have installed the most sophisticated water-recirculation systems; the least stringent applies to old-fashioned mills that use water once and then must treat and discharge it. Although it is impossible to say without careful analysis of costs and performance whether or not these guidelines remove all incentive to adopt recirculation systems, it is clear that they at least substantially reduce the incentive, since the discharge standard is more stringent for improved capabilities.

Second, in many situations the federal effort to develop pollution control devices is largely captive to the industrial community that is to be controlled or to manufacturers whose devices are to be controlled. The prime example here is the automotive industry, although numerous other examples exist.

Mobile-source control technology has been produced largely by the auto manufacturers in compliance with federal requirements. When the auto industry has been unwilling or unable to meet progressively more restrictive emission requirements, control program deadlines have been extended one or more years by the EPA Administrator or by Congress. There have been three apparent failures for the mobile-source control program to date: (1) inability of emission-control devices to maintain effectiveness in normal service, (2) generation of a sulfuric acid mist discharge from tail-pipe catalytic converters that otherwise are effective and do not impose a severe fuel penalty on car operation, and (3) inability to control emissions of oxides of nitrogen.

The federal research effort in support of mobile-source controls (including aircraft engine emissions) has been modest in scope and objectives. The principal purpose has been to verify the effectiveness of control methods proposed by manufacturers and to develop sufficient knowledge and experience in control technology to negotiate successfully with auto manufacturers. As a consequence, the auto and aircraft manufacturers control most of the research capability in the field of mobile-source control R&D, and it is difficult to determine whether the federal program represents a goal for the auto manufacturers or whether the auto manufacturers will establish a standard that the federal government is constrained to enact because it provides an attainable goal.

Third, the burden of proof concerning hazards of a toxic chemical, such as a pesticide or industrial material, has historically fallen on the opponents of its use. Thus, once the requirements for initial registration had been met, incentives to the company manufacturing the chemical to undertake continued research and monitoring to check on environmental and health effects were sharply reduced.[3] Were the developer/marketer facing some liability for damages or the necessity for proving some level of safety once a reasonable challenge had been mounted, there would be strong incentives for him to undertake long-term programs of toxicological research.[4] Recent steps, such as requirements for pre-market testing of toxic substances and for periodic reregistration of pesticides, are evidence that public policy on where the burden of proof should lie may be shifting.

RECOMMENDATIONS

1. Approaches from State, Local, and Foreign Governments

EPA should undertake empirical studies of existing state, local, and foreign environmental management systems to identify and evaluate conditions that influence efficiency of performance, and to provide a basis for selecting or modifying management systems.

Under our federal system of government, the states have, at least until recently, been relatively free to pursue their own separate initiatives in the field of environmental management. EPA can perform an important function by conducting research in the "laboratory of the states" to learn of their successes and failures so that successful approaches might be imitated and unsuccessful approaches avoided. Furthermore, such information would be useful to EPA itself in deciding on the best approaches to implementing the many federal environmental control programs for which it shares responsibilities with the states.

State approaches that are researchable include the tri-partite environmental management program of Illinois, created by the Illinois Environmental Protection Act of 1970; the California Environmental Quality Act of 1970 and similar legislation or programs in 24 other state jurisdictions; the Michigan Environmental Protection Act of 1970, which confers a large measure of independent environmental decision-making authority upon the state courts, and its imitators in six other states; the Maryland Environmental Service, a public corporation that constructs and operates waste-treatment plants and solid-waste disposal sites and provides water-supply services; the Land Use Control Programs of Vermont, Delaware, Massachusetts, Maine, Hawaii, Wisconsin, Colorado, and Florida; the executive reorganizations of environmental agencies that took place in New York, Washington, and Wisconsin between 1967 and 1970; and the regional governments that have been established in Dade County, Florida, the Adirondack region of New York State, the Twin Cities (Minneapolis-St. Paul) metropolitan region, and the Nashville and Indianapolis metropolitan area governments. Similar techniques adopted by communities at the local level also afford a rich body of researchable experience, but are too diverse and numerous to be usefully listed here (see the working paper by Selig, cited in the Appendix).

The experiences of other countries may also furnish insights relevant in the United States, despite differences in legal traditions and governmental structure. Types of research EPA might undertake include analysis of the action programs of the European communities; the environmental impact assessment and development assistance programs of the World Bank, the U.N. Development Program, and the U.S. Agency for International Development; the experiences of developing nations in regulating national business activities affecting the environment; the implementation and enforcement of international agreements; the impact of environmental control on international trade; the experience of other nations with laws patterned after the National Environmental Policy Act of 1969 (42 USC 4321 et seq., 1970, PL 91-190); the techniques by which deadlines for pollution abatement might be internationally coordinated; and the techniques by which pollution of the seas, especially by oil, might be controlled (see Advisory Committee on Intergovernmental Relations 1973).

Research along the general lines specified should consider the following important attributes of the institutions and systems being investigated: (a) their potential for adoption elsewhere, including likely attractiveness to legislatures in other states; (b) the likelihood that they will contribute to the solution of key environmental problems; and (c) whether there has elapsed a sufficient length of time since adoption to result in data and experience that can be analyzed and assessed.

2. Approaches from Domestic Social Programs

<u>EPA should support research to determine the feasibility of adapting environmental management systems to approaches that have proven successful in the management of other domestic social programs</u>.

The institutional framework for residuals management could be enriched by incorporating ideas that have already been implemented in other domestic programs. From these experiences environmental managers might learn how to design better environmental control programs, how to make the best choices for supervisory authorities, how to improve the design of fiscal implements, how better to time the incidences of fiscal measures and regulation, and the like. Consider the following examples:

a. A number of domestic programs provide relocation and retraining assistance for workers who have become unemployed because of government programs. Industries subject to environmental controls may generate a need for similar assistance.

b. Operating subsidies are available for maritime and mass transit enterprises, but in the field of environmental control, federal subsidies are almost entirely confined to construction of treatment facilities. The same reasons that justify federal subsidy for construction of treatment plants could also justify subsidies for operating them. A carefully designed program based upon well-founded research would include incentives for improved operation, and might thereby go far toward correcting the record of operation and maintenance prevalent in many treatment plants across the country.

c. Government benefits are now withheld from universities and other institutions that fail to pursue affirmative action programs designed to eliminate unwarranted discrimination in hiring practices. The federal government already has a list of polluters in violation of law from whom it will not purchase goods or services, but the more generalized use of withholding benefits as a social policy implement has not been researched.

d. Workmen's compensation provides no-fault insurance against injury suffered on the job, and there is a limited program of federal insurance against nuclear accidents. Research might well be directed toward the possibility of applying the insurance principle to other kinds of environmental harm. Public and private dischargers, for example, could be required to provide insurance against fish kills and other damage resulting from malfunctioning treatment plants or slug loading. This could be an important alternative to effluent charges.

e. The various schemes for rationing and establishing quotas (agricultural quotas, export licenses, taxi medallions) suggest the notion that the quantity of pollutants released into the environment can be regulated by systems of marketable discharge permits. An interesting development in this area is EPA's recent ruling that an "emissions offsets" policy is consistent with the Clean Air Act (84 Stat. 1685, PL 91-604). This policy allows a major new or modified source to be located in an area that does not meet national ambient air standards provided equivalent offsetting reductions in emissions from existing sources are also obtained (U.S. EPA 1976c). Some research has already been done on the subject of marketable discharge permits (Croke and Roberts 1971). Further studies of this concept might consider whether, as a practical matter, it can function consistently with and as a supplement to existing environment laws.

3. Institutions for Managing Hazardous Materials

<u>EPA should seek to identify innovative methods for use by institutions managing hazardous materials. The methods should be analyzed to identify incentives for adopting non-hazardous alternatives. Assessment of the methods should also judge how well they deal with important characteristics of the hazardous materials involving low-level chronic exposure, wide environmental dispersion, and long latency</u>.

Enormous risks are posed to mankind by the hazardous materials we create, store, use, or discharge into the environment. While a complete catalog of the materials and the threats they pose would be a large research task in itself, a few examples will illustrate the meaning we give to the words "hazardous materials" and the range of threats created. One familiar example is radioactive material, especially the residuals from nuclear reactors. These residuals include some extremely long-lived and toxic radioactive substances. Beyond that, some of the residuals can be processed, using technology that can be constructed from information in the public literature, into material for nuclear weapons. Thus, we have the threat of sabotage and massive blackmail as well as the more common problem of accidental release to the environment. A second example is provided by the organic chemicals that our technology has produced for use as pesticides and herbicides, solvents and cleaning agents, flame retardants and heat transfer agents, and so on. These chemicals often enter into use without anything approaching adequate knowledge of their properties once released in the environment. We then find, with discouraging regularity, that we have been introducing carcinogens or mutagens that act at very low concentrations, that are stable and hence can spread widely, and that may not produce human health effects for years or even decades (Tomatis 1976). A third, and even more exotic, example may well be provided for us before too long by the "genetic engineering" on DNA. Many believe that sooner or later an organism toxic to man and untouchable by existing vaccines and medicines might appear as a result of this research. One wonders whether it will accidentally or intentionally become part of our world before its dangers are understood.

The institutional challenge raised by hazardous materials, actual and potential, is complex and difficult. It is clear enough that we cannot tolerate a system that places only trivial obstacles in the way of registration and use of any new drug or pesticide or industrial chemical, and that requires incontrovertible proof of human sickness or death before acting to prohibit further use. It seems equally clear that to forbid the use of every new material until its "safety" has been demonstrated would create its own heavy costs.[5]

Instead we need an innovative system, somewhere between the extremes, that would create self-governing processes. Those whc profit from the development and sale of new materials should have a built-in incentive continuously to monitor their safety, long after the initial registration/use decision has been made. To provide such incentives it will probably be necessary to assign liability for health or other damages to the producers. If the situation is such that some use seems socially desirable, but if the risk variance makes commercial insurance unavailable and self-insurance impossible for the single firm, the government might be made the insurer.[6] A first step toward actual innovation is research into alternative possibilities, beginning with identification of suggestions that have been made, and continuing through analysis of likely incentive effects of the alternatives, their equity aspects, and so forth. It is such a program that we strongly recommend.

4. Use of Economic Incentives for Managing Environmental Quality

Research should appraise the utility of economic incentives as an element of a residuals-management strategy. Specifically, this research should seek to: (a) provide an understanding of why such incentives appear to be ignored by policy makers, and (b) explore ways of defining and constructing incentive systems that will be seen as more useful.

Most traditional styles of regulation call for elaborate bodies of law and administrative rules that, in turn, call for large staffs and for heavy legal and administrative costs. Negotiation and adjudication consume a great deal of time. The need for reviews, permit approval, licensing arrangements, and the like are seen by the discharger as negative constraints; they furnish him with excuses for avoiding reduced discharges.

The most effective focus for control of residuals discharges is the source--the factory, the power plant, the farm. One of the most effective methods for achieving control may well prove to be publicly prescribed economic incentives, either charges or payments, used either alone, or in combination with required discharge standards. There has been, for a long time, a body of expert opinion that holds that such systems would have considerable advantages over current methods (Kneese and Schultze 1975).

Usually economic incentives are seen as the policy instruments through which some ambient quality standards, chosen politically, would be obtained, although in more theoretic treatments charges are assumed to be set equal to the marginal social damages caused by a particular discharge. In either case, the argument holds that the incentive will be seen in the same way as other prices the discharging firm, individual, or municipality has to pay. These dischargers will be encouraged to adopt abatement measures up to the point at which their marginal costs are just equal to the charge or payment--a condition that is necessary for economic efficiency.

An important argument supporting a system of economic incentives is that it provides a continuous and essentially permanent motivation for developing new treatment technologies and new production processes that generate smaller quantities of residuals.

Economists and engineers have spent considerable time and effort on research on various aspects of incentives. The economic theory of such systems has been argued and reargued. Models have been developed that would allow the appropriate public agency to estimate the price required to produce a particular desired result in ambient quality. Significantly, research has been done to measure the actual impact of existing charge systems on discharges.[7]

Despite this activity and the impressive array of resulting knowledge, there has been no serious attempt to apply effluent charges or payments in the United States. One reason often given for this is that economic incentives are "harder to administer" than the discharge standards and permits for discharging pollutants that are currently required. Another claim is that incentives require "too much" information to set up. A third is that they are somehow unconstitutional or, at the very least, unfair.

It seems clear that the people who make and influence public decisions have yet to find economic incentives attractive. Although attitudes may be changing with the current efforts to "deregulate," it will still be important to understand why such systems have not been used and then to explore possibilities for system designs that will resolve valid complaints.

This research could begin by clarifying the issues--separating relevant from irrelevant criticisms. For example, one often hears that economic incentives would be hard to administer because the public agency would be forced to keep tabs on actual residuals discharges. However, this claim ignores the inconvenient fact that no system of discharge control will be self policing. For example, some monitoring activity will be necessary to see that the terms of discharge permits are not exceeded. The income tax system depends on self-reporting enforced by random and systematic audits (monitoring). In the process of the studies recommended here we might discover what, if any, are the real administrative handicaps, and thus set the stage for imaginative redesign of economic incentive systems.

5. Personal Perceptions

<u>EPA should launch an entirely new program of research into personal perceptions and inferences regarding pollution, health risk, and value of environmental amenity to assist public agencies in developing realistic strategies for the management of residuals</u>.

Man's effects on the environment may pose such grave dangers as waste of irreplaceable natural resources, pollution of life-supporting air and water, dispersion of destructive chemical, biological, and radioactive materials, and destruction of the earth's protective ozone layer. Can we recognize these facts and make necessary modifications in behavior to arrest degradation? The social and behavioral sciences can suggest productive avenues of research on this issue. Research supported by EPA may yield practical results useful to the Agency in assessing the acceptability and efficacy of its programs.

Public attitudes about proper roles of government, constituents contributing to quality of life, and threats to the human environment all affect what governmental actions people will support and the incentives and disincentives to which they will respond.

As an example, consider perceptions, habits, and behavior regarding use of the automobile, and its linkages to automobile emissions and fuel use. Auto use involves personal decisions, made several times daily, whether to employ alternative means of transportation, whether to make the effort to car-pool or to consolidate a number of errands, whether or not to make a trip at all. Understanding these decisions involves an array of questions. What are peoples' attitudes about driving? How important are flexibility, speed, status, privacy, cost, and dependability in determining trip-making behavior? Are perceptions of the social status of auto transport relative to alternative modes among the important dimensions? These and many other questions might help EPA to develop better implementation strategies and, eventually, legislative recommendations.

NOTES

1 The first five-year plan for research by EPA's Office of Research and Development (U.S. EPA 1976a) gives formal recognition to the broad line of research under discussion here, but the plan contains no specific thoughts on how to proceed.

2 A pertinent question is what should be done in the case of emission standards for which the promise of new cost-effective technology is small despite sustained effort by government and industry? Should the level be set with no consideration of when it might be attainable, placing all who cannot technically conform at odds with existing laws or regulations? Affirmative answers would not seem to be socially or technologically desirable. Here it would appear that the concept of "best available technology" (BAT) is perfectly valid as a stopgap standard where full compliance cannot yet be achieved technically or at an acceptable cost. A shortcoming of our current handling of emission standards and BAT (concerning which the effluent charge appears to have unique utility) is that all dischargers are constrained at present by the same deadlines, and a reasonably well-controlled operation--say one that exceeds the standard by only 5-10 percent--has the same time constraint as an operation that violates the standard more significantly, for example fourfold. The latter is more serious and should be given first attention in meeting the new standard; the former could be given maximum time to meet the standard without doing serious violence to environmental quality or the intent of the regulations. Overcoming the shortcoming outlined above implies a rolling deadline for compliance, i.e., the worst violators first and those least out of line, last. This strategy would reduce opposition to controls because those with the best systems would be allowed the longest time to amortize these systems before investing in new, improved methods and devices. It would also help the control agency inasmuch as fewer installations would be subject to control at a given time and therefore could be given more careful attention. The use of effluent charges based on closeness of approach to optimum standards as well as rate of pollutant discharge is worth considering.

3 The "cranberry" episode in 1959 provided an example of the impact that could be produced by improvement in analytical procedure. In this case, detection of the herbicide aminotriazole on cranberries prepared for the Thanksgiving market resulted in rejection of a major portion of the nation's cranberry crop. This action was mandatory under the Delaney Clause of the Federal Food, Drug and Cosmetic Act (21 USC 348, PL 85-929) which requires the complete absence of any carcinogen in food. It was presumed that the concentration of aminotriazole was not greater than in earlier years and that an improved analytical procedure was one factor in the finding.

4 See Recommendation 3.

5 Indeed, proving "safety" may be impossible, and any such law would probably very quickly dry up the flow of research and development in the relevant fields.

6 For one suggested system of classification, liability assignment, with use taxes, see the suggestion of the Panel on Market and Private Sector Decision Making (NRC 1975a). Note that the government felt obliged to assume the role of insurer in the swine flu vaccination program, an action that requires careful study for the lessons it may contain for future activities of this type.

7 The systems studied have involved sewer service charges, which are analogous to fees for discharges to the environment although the constraint of preconditioning to produce a treatable discharge applies to the former but not to the latter (Elliott 1973).

CHAPTER 2

ECONOMIC ISSUES

BACKGROUND

Economic research occupies a critical role in our proposals for a research agenda because:

a. Questions of cost (of what society must give up to buy a cleaner environment) are central to environmental decisions whether made by Congress, by EPA, or by states or cities.

b. The current state of knowledge and technical abilities in economics leaves much to be desired, especially when we interpret "economics" broadly enough to include efforts to combine information about costs of reducing discharge with data or predictions on resulting changes in ambient environmental quality and on the consequent benefits from such changes.

c. EPA in general, and the Office of Research and Development (OR&D) in particular, while recognizing the importance of research in this area have yet to develop an adequate research program (e.g., see U.S. EPA 1976a: 3, 9, 43, 63, 64).

This chapter, then, suggests several specific research topics that, in our opinion, shculd fcrm major concerns of a "socio-economic studies program." These topics, in turn, arise from the need for practical methods for accomplishing the task mentioned in (b) above--the linking of control costs and ambient quality changes. The topics are not necessarily arranged here in order of priority, but rather in the logical sequence required to build the regional models needed for accomplishing this task. That is, we begin with a discussion of plant-level studies, move to regional management models, and then expand our horizon from efficiency to equity, dealing with the study of the distribution of control costs and benefits.

At the most fundamental level, laws and institutions determine whether economic considerations are explicitly to be taken into account. In the extreme, the Clean Air Amendments of 1970 effectively rule out consideration of the costs of meeting national ambient air quality standards to protect the public health (84 Stat. 1680, PL 91-604). Any evidence of harm is taken to prove that meeting the standards will produce benefits greater than any conceivable costs. Some of the rhetoric in other environmental legislation, stressing protection of public health or achievement of zero discharge, appears to aim at the same kind of independence from the calculation of costs. As we have already hypothesized, it may be the prevalence of this kind of legislative provision that accounts for the step-child status of economics found in EPA. The public health orientation of EPA's predecessors is probably also a factor. A different approach is found in the basic language defining the nature of the 1983 discharge guidelines in the Federal Water Pollution Control Act Amendments of 1972--"best available treatment economically achievable"--where the importance of cost is explicitly, if ambiguously, recognized.

At an operational level, the choice among instruments for achieving some desired effect defines what economic calculations will be relevant. For example, legislation and regulation that focus on technologically-based discharge standards, require, if costs are relevant at all, only information on specific treatment systems applied in specific circumstances. On the other hand, the use of economic incentives to achieve some desired change in ambient environmental quality, if the aim is to set the charges "correctly" on the first try, demands knowledge of the costs of achieving small changes in discharges over the whole range of possible discharge levels, for all the dischargers in a particular region. This, in turn, means that complex sets of alternative methods of achieving the reductions must be evaluated economically. Our recommendations aim at a research program that will provide a foundation for current and future actions under possible alternative approaches to environmental quality management.

RECOMMENDATIONS

6. Fitting Process Level Costs into Plant-Wide Cost Functions for Managing Residuals

EPA should support research to understand better the magnitudes of and the factors influencing the costs associated with the management of residuals.

Flexible techniques are needed for assessing the costs of alternative levels of residuals control under varying sets of conditions at the level of the plant (or, by analogy, the farm or feedlot). Heretofore such work has focused on single plant configurations, with few options for changing inputs or outputs. Costs have been estimated primarily for applying specific control technologies rather than for meeting alternative discharge standards. The former, limited approach has its merits, particularly in the context of existing legislation, which calls for standards of plant discharge that are based on technology. Even in the current situation, however, it would be valuable for EPA to know the costs of alternative discharge standards, since legal and political arguments are likely to center on the costs implied by the Agency's suggested standards. EPA should have its own cost estimates, since industry has every incentive to exaggerate control costs. In parallel, EPA has incentives to underestimate costs, and this symmetry suggests the need for third-party assessors, probably from the scientific community. That approach would also be important if a system of effluent charges were to be established.

The chosen method for assessing plant-wide costs of pollution control technologies should reflect in some way the links between residuals generation and input type and quality, production processes, output mix, product specification, and so forth. It should also allow for possibilities of materials recovery or by-product generation that result either in additional final products or substitutes for purchased inputs. It should, of course, reflect the intermedia effects implied by treatment and recirculation alternatives. Steps should be taken by EPA to apply the method to all activities producing significant residuals in any area, and new processes and products should be incorporated on a continuing basis. The major alternative methods that should be considered include:

a. Extension of the standard engineering approach such as that used by EPA and its predecessors (and by the National Commission on Water Quality). Here the extensions should concentrate on adding more options at both the process and treatment stages, for each product, to capture the variety of actual situations.

b. Application of econometric analysis to available data on residuals management costs and the relevant independent variables, including discharges as well as measures of plant type, product mix, and factor prices.

c. Development of process analysis models that account for residuals generation and the relevant control options for activities beyond the few covered by independent work done to date (Russell and Vaughan 1976, Thompson 1975, Russell 1973).

This recommendation stresses, by the very nature of the language used, research connected with point sources such as industrial plants and municipal treatment plants. However, it should be recognized that an analogous approach and set of research goals should also be applied to nonpoint sources of residuals such as farms, construction sites, mines, and forests, and to institutions such as hospitals.

7. Cost of Regional Residuals Management Policies

> EPA should expand and intensify its efforts to produce a practical, operational method for assessing on a regional basis the magnitudes of costs and implications for ambient environmental quality, appropriate, of alternative residuals management policies, including ambient quality standards, technologically-defined discharge standards, and input requirements or restrictions.

Regional cost studies, and studies of process-unit and plant-level costs, may someday become a routine part of decision making for environmental quality in the U.S. The methods used should be capable of addressing different problems, such as (a) finding the least-cost way of satisfying alternative sets of ambient standards, (b) meeting uniform discharge standards and predicting the implications of those standards for regional ambient environmental quality, (c) estimating the costs and the implications for regional ambient environmental quality of imposing specified requirements for treatment equipment, and (d) identifying the "optimal" standard set, given (an unlikely assumption) data on benefits as well as costs. In addition, the methods should be extendable to consideration of any of the above problems with additional constraints on the distribution of the costs and of benefits, geographically by income groups, or on some other basis (see Recommendation 8, below).

The Federal Water Pollution Control Administration, an ancestor of EPA, funded the pioneering Delaware Estuary Comprehensive Study, which formed the information base for the exercise in setting water quality standards undertaken by the Delaware River Basin Commission (Delaware River Basin Commission 1970, Johnson 1967, Kneese and Bower 1968, Ackerman et al. 1974). More recently EPA has preferred to favor macro-models such as the Strategic Environmental Assessment System, although the work of Paik et al. (1974), conducted under an EPA grant, did involve a hypothetical example of regional modeling.[1]

Some good starts have been made; one example is the Implementation Planning Program, which was designed to make use of Air Quality Control Region pollution source inventories and to predict levels of ambient environmental quality and costs of specified standards applied to dischargers (e.g., required switching away from coal; required SO_2 reductions) (TRW Systems Group 1970). Further, EPA staff members have published in the open literature analyses of alternative regional management techniques (e.g., uniform percentage discharge reductions vs. emission charges) and their implications for the total cost of a regional program designed to achieve the existing federal primary standards. (Atkinson and Lewis 1974a and 1974b). Thus, EPA retains a presence in the field, but it has fallen to the RANN program at NSF to fund most of the research in this area. Resources for the Future, Inc., has also supported a large scale research effort along these lines. (See for example Russell and Spofford,[2] and Spofford et al. 1977.)

Various tools are useful in studying economic aspects of regional control systems. We have already emphasized the study of regional systems using component models of individual dischargers. Sufficient work has yet to be done linking discharging activities through the economic system as well as through their effects on the environment. Regional pollution control models have generally not reflected the many rounds of economic adjustments that would follow from a public policy imposing control costs on firms. On the other hand, one of the economists' favorite tools is the input-output table and its associated algebra, which is in fact designed to show the effects of these rounds of adjustment. The input-output methodology is also appealing because it is detailed--reflecting the activities of units as small as four-digit Standard Industrial Classification Codes industries (e.g., petroleum refining)--and because, for certain regions and the nation as a whole, tables are already available. Finally, techniques have been developed using input-output matrices to predict the implications of changes in economic conditions (such as the demand for a particular industry's product) for regional environmental quality changes (Isard 1974).

Input-output methodology cannot be used, however, to answer questions such as, "How much will it cost region x to meet the primary ambient air quality standards?" One cannot work "backwards" from a proposed set of ambient standards (or even of discharge standards) to find the corresponding economic situation.[3]

It has been suggested that a regional management model might contain a regional input-output table as part of its set of constraints (Thoss and Wiik 1974). The problem with this approach seems to be that in general, for purposes of predicting ambient environmental quality, the location of residuals discharges must be defined with a greater degree of resolution than is consistent with the industrial aggregates found in available input-output tables. Construction of input-output tables with individual plants instead of industries as the basis for the rows and columns would require very expensive data gathering.

Input-output techniques can be useful on the macro-economic level, where the interest of public agencies is to translate the initial round of estimated pollution control expenditures implied by legislation into effects on the overall economy. However, other techniques are available for estimating macro-effects, notably, several elaborate econometric models of the economy that are as useful as input-output analysis (Evans 1973, Chase Econometrics Associates, Inc. 1975). These sophisticated econometric models seem capable of meeting EPA's need for macro-economic predictions; hence there should be no reason for the agency to sponsor further research for this purpose. This seems especially clear since several of the models being used are the property of private firms that have already found it worthwhile to go into pollution-related forecasting work without significant EPA support.

8. Distribution of Costs of Alternative Residuals Management Policies

EPA should continue and, if possible, expand its research aimed at increasing knowledge of how costs of alternative policies for residuals management are distributed over income classes, geographic areas, and political jurisdictions.

To date, estimation of distributional effects has involved translating the results of assumed control expenditures through general equilibrium models to determine how the initial costs are ultimately borne. Table 2.1, adapted from the special working paper prepared by Portney (cited in Appendix), summarizes the work done so far.

Table 2.1 shows a disappointingly large number of areas without research results; however, EPA has been an important contributor to what has been done. Geographic distribution, an important consideration in a representative democracy with areal jurisdictions, is a major area of research need. It is important, for example, to improve information about regional distributions of pesticides in view of the many pesticide decisions being forced by environmental litigation.

Distributional information is especially useful in the consideration of regional strategies for residuals management. The traditional concern of the economist and engineer with total costs of projects and policies is only a partial guide to policymakers. There is inevitably concern about those who lose and those who gain, and because our system has little flexibility for compensating the losers from a public policy decision, especially one involving common property resources such as air and water, those charged with the decisions may be ready to seize on a chance to accept a higher total cost while obtaining what seems to be a "fairer" distribution of the burdens.*

While distributional information on a geographic basis is particularly important to legislators, the Executive Branch, as the representative of all the people, may be interested in distribution over income groups. Either approach or both might be used in a particular region, depending on the institutional structure defining responsibility for the policy decision.

TABLE 2.1 Distribution of Cost Studies

	Air Pollution							
	Private			Water Pollution		Solid		
Distributional Group	Stationary	Mobile	Government	Private	Government	Waste	Noise	Pesticides
Income Class	Dorfman (1973)	Dorfman (1973) Freeman (1975) Harrison (1975)	Dorfman (1973)	Dorfman (1973) Urban Systems (1975)	Dorfman (1973) Dorfman and Jones (1974) Urban Systems (1975)	Dorfman (1973)	–	–
Factors of Production	–	–	–	–	–	–	–	–
Geographical or Political	Russell, Spofford, Haefele (1974)	–	–	Urban Systems (1975) Russell, Spofford, Haefele (1974)	Urban Systems (1975)	–	–	–
Socioeconomic (age, ethnic group)	–	–	–	Urban Systems (1975)	Urban Systems (1975)	–	–	–
Macro Aggregates	Chase (1975)	Chase (1975)	Chase (1975)	Chase (1975)	Chase (1975)	–	–	–

NOTES

1 The EPA/OR&D five-year research plan (U.S. EPA 1976a) does contain an indication that work along these lines is contemplated. Unfortunately, the language chosen is rather vague and the exact intentions therefore remain obscure.

2 Russell, C.S. and W.O. Spofford, Jr. (1975) A regional environmental management model: An assessment. Presented at the First Annual Meeting of the American Association of Environmental Economists. Dallas, Texas.

3 It is possible to write the equations that would have to be solved to do this, but there is no guarantee that they have realistic solutions.

4 If benefit (damage) estimates are available, then decision makers will also be interested in the distribution of net benefits.

CHAPTER 3

ANTICIPATING ENVIRONMENTAL PROBLEMS

BACKGROUND

On one aspect of our nation's environmental program there seems to be widespread, if not very encouraging, agreement. We do a poor job of anticipating environmental problems, and are much too frequently caught by surprise, for example, when materials in common use turn out to have dangerous side effects (DDT, fluorocarbons), or suggested solutions in one area are shown to pose threats in a different area (catalytic converters for autos, ocean dumping of sludges from sewage treatment plants). The evidence of past failures seems all the more ominous when we contemplate the characteristics of some of the recognized threats and imagine the consequences of a similar situation evolving undetected for any significant length of time. In particular, our knowledge of potential irreversibilities and long latency periods raises the possibility that society might be rushing in ignorance toward some catastrophic event or events.

Given this possibility and the current fashion for doomsday prophecies, we find ourselves awash in dire predictions--some from scientists, some from popularizers, and some presented as fiction--making it all the more difficult to separate the kernel(s) from the tons of chaff. But an attempt at separation is imperative, since careful case studies of past failures alluded to above would almost certainly show that someone, somewhere, had predicted the very effects later observed, or at least that the "raw material" for such predictions was ready at hand. We remember the cases as failures because no action was taken, regardless of whether correct predictions were available. Will we, at some time in the future, remember that some climatologists <u>did</u> predict a global warming due to CO_2 discharge? This is a spectre that haunts many people, but the grave difficulties for action are perfectly illustrated by the fact that other scientists, equally reputable, are predicting a global cooling trend, while still others focus on supposed shifts in the low-latitude monsoon belts.[1] There remains, of course, the possibility that some disaster will arrive completely unpredicted, so that we cannot even be sure that our tons of chaff contain the kernels we seek.

Thus, while we recognize the grave importance that may attach to society's success or failure in anticipating some particular threat, we are also fully aware of the difficulty of designing for improved predictive capability.

The characteristics of the anticipation problem are shaped by existing institutions, and any attempt to improve matters that ignores this fact will probably come to very little.[2] No amount of monitoring, or science advising, or projection modeling is going to improve our record if the relevant problem-recognition system, the approaches to mitigate the problems, and the incentive systems to provide alternatives remain unchanged.

So, while the recommendations below stress mechanical activities that may be expected to increase the probability that the appropriate predictions will be made, it is appropriate to re-emphasize here the recommendation made above for research into alternative management institutions and accompanying incentives. This institutional effort should be broad enough to reflect the central problems and considerations in the matter of anticipation. The key here is to ask if a system can be devised that gives some person, board, or group the incentive to keep up with the relevant predictions, to sift through them, to make recommendations for action where that seems warranted, and to provide funds for current study if there is reason to doubt the wisdom of any action.

The current system has often been dismissed as one in which political and bureaucratic horizons are too short for any serious attention to be paid to problems predicted to be 30, or even 10, years ahead. There is more than a grain of truth in this judgment, since it is clear that events far in the future will occur after most incumbent policymakers have moved on or out. And, in any event, the public record is almost never precise enough nor the public memory long enough to hold a person accountable for what he or she failed to do 30 years before. On the other hand, to say that governmental eyes are riveted on the crisis of the month is not to say that a predicted future crisis cannot capture some attention. Clearly, the media can make the threat of some future catastrophe an issue of the day.[3] Such questions as "What are we going to do about the possibility that we shall run out of X or get cancer from Y?" can be made as immediate (or nearly so) for the politician as more familiar ones ("What are you going to do about the closing of the Air Force base at Z?") The problem with the current system, then, is not that no attention can be given to potential future problems, but that this attention, a very scarce resource, is distributed on the basis of press-agentry rather than on politically responsible judgments about competing predictions. It can, of course, be argued that all problems, current or future, take their places on the government's agenda in accordance with shifting media pressures (fads).[4] But it does seem that for current problems there are some limits to neglect--and to hysterical overreaction--posed by the realities. People, or some things people value, are actually suffering; events are taking place. Under such circumstances fakery is risky.

Is it possible to encourage routine, responsible consideration of and follow-up on potential environmental threats? We are inclined to believe that rules about liability for future damages can have a powerful influence in this regard. If, for example, the British and French airlines flying the SST were to be declared unconditionally liable for some fraction of the medical bills of all skin cancer patients with cancers occurring after, say, 1980 (with the fraction to depend on the measured percentage changes in the stratospheric ozone), one can guess that monitoring, and even action, might be forthcoming. Similarly, if product liability laws were redrawn in such a way that future risks were concentrated in the same hands as future profits, instead of being diffused over the population at large, potential environmental problems would receive much more serious attention.[5] Such changes would probably involve the locus of the burden of proof as well as the locus of liability itself, once a connection were established.

Basically, the idea would be to attach liability for future damages more firmly to a "person" with a long public life.[6] Currently, the probability of being held liable is small and, when multiplied by the small probabilities of there actually being any damages and by the normal discounting factors, the expected damages must (almost always) be very small. Greater certainty on the liability side would at least encourage the decision makers to focus on the expected social damages (and their variance). This approach fails, of course, when the threats involve national or global catastrophes, so that the concept of compensation loses its meaning. It may be that in such cases only by raising hell in public is it possible to gain attention and action. We suggest, in this connection, only that the constitutionally-protected freedoms that promote open debates and controversy are probably the most effective means for exposing latent hazards. Public and private institutions that have the scope and power to bottle up and conceal controversy may be tempted to buy bureaucratic peace-of-mind at the expense of the general welfare.

Beyond these general considerations, which dc seem frequently neglected, we have concentrated our recommendations for research on the more mechanical problems of characterization, prediction, screening, and monitoring.

RECOMMENDATIONS

9. Extension and Expansion of Identification and Measurement Activities

EPA should extend and expand its programs aimed at identifying and measuring the chemicals and other potentially hazardous materials that are being introduced--by use, discharge, or accident--into the natural environment.

The connection between this general recommendation and problem anticipation should be abundantly clear. Unless we know what materials are getting into the environment through man's activities, in what quantities, and at which locations, we can hardly begin to guess what future problems we may be creating. Therefore, rather than dwell on this point, we propose here to introduce several more specific proposals concerning identification and management (or characterization).

a. EPA must maintain and keep continually up to date lists of both specific hazardous materials and general classes of compounds, the members of which are likely to be dangerous.

At the present time, many useful sources of information and principles can be tapped for such an effort. For example, EPA can make use of the lists of hazardous substances published in the Federal Register (U.S. EPA 1974); in the Registry of Toxic Effects of Chemical Substances (National Institute of Occupational Safety and Health [NIOSH] 1975); in the TRW Systems Group (1973) report on hazardous wastes; in the recent list of 65 toxic chemicals for which EPA is being required to prepare criteria documents for effluent regulations (U.S. EPA 1976b); in numerous criteria documents prepared by EPA, NIOSH, and various state agencies; and in Principles for Evaluating Chemicals in the Environment (NRC 1975a). The latter, as the name implies, provides general guidance for the identification of chemical threats as well as an enormous amount of specific information on chemicals known to cause particular problems.

b. Using the lists of known and potential hazardous substances, EPA should organize and conduct a program involving routine attempts exhaustively to characterize residuals streams and other sources of hazardous materials in the environment.

Such attempts to characterize residuals streams must be preceded by some decisions on what to test for, since a priori expectations, even with sophisticated modern test equipment, create some limits on a posteriori identification. Since exhaustive characterization is expensive, it would be foolish to assert that every waste stream should be analyzed at once; rather priorities must be set on the basis of judgments about the likelihood of finding hazardous materials, the size of the discharges, and their location.

c. Lists of hazardous material should provide a basis for updating regulations concerning routine measurements of waste streams.

All too frequently, regulations for the routine monitoring of waste streams reflect an outdated view of what is environmentally important. Where hazardous materials have been identified and are known or suspected to be constituents of particular discharges, the rules governing routine monitoring should be expanded to include tests for those materials.

d. With these efforts to identify and measure must go research to develop improved sampling techniques and instruments.

Improvement in this broad area can mean several things. First, for substances that are hazardous in trace amounts, sensitive and accurate detection and measurement technology are needed to assure that maximum allowable concentrations are not exceeded. Second, to reduce the importance of a priori limits, tests that are not crucially limited by our imagination are needed. Third, to complement the effort recommended in (c), above, the development of less costly tests for the most common hazardous materials must be a constant objective.[7]

In one particular area, however, a major characterization task still is required. That is, recognition of the importance of <u>all</u> gaseous and particulate primary species emitted into the air, for their possible role in contributing to the evolution of secondary pollutants (see Chapter 4 and Recommendation 14) has not yet been given as much attention in the air pollution community as it merits. There has been an increasing awareness of this point in recent years, because several discoveries have shown that species previously considered unimportant can play a major role in secondary pollutant formation (Brookhaven National Laboratory 1972). An example is the role attributed to carbon in the catalysis of compounds of sulfur and nitrogen. It is recommended that significant exploratory R&D be supported whose goal is understanding of species other than those now regulated or now "in fashion".

10. Development and Application of Predictive Modeling Efforts

EPA should continue to support the development and application of modeling techniques that aim at predicting future environmental problems by combining current knowledge of hazardous materials and of products of their reactions in the natural environment with predictions about the rate and direction of economic change.

The unaided human imagination is of limited use when faced with the complex problem of attempting to predict how changing patterns of population size, consumption, and transportation will affect even economic variables such as the gross national product or product levels in various industries. Clearly, then, ad hoc speculation is even less adequate to the task of predicting future environmental problems. For this purpose it is necessary to look beyond the economy to the resulting discharges of hazardous materials (and of materials which, like nitric oxide, may become hazardous through environmental mechanisms), and then to the environment itself and to its mechanisms of action on materials such as transportation, dilution, bioaccumulation, and transformation.

EPA, recognizing this, has for some time supported an intramural modelling effort called SEAS (Strategic Environmental Assessment System). Modelling of this type still needs significant further development; the state-of-the-art is in its infancy compared to the need and to the promise. EPA should be encouraged to continue efforts using the general approach in which economic factors and environmental constraints are combined for application to problems of environmental prediction.

Subsequent research on modelling should emphasize the identification of potential problems to supplement the present emphasis on the issue of predicting the impact of specific legislative or policy initiatives on known ("routine") parameters such as dissolved oxygen in streams or particulate matter suspended in the atmosphere. This change in emphasis may involve giving up some sophistication on the economic side for the benefits of incorporating new and innovative models of environmental phenomena.

EPA should explore other approaches as well. For example, for purposes of looking at the tantalizing problem of CO_2 and particulate levels and related climatic change, it might be desirable to combine a very highly aggregated world economic model, with appropriate discharge information, and a global climate model. It might also be productive to integrate the record of the past, such as data on residues in Antartic ice, history of volcanic eruptions, tree ring data, and histories of rainfall, floods, sunspots, etc. At least the appropriate sensitivity analysis could then be conducted, even if the results left us currently unsure of the sign of the effect of discharges on average temperatures.

It is occasionally suggested that broad-scale, long-term modeling activities of the sort recommended fall naturally to the Council on Environmental Quality (CEQ) rather than to EPA. While this may or may not be true in the abstract, the realities of staffing are currently such that, in our opinion, CEQ could not do the job as well as EPA.

It should not be necessary to add that in such exercises, every opportunity should be seized to verify the models. Verification may involve running the model with historical, base-line data over some period in the past. Certainly, as it becomes possible to check actual predictions made during earlier development phases, this should be done. The temptation in these efforts is frequently to use new data to "tune" the model rather than for testing it. Test results are absolutely essential to the process of evaluating predictions. In the absence of some evidence of ability to predict, the models should be viewed with great skepticism. The extraordinary reception accorded Limits to Growth (Meadows et al. 1972) leads to the suspicion that some people may be inclined to excessive gullibility where computer models, especially ones predicting disasters, are concerned.

11. Investigation of Historical Cases

<u>EPA should institutionalize the ability to conduct studies of "environmental surprises" as they occur. These studies should look at the historical background of relevant predictions and regulatory judgments as well as at the characteristics of the actual events and responses to them</u>.

There is nothing like learning from past mistakes, and we appear to have plenty of such material. Each time we are, or appear to be, affected by a new hazardous substance, by an old substance suddenly perceived to be hazardous, or by some activity launched without appreciation of its environmental effects, the event should be the subject of a case study. This study should search the appropriate professional literature to discover if the problem had been predicted or could easily have been predicted with then current knowledge. It should further try to determine whether these predictions had ever come to the attention of the relevant decision makers, whether explicit judgments had been made regarding the risks, or whether (and ideally why) the decision process went forward without the available information. It should be kept in mind, of course, that some contemporary "surprises" are simply the results of informed judgments made by some responsible person or body in the past. Not every surprise arises from ignorance or from callous disregard of the environment or the health of future generations. Just because the decision appears wrong in hindsight we cannot conclude that it was, in its own context, unreasonable.

We are pleased to note that EPA appears to be moving toward establishing such a capability, and hope that study of the Kepone incident can provide a useful prototype for future efforts.

NOTES

1 For some recent data and a long list of references, see Damon and Kunen 1976.

2 For a summary of existing laws, see U.S. EPA 1975a.

3 Recall the uproar and furious spate of comment produced by the carefully managed debut of Limits of Growth (Meadows et al. 1972); and, more to the point, observe the enormous amount of private and government energy being expended on the long- as well as short-run features of "the energy crisis." The SST case also demonstrates the possibility of provoking great activity, and even a real decision, on a future threat.

4 See Davies, J.C. (1976) Setting the National Agenda. Washington, D.C.: Conservation Foundation, unpublished manuscript.

5 The principle ought also to be applicable to the discharge of hazardous material to the environment.

6 This discussion neglects the difficulties of measuring damages in a conceptually satisfactory manner.

7 The obvious analogy is with routine, multiphasic clinical testing of blood samples based on automated analysis of a large number of relevant components at trivial unit cost (when the equipment is "busy").

CHAPTER 4

TECHNOLOGY

BACKGROUND

We noted earlier our intention to give visibility to nontechnological research needs that have been too long ignored. National failure to develop new and effective institutional arrangements has both narrowed the range of technological options available for residuals control and obstructed the implementation of those that are available. A sensible program of research on residuals control must fit technology research into a framework of relevant research on institutions, economics, and problem anticipation. We are therefore dissatisfied with the existing system, which focuses virtually entirely upon technology and yet works to discourage the normal market forces of technological innovation.

A substantial intellectual community is at work in the private sector, engaged in research on technology for residuals control. They, plus the many researchers within and supported by federal, state, and local governments, comprise a highly competent body of workers, many of them effectively organized in professional societies for the exchange of information and findings. Workers in other countries are also making valuable contributions to improvement of technology.

Despite the historical emphasis of research on technology, questions arise about the direction and adequacy of current programs. Nevertheless, no useful purpose would be served by attempting specifically to list all important research and development needs in this report. Professional societies and others have already accomplished this task in some areas. This is particularly true for wastewater technology (Water Pollution Control Federation 1975), and would seem to be the appropriate role for the relevant societies concerned with air pollution sources, solid wastes, and radioactive emissions and their control.

Yet certain questions present themselves in an insistent way: Can the utility of EPA's technology research to date be assessed realistically? What areas of research should be sponsored primarily by federal funds? What areas of need are either so important or so under-supported as to warrant special attention? And there is the fundamental question that applies to all research programs: how are priorities to be determined? The Panel offers its views on these questions.

Few engineers and scientists in the fields of environmental quality would deny that research supported over the years by EPA and its predecessors has produced much information that has stimulated the development of improvements in residuals control technology. This does not mean that EPA has itself undertaken a major role in the post-development phase. Private organizations, and in particular the equipment companies, are expected to perform this role and, in fact, are doing so. For the most part, and not unexpectedly, the improvements represent incremental changes in conventional systems of technology. The point need not be belabored that the field of control technology is hardly unique in this regard. New departures in understanding and breakthroughs in problem-solving are hard to come by; and, in fact, they are not often the goals in much of our research on residuals control.

An exception may be EPA's former program of innovative research and development on physical-chemical methods of separation of solids from liquid waste streams (McCallum 1960). It seems, however, that the results of these efforts have yet to be reflected in major alterations in residuals control technology. It is assumed that questions of cost have deterred the applications of such research findings. This is not to say that important technological improvements have not been developed and employed. Some segments of industry have been particularly alert to processes that promise to increase efficiency of residuals control at reduced costs. The use of ion-exchange processes to treat waste waters from metal-finishing industries, and of membrane processes by the pharmacological industry attest to such interest on the part of certain industries. Not surprisingly, the promise of economic advantage through materials recovery is often a strong motivating factor.

The advanced wastewater treatment program raises interesting questions concerning EPA's perception of its role in demonstrating novel modes of control technology evolved in its laboratories or with the help of its funds. We cannot deny the deterrent of high cost. But is this the sole deterrent? Although the use of such large pilot plants as the Blue Plains sewage treatment plant serving Washington, D.C. provides information of great value, it has not gone far in changing the nature of control technology now being installed. While design and operational criteria have been developed for granular carbon beds and phosphate removal (and there is no question about the value of these contributions), these processes are hardly new; they have been used for generations in water treatment. Obviously, innovation means risk, and, equally obviously, decision-makers, politicians, and their engineering consultants wish to minimize risk. We note here that EPA's expenditures for demonstration programs are declining. We believe greater support should be given to these programs.

Such support should go beyond the current Step 1 facilities grants permitted under the construction grants authorization of The Federal Water Pollution Control Act Amendments of 1972 (86 Stat. 833, PL 92-500). EPA should assume a major share of the risk involved in the demonstration of innovative processes in full-scale municipal waste treatment plants. A strategy of selective support of this type would help allay the fears of decision makers, and would encourage use of novel control technology where justified.

We, as well as others, have a fuzzy picture of how EPA's responsibilities for conducting research on control technology relate to those of other agencies, e.g., the Department of Agriculture. Inevitably, we question the actual utility of cooperative arrangements--not to prove that no mechanism for interagency coordination is effective, but rather to learn what the conditions must be to provide a high probability of success. We offer no panacea for this problem.

Whatever the roles of other agencies and the responsibilities that rightfully belong to waste producers rather than to EPA, the Agency must assume these two responsibilities:

1. EPA should be an appropriate source of support for research aimed at investigating fundamental chemical, physical, and biological phenomena that <u>might</u> find application in a wide variety of control technologies. This might include, for example, work with highly specialized aquatic bacterial systems designed to render environmentally less harmful particular types of residuals from production and consumption processes, or investigation of aspects of sulfur chemistry that might make new SO_2 removal schemes possible. EPA should perform and fund basic research for the same reasons other parts of government do--it is too risky for private firms, or its fruits cannot be captured by these firms because, for example, nonpatentable knowledge is the output, rather than specific processes or equipment.

2. EPA should step in at more applied research levels when the user groups involved face special problems in funding their own research. For example, some industries are particularly fragmented, being made up largely of many very small, competitive firms. The costs of research on treatment or other control processes may be beyond any single firm in the industry, and either the anti-trust laws or the sheer problems of communication and organization may make an industry-wide effort impossible. In the public sector, a similar situation may exist with respect to municipalities.

This state of affairs seems to be true particularly in characterizing air pollutants, including those in which effluents from different sources interact in the ambient air to produce secondary pollutants that "nobody has emitted". By secondary pollutants, we do not mean those of secondary importance but those formed by physical, chemical, and biological transformations that occur in the environment. In this sense, the "sources" of secondary pollutants are the residuals discharged directly from primary sources or the intermediate products of other transformations.[1] The foremost example of this process is the chemical composition of airborne particulate matter. To date, only a few chemical species have been studied, and even in these cases only a few sources have been well characterized. For some species, almost no real-world data exist. Pollutants in need of measurement, which only the governmental sector can support or perform, include particulate organic species, especially those thought to be hazardous themselves or to be precursors of hazardous species; particulate carbon, now thought to play an important role in oxidative and reductive catalysis of sulfur and nitrogen species; and particulate sulfur and nitrogen species themselves, especially in light of the variety of reduced nitrogen species recently discovered, and the various oxidized sulfur species and their interactions.

The enormous national expenditure, much of it federal, for control technology justifies federal support of research to assure that maximum benefits will be derived. Six generic tasks appear to be especially important to EPA's missions in control technology research. Two have been described in Recommendations 9 and 10 of Chapter 3. We make four additional recommendations here as particularly timely and appropriate for EPA support.

RECOMMENDATIONS

12. Maintaining the Efficiency of Discharge-Control Systems

EPA should direct further research toward identifying factors that affect the efficiency of performance of installed municipal, agricultural, and industrial wastewater management systems and of air emission control technologies, for both stationary and mobile sources.

It is now widely recognized that when particular pieces of control technology are actually in place and operating, their performance often does not measure up to design specifications. Such substandard performance may have serious implications for ambient environmental quality and may, in fact, vitiate complex and expensive attempts at planning. Many reasons have been suggested for these failures, including:

a. The presence, in plant influents, of toxic compounds not anticipated in the design and not routinely monitored. This is particularly a problem where industrial plants discharge to municipal sewers.

b. The occurrence of operator problems, whether these be errors due to insufficient training, overly-complex systems, lapses in attention, or simple under-staffing because of the peculiar policy which subsidizes installation but not operation.

c. The acceptance of inflows above design levels because of groundwater infiltration to sewers or because of unanticipated changes in industrial loads. (This is, of course, specifically a problem for wastewater treatment technologies.)

The generic problems are monitoring and incentives for proper performance. A system that combines a high probability of detecting performance failures with some penalty for such failures would perform far differently from the existing one. But, given the existing situation, efforts must be made to analyze data on treatment process operation; to isolate the most important factors causing substandard performance; and to design supplementary training, technology, inspection methods and the like to compensate for these factors. EPA's initial efforts in this direction are encouraging, but more can and must be done.

Retesting of automotive emission control devices by EPA after one or more years of service has demonstrated a degree of deterioration that far exceeded predicted rates. Research is especially needed to determine the specific factors responsible for rapid deterioration of these devices installed on cars that receive the full range of public use conditions and maintenance. Of particular interest would be the influence on deterioration rates of individual driving patterns (city, highway), fuel grade, maintenance frequency, and car loading. A study of this nature would permit a determination of the fraction of deterioration that is attributable to defective devices and the fraction that results from consumer misuse; the findings would make it possible to develop rational remedial measures. In addition, research is needed to develop rapid, inexpensive methods and analytical instruments for evaluating the performance of automotive emission control devices. This would provide a first step in a routine testing and maintenance program designed to keep up the effectiveness of automobile controls over the life of the vehicle.

13. Estimating the Costs of Control Technologies

EPA should, in its current role of motivator for and developer of control technologies, make a serious effort to estimate the magnitude of the costs of installing and operating the control units developed. These costs should be reported in a form that makes them useful to prospective users as well as to those developing the plantwide cost models advocated in Recommendation 6 above.

Research and development on individual units of control technology by EPA appears, from the published output, generally to progress in a natural enough way from conceptual stage, through bench tests, to pilot plants, and perhaps to full-scale demonstration.[2] At each of these stages, publications usually describe the idea, the equipment and techniques, and the results in terms of removal levels and other technical matters. All too frequently, however, no useful economic cost data are provided at any of the levels.[3]

It is also worth noting that the reports are likely to be silent about the costs and environmental considerations related to residuals (such as sludges) generated in the treatment process being assessed.

It is, of course, difficult to be accurate about future costs of a process currently in only bench-scale or pilot testing. There are nonetheless both compelling reasons for at least making the attempt and ways of increasing the likelihood of obtaining useful results. It is important to know early about the costs of new processes, first because those who must plan, regulate, and legislate in the environmental field should be in a position to anticipate the impacts of these processes on regulations and environmental quality.* Second, if the public managers wait for actual applications to produce firm cost estimates, they often wait in vain, since private users often treat their cost experience as proprietary information.

Difficulties in the way of producing useful cost estimates for individual process units in research and development are real, but there are three steps that could be taken to ensure that better numbers result from these studies:

1. Research groups should include an engineer who is familiar with existing treatment technology and standard process components, since many new processes are actually recombinations of existing units or at least will use structural components already available (e.g., tanks, valves, aeration equipment, skimmers, etc.). It may be possible to produce quite respectable estimates of cost figures from knowledge of the prospective use of such standard components and other standard inputs such as labor, water, steam, fuel and electricity.

2. Sound economic principles should govern when making and--perhaps as important--when reporting cost estimates. Assuming the best possible success for the effort described in (1), the results will not be particularly useful if costs are not carefully assigned to meaningful categories; if the report simply states an estimated annual cost without detailing the assumptions used and the individual cost items; if discounted and undiscounted capital costs are not distinguished; and so forth. It is particularly useful if cost estimates are paralleled by information on the physical units involved (kilowatt hours of electricity, gallons of water, hours of labor, pounds of chemicals, etc.). This allows the use of alternative price sets by other researchers and the easy conversion of costs to other years.

3. If it is possible to run the process unit at different efficiencies by varying the level of mix of inputs, this fact should not only be noted, but the costs of each of several levels estimated.

14. Secondary Sources of Pollutants

<u>EPA should expand its support of studies on the generation and control of sources of pollutants whose origins are in the transformations of residuals in the ambient environment</u>.

We define the term "primary residual" to denote a direct discharge from an identifiable source. Here the cause and effect relationship between the source and the resulting impairment is usually, although not always, clearly perceived. This is less often the case for secondary pollutants, the products of interactions in the ambient environment, which, acting as a reaction vessel, generates new substances whose origins may be difficult to trace. The most troublesome condition involves secondary generation in a medium other than that receiving the original discharge--the intermedia problem. Research on control of this problem is handicapped by constraints imposed by media-specific legislation and waste management practice. Only in the last decade or so have we become aware of the importance of secondary and intermedia effects. Except for the National Environmental Policy Act of 1969, this awareness has yet to be reflected adequately in environmental legislation.

Workers in many fields have sought, and continue to seek, solutions for the problems of secondary pollutants. Current research is, for example, being directed to the characterization of seepage from sanitary landfills, of complex substances resulting from interaction of sunlight with constituents of automobile and industrial stack emissions, and of sulfate particulates generated by sulfur dioxide emissions. Eutrophication of surface waters resulting from excess influxes of nutrients, natural as well as man-induced, has been studied for many years. Increasing attention is being given to unique problems of source control associated with the role of precipitation in depositing airborne pollutants of uncertain origin to land and water surfaces (Likens and Bormann 1974). However, while many research programs may be described, few successes may be cited. The eutrophication problem is a case in point. Although this problem has been studied for decades, our understanding of the phenomena of algal blooms and their disappearance is still inadequate to permit sure prediction and control.

There is no dependable alternative to federal support for research on residuals transformations in ambient environments, on transport of residuals across environmental media, and on the ultimate effects of the products of transformations. Although a consideration of fates and effects of residuals does not fall within the purview of this Panel, this knowledge is essential to intelligent residuals management. Without this understanding, control is impeded by uncertainty with regard to the sources of the problem, or even worse, results in wasteful investment in ineffective control systems.

We can provide no new guides to the task of producing answers to the difficult problems that challenge us. Persistence and continuing research support are, however, essential ingredients in this effort. We have noted that present media-specific legislation administered by EPA weakens the approach to research on control of secondary and intermedia problems. It is timely that this obstacle be removed.

15. Resource Recovery

EPA should support research that will reduce the losses of valuable materials in wastes.

The logic of recovering materials in wastes that have value in reuse has long been recognized, and, in certain cases, has been applied successfully. One might note, in fact, that few components of a waste are completely without value. Nonetheless, recovery of waste components does not as yet represent a major thrust in source control practice. The reasons lie in the nature of traditional waste management institutions, weak motivation, unfavorable economics, and unsolved technological problems.

Several recommendations in this report are applicable to resource recovery in source control: Recommendations 1 and 2, which describe research opportunities on waste management institutions; Recommendation 4, which stresses the need for research on economic incentives; Recommendation 5, which points to the need for research on personal perceptions and attitudes; and Recommendation 11, which is concerned with problem anticipation. Improvement in the technology of separating and conditioning valuable work components is an essential component of a comprehensive research strategy dealing with recovery of resources in waste. In certain cases it reflects the most critical need.

Sludge produced by urban wastewater treatment plants offers a good example. It is perhaps the single most troublesome residual in terms of rate of production, nuisance character, and cost of handling. Yet sludge constitutes an important resource because of its soil building and nutrient values. Many believe that if sludge could be freed of toxic chemicals, particularly heavy metals, its disposal in marine waters or on land would produce benefits in improved productivity far outweighing any adverse effects that might result. It is conceivable that this may be accomplished through modified wastewater management institutions supported by appropriate economic incentives. Even so, it is unlikely that present policy on disposal of sludge can, or should, be changed in the absence of technological capability to remove hazardous substances.

Practicable systems for recovery of valuable materials in solid wastes have for years seemed close at hand. However, the few that have been tried in the United States have foundered, presumably for lack of acceptance and interest, unfavorable economics, and an inadequate technology.

We have stated that the logic of resource recovery in waste management is generally, even intuitively, recognized. We have also suggested that a successful plan for resource recovery must be based on a strategy of improving the waste management institution, increasing the motivation of those served by the system, providing incentives (including economic ones), and improving the technology of waste handling. The key word here is conservation. It must be impressed on those served by the system, as well as those serving the system. EPA's research should be broadened to reflect this strategy.

NOTES

1 See the report of the Panel on Fates of Pollutants (NRC 1977a) for a detailed discussion of the processes that create secondary pollutants and of principles for use in determining research needs in this area.

2 The comments made here apply with particular force to the industrial setting, but it is our impression that the general message is relevant as well to nonpoint source control techniques, such as those for agricultural and construction-site runoff. "Units of control technology" should, of course, be understood to mean not only treatment processes in the usual sense, but also options for recycling and by-product production.

3 This statement is partly based on examination of 23 publications in the Environmental Protection Technology and the Water Pollution Control Research Series of EPA Research Reports. Of this total, 22 are monographs, and of these, five contain no cost information at all; seven contain vestigial cost information which is essentially useless either because of poor presentation or bad economics; five could be classed as fair in terms of their cost estimation; and five good or better. Only one is really excellent. One report contains a set of papers from a conference. Of these papers, 76 percent have no cost data at all; less than 10 percent would even rank as fair in this dimension. On the other hand, models of useful cost estimation may be found in the work on municipal treatment processes done at the Taft Center in Cincinnati (Smith and Eilers 1970, Smith and McMichael 1969, and Gregorio 1968).

4 If new control technology really represents "progress" it ought to reduce the cost of meeting existing discharge constraints, or ought to imply a smaller discharge in response to a given effluent charge set. The question, of course, is <u>how</u> much.

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APPENDIX

WORKING PAPERS

The following papers were prepared to support the work of the Panel on Sources and Control Techniques of the Environmental Research Assessment Committee:

Prediction of Environmental Problems

Robert H. Harris
Environmental Defense Fund
Washington, D.C.

Talbot Page
Resources for the Future, Inc.
Washington, D.C.

Summary of Recommendations for Research on Legal and Institutional Dimensions of Residuals Management

Edward I. Selig
Bracken, Selig, Padnos, Baram and McGregor
Boston, Massachusetts

Characterization of Airborne Residuals from Point, Line, and Area Sources

Craig D. Hollowell
University of California
Berkeley, California

Robert J. Budnitz
Lawrence Berkeley Laboratory
Berkeley, California

The Characterization of Waste Effluents

Khalil H. Mancy
University of Michigan
Ann Arbor, Michigan

Characterization of Residuals: Industrial and Municipal Sludges and Slurries

Robert S. Ottinger
TRW, Inc.
McLean, Virginia

The Distribution of Pollution Control Costs

Paul R. Portney
Resources for the Future, Inc.
Washington, D.C.

Maintaining the Efficiency of Industrial Residuals Management Systems at the Design Level

William J. Katz
Envirex, Inc.
Milwaukee, Wisconsin

Maintaining Efficiency of Municipal
Residuals Management Systems at the Design
Level

Walter E. Garrison
Los Angeles County Sanitation
Department
Whittier, California

Potential Contributions by the Social and
Behavioral Sciences to Environmental
Protection Research

Lou A. McClelland
University of Colorado
Boulder, Colorado

Stuart W. Cook
University of Colorado
Boulder, Colorado

Gilbert F. White
University of Colorado
Boulder, Colorado

Copies of these reports may be obtained by request from the Environmental Studies Board, National Research Council, 2101 Constitution Avenue, N.W., Washington, D.C.