today appear to require a politically unacceptable level of sacrifice. This lack of public understanding, support, and willingness to bear sacrifices did not change until the debacle at Pearl Harbor. After World War II, the Truman Doctrine and the Marshall Plan, by most accounts, enhanced our security for decades. But public support for these policies emerged only after a devastating war that took over 400,000 American lives.

This historical analogy raises a central question: How much punishment will be necessary before we take oil supply interruptions seriously? Seven years have passed since the Arab oil embargo exploded on the world scene, leaving inflation, recession, and disruption in its wake. Two years ago the Iranian revolution set in motion similar forces. The troubled waters of the Persian Gulf seem to be inflamed, not soothed, by oil. And yet we have not faced up to the challenge posed by supply interruptions—a challenge that affects every aspect of our personal lives, our economy, and our position in the world. Further delay in facing the hard decisions will cost us dearly.

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6. Stock releases reduced this level to 75 million barrels by the beginning of December 1980. Estimate derived from *Weekly Status Report*, Department of Energy, 16 January 1981.
7. During the Iranian crisis, shortages reached 8.5 percent of gasoline supplies, leading to the specific shortages described earlier.
8. D. Robinson, assistant administrator, Economic Regulatory Administration, testimony before the Senate Energy and Natural Resources Committee, 2 June 1980.
9. For large interruptions, greater weight is given to imports than to consumption in allocating shortages. Since the United States imports only about 40 percent of its oil, it would face smaller percentage shortages than Japan, which imports nearly all of its oil supply.

Research in EPA: A Congressional Point of View

George E. Brown, Jr., and Radford Byerly, Jr.

The fact that a problem will certainly take a long time to solve, and that it will demand the attention of many minds for several generations, is no justification for postponing the study. . . . Our difficulties of the moment must always be dealt with somehow: but our permanent difficulties are difficulties of every moment.—T. S. Eliot (*1*).

This article arises from what we have learned in the last few years in the course of congressional oversight of the Office of Research and Development of the U.S. Environmental Protection Agency (EPA). It addresses the basic question of how research can best serve the needs of that agency, and it is aimed not only at EPA managers, advisers, and researchers but at all who share responsibility for the conduct of research programs in regulatory agencies, including members of Congress, the Office of Management and Budget, and the Office of Science and Technology Policy.

The message we wish to deliver is first that achievement of EPA's regulatory mission demands a foundation of basic information which must be built through a program of rigorous basic research, and second that this means a change in the way research is viewed and managed in EPA. Scientific quality must become the first criterion for research programs. No matter how "relevant," proposed research that does not meet this standard should not be funded.

The Nature of Research at EPA

The EPA Office of Research and Development (ORD) is one of six major units of that agency. Three of the others are responsible for the development of pollution abatement programs, and one is responsible for enforcement activities. A fifth unit is responsible for agency-wide planning and management. We refer collectively to these five other offices as program offices.

One of our fundamental premises is that EPA should conduct or fund only such research activities as will support its mission. That mission is defined in large part by several federal statutes, principally the Clean Air Act, the Clean Water Act, the Safe Drinking Water Act, the Federal Insecticide, Fungicide, and Rodenticide Act, the Resource Conservation and Recovery Act, the Noise Control Act, and the Toxic Substances Control Act. The details of the laws provide guidance to research managers. Thus for our purposes the agency's mission (now seen to be manifold) is well defined, and the need is to translate legislated regulatory objectives into criteria for managing research.

Research being planned or conducted in the present will bear fruit only in the future, but the problems facing the agency exist now; so the question for EPA research managers becomes one of how to plan and operate a program that will be supportive of the immediate agency mission. Part of the answer lies in the realization that while the problems facing EPA indeed exist in the present with terrible urgency, they are likely to be disappointingly similar and just as urgent in the future. For example, even after years of research there are still fundamental questions concerning the best way to control photochemical oxidants (*2*).

Because of the regulatory (and thus adversary) nature of EPA's mission, in order to be supportive the research must withstand rigorous scrutiny. Litigation has come to comprise a significant element in the overall EPA program. What is not clear is the degree to which EPA in response to this turn of events must prepare or preserve a legal chain of evi-

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dence. So far the agency does not appear to be substantially controlled by such considerations. It might be argued that preoccupation with legal accountability could have a damaging effect on research. But in fact, there have been at least some cases in the past where the agency did not exercise data quality control even at the level of normal scientific standards (3).

Summary. From lessons learned in several years of overseeing the research programs of the Environmental Protection Agency, the authors conclude that the agency should give more support to the accumulation of the scientific "intellectual capital" needed for managing the environment in the long term.

A continuing problem that faces EPA research managers is the need to do basic research versus the demand from regulatory program offices for research results immediately applicable to abatement purposes. For example: Provisions of the Clean Air Act call for "state implementation plans" to achieve "national ambient air quality standards." In principle, what is needed is a determination of the relation between ambient air quality and pollutant emissions. Once the relationship has been established, emissions can be controlled by a state through its implementation plan; that is, one can work backward from a desired ambient air quality to calculate allowable emissions and from this determine an abatement and enforcement strategy. Thus, the regulatory side of EPA demands models relating emissions to air quality. Unfortunately, even after years of work, good models do not exist for pollutants such as photochemical oxidants and sulfates, because the fundamental atmospheric science is simply not far enough along. What started out as a straightforward applied research program to "develop a model . . ." becomes a basic research program (4) asking, "What is the mechanism for . . .?" What seems to happen over and over again in environmental programs is that the policy-makers run quickly through the "packed-down" knowledge existing in published sources and into areas of fundamental ignorance. Perhaps this is not surprising, inasmuch as the environmental sciences received inadequate attention for years, but it is unnerving, given the importance of the regulatory decisions that are based on our present unsatisfactory understanding (5) and the fact that this situation has been repeatedly cited (6–8).

Another recurring problem is how to set health-based standards. Because we cannot experiment freely on human beings, there is the fundamental question of how to use high-exposure animal tests to set low-exposure human health standards. If we try to avoid this problem by doing epidemiological studies, other difficulties arise: how to measure the health of large numbers of people unobtrusively, how to measure the effect of one pollutant (or two) in a very "noisy" world, how to obtain incontrovertible results in timely fashion.

To cope with the need for immediate results and also with the need in many matters for the basic understanding that is lacking, the agency should have under management a wide spectrum of activities ranging from routine testing to very advanced research. This is not to say that all fundamental research must be allowed, but rather that research aimed at understanding certain well-defined areas of EPA interest must be encouraged. In each part of the research program (in the air program, the water program, and so on), the balance between the kinds of work to be supported must be determined by analysis of the problems at hand. What we emphasize is that agency management must not arbitrarily or automatically eliminate or discourage any part of the research spectrum. Further, it is clear that EPA cannot conduct all the basic research it needs in biomedicine, aerometry, physical chemistry, and so forth. Programs in other agencies can be used, however, and EPA's research management must support, encourage, and communicate with these other programs; they must be a part of the research spectrum from which EPA can draw. This course is not without problems, such as slowness of response and conflict of interest. EPA cannot expect a sister agency with its own mission to respond as a contractor would, but this only makes it more important for EPA to know what its real, long-term information needs are and to continue to point out the interesting research that would meet these needs.

Table 1. Environmental Protection Agency's research budget for fiscal year 1980, by "medium" and "discipline," in millions of dollars. Totals may not check because of rounding. The table does not include $4.6 million for management.

Medium	Discipline					Anticipatory	Total
	Health effects	Ecological effects	Transport and fate	Monitoring	Control technology		
Air	27.1	2.4	12.6	20.6	4.1		66.8
Water quality	7.0	15.2	1.4	12.2	29.3		65.2
Drinking water	12.0			1.0	9.7		22.7
Solid waste				0.5	13.7		14.2
Pesticides	5.8	2.7		0.5	3.9		13.0
Radiation	3.0			0.2			3.1
Toxics	13.9	7.4	2.3	2.7	1.6		27.9
Energy	18.2	15.3	7.2	8.2	52.5		101.4
Intermedia	5.1			2.9		14.7	22.7
Total	92.2	42.9	23.5	48.8	114.9	14.7	337.1

Resources Available for EPA Research

We turn now from general comments to description of the resources devoted to research in EPA. For fiscal year 1980 EPA's research budget is $337 million out of a total operating budget of $1287 million. (In comparison, the 1980 budget of the National Cancer Institute is $937 million, the environmental research budget in the Department of Energy is $277 million, and the budget of the National Institute of Environmental Health Sciences is $79 million.) The EPA research budget may be presented usefully in the form of a matrix (Table 1) in which the columns correspond to the various "disciplines" of research (health effects, control technology, and so on), and the rows, labeled by what EPA calls "media" (air, water, and so on), correspond to the regulatory statutes. Programs not reflective of specific regulatory legislation are included in three separate rows: the radiation medium refers to work that EPA does on the health effects of nonionizing radiation, the energy row represents work initiated in response to the oil embargo of 1974, and the row labeled "intermedia" represents work that cuts across media, for example, quality control activities that apply to monitoring in several spheres. The intermedia category also includes the "center support program," under which there is intended to be long-term support of linkages between basic and applied activities in key areas such as epidemiology. [For details of ORD's programs see (*9, 10*).] Another $50 million to $75 million worth of technical work is carried out in the program offices and is reported as research in some budget analyses; for the most part that work is limited to testing, monitoring, and assessment activities.

The staff of ORD numbers about 1750. It has several laboratories, but over half its budget of $337 million is spent extramurally. If the fraction of work done inhouse by EPA staff continues to decline, as it has for several years because of staff cuts and budget increases, the agency may be forced to consider establishing national laboratories such as the Department of Energy has.

The funding for some program categories is small (Table 1), and one can see why for those categories there is such a struggle over which work gets top priority. These resources are arrayed against an even more impressive matrix of problem areas. Indeed, the problems facing EPA could be displayed in the same matrix, and each element would consist of a long list of items (*11*). The point is that although EPA has a large research budget, it is not large compared to the mission, and therefore the agency must use its resources most wisely.

Congressional Hearings on EPA Research Management

Oversight of EPA's research program was a major activity of the Subcommittee on the Environment and the Atmosphere for several years (*12*). Because of the evident persistence and apparent importance of several research management issues, hearings were held in 1978 to address them in a comprehensive manner.

In planning the hearings, we were aware of studies critical of the tendency of EPA's research program to be crisis-oriented. For example, the National Academy of Sciences (NAS), in a study in 1977 of EPA research and development, recommended that "more research . . . be done in anticipation of decisions rather than in response to crises" (*1*, p. 4). We were also aware that a somewhat different perception of ORD and thus a different set of criticisms existed within the agency itself. This was expressed by the head of the Office of Toxic Substances (OTS) at the 1978 hearings: "Over the years, the system for coordination between ORD and OTS has not always worked well. On occasion, research results have been delivered late or not at all. There have been instances in which final reports have addressed problems different from those originally agreed upon" (*13*, p. 86). On comparison of the NAS findings with the criticisms voiced by the program offices, two distinct perceptions of the role of ORD emerge: scientists seem to perceive the program offices as demanding "response to crises," whereas the program offices see the scientists as wanting to follow their research where it leads them, without regard to agency missions and deadlines. Although not logical opposites, these two views are sufficiently different to cause major difficulties in planning, managing, and evaluating the research program of the agency. In light of such divergent perceptions, and because of the consensus ZBB (zero-base budgeting) method by which program funding decisions are made in EPA (*14*), we sought the views of all the major EPA officials in the hearings, to try to determine the climate of expectations in which ORD must operate.

Perhaps the most significant characteristic of that climate is the pressure imposed by a growing body of regulatory legislation. Referring to the many deadlines imposed by Congress, Douglas P. Costle, then administrator of EPA, compared the agency's job to "having to perform an appendectomy while running the hundred-yard dash." He went on to say that this "has led to a significant portion of the R & D program being directed at responding to short-term regulatory demands" (*13*, p. 3). Certainly Congress has given the agency a great many short-term requirements, and this has led to difficulties in implementing sound research in what is first and foremost a regulatory agency. As the head of ORD testified, "Research results were often not responsive to regulatory needs, nor was the quality of research consistent" (*13*, p. 6). We are reminded of the apocryphal operating principle of bureaucratic support groups: "If you want it bad, you'll get it bad." Perhaps not even a scientific saint could face one unreasonable deadline after another without occasionally responding in this way.

It seems clear to us that EPA's research predicament cannot be overcome or even mitigated solely by making ORD more "responsive." To the extent that the program offices could be characterized as not having a broad or long-range view of either research and development or the agency mission, there is a need to ensure that their demands for research support are tempered by such a view. This is not a criticism of the abatement efforts of the program offices; their need to meet statutory deadlines is well known. They have, however, been somewhat unrealistic in what they have expected to receive from ORD.

The hearings provided examples. The assistant administrator in charge of air programs summarized what he expects from ORD, including assessments of the public health implications of air pollutants (*13*, p. 71). He said, "In most cases, these assessments do not impose new research requirements on ORD, but do call for, in a fairly short-term response, the application of understanding of the nature of the pollutant's health effects" (*13*, p. 70). What seemed to be lacking in his statement was recognition that the "understanding" desired can only come from a program of sound, continuing research. He also described a need for air quality simulation models for testing state implementation plans. This need was seen as "immediate" because of a regulatory deadline imminent at the time of the hearing. Of course, such models have been an "immediate" need since passage of the Clean Air Act in 1970, and will continue to be needed in the foreseeable future (*15*), so the need could more properly be called "continuing." One kind of research can provide incremental information to meet a deadline; another kind would better address the continuing need. If the program offices only approve research to meet "immediate" needs, ORD will not be able to develop the understanding the agency needs.

Another example of mixed expectations came from testimony related to neurotoxicological research. The assistant administrator for toxic substances programs and Administrator Costle agreed on the importance of research in this area. The administrator said, "We have found that a critical gap exists in our understanding of the behavioral and neurotoxicological effects of chemicals" (*13*, p. 4). The assistant administrator described one category of research needed by his program as "long-term, 3 to 5 years, more fundamental research from which the payoff to the regulatory program is more speculative but which serves to keep EPA expertise at the forefront of scientific inquiry. This would include basic research in areas such as behavioral toxicology, epidemiology, and the fate and effect of chemicals in the environment" (*13*, p. 85). When asked why he characterized the payoff of such work as "speculative," he answered with the truism that whenever research is done at the frontiers of scientific knowledge it is impossible to assure that it will yield immediately applicable results. The point we would make is that whenever such a critical gap exists EPA must establish a position at the forefront of scientific knowledge. The position is the payoff, and it is not speculative. The program offices should, through the ZBB process, encourage ORD to achieve such leadership positions, because ultimately this is what will best support their abatement and enforcement efforts (*16*).

Another assistant administrator stated, "Regarding long-term research, I would find it difficult to slight the critical immediate regulatory needs of the program in favor of long-term research projects which, while scientifically interesting, may or may not produce usable data" (*13*, p. 30). Assuming that he is not talking about misdirected research, it would seem from his statement that the terms "critical" and "long-term" are used in opposition—or perhaps it has not been considered that the agency has research needs that are both critical and long-term (such as those in neurotoxicology and photochemistry).

The paragraphs above contain some statements of the assistant administrators who represent the principal "customers" of ORD and who translate work done by ORD into abatement programs through which EPA's overriding mission of environmental protection is effected. The disturbing lesson of the hearings seems to be that the customers do not understand what to expect from a research program. Perhaps they do not pose the right questions; and if their questions are invalid, the answers will be also. In comments surprisingly applicable to EPA research, Henry Kissinger has described the general difficulty the intellectual has with the policy-maker (*17*):

> The contribution of the intellectual to policy is therefore in terms of criteria that he has played a minor role in establishing. He is rarely given the opportunity to point out that a query delimits a range of possible solutions, or that an issue is posed in irrelevant terms. He is asked to solve problems, not to contribute to the definition of goals. . . . In short, all too often what the policy maker wants from the intellectual is not ideas but endorsement. . . . The policy maker sincerely wants help. His problem is that he does not know the nature of the help he requires. And he generally does not become aware of the need until the problem is already critical. . . . Of necessity, the bureaucracy gears the intellectual effort to its own requirements and its own pace: the deadlines are inevitably those of the policy maker and all too often they demand a premature disclosure of ideas which are then dissected before they are fully developed. The administrative approach to intellectual effort tends to destroy the environment from which innovation grows. Its insistence on "results" discourages the intellectual climate that might produce important ideas whether or not the bureaucracy feels it needs them.

It is clear that senior EPA officials see ORD in much this way, and that they do not like to be told that they are not posing the right questions, that "a query delimits a range of possible solutions."

Thus many forces now at work in EPA push its research program in a direction that could be characterized as short-term, limited in scope, routine, and lacking in the stimulation needed to sustain high-grade work. A review of the hearings suggests the following as epitome: "The fact that the need for excellence is constantly invoked is no guarantee that its nature will be understood" (*17*).

There are indications, however, that the research climate may be improving. Administrator Costle, at least, seemed to recognize the need to institutionalize a sound research program: "We believe the Agency has erred in the past by short-changing long-term research. I have been forced, as I said earlier, to make decisions based on inadequate knowledge. If I could leave a single legacy for my successors, it would be for them not to find themselves in that position" (*13*, p. 4). The agency seems to have an idea of what has been wrong and is groping for remedies (*18*). In the next section we suggest a general criterion for future action.

What We Must Do

Our principal assertion is that EPA's research program must be dealt with from a new point of view by Congress, by the Office of Management and Budget (OMB), and most of all within EPA itself. Although OMB is often the naysayer, cutting research funds that do not seem immediately justified, we think that if the agency went to OMB with a budget embodying a new point of view it would be more successful. In this new point of view, research would be considered as developing intellectual capital for the agency. Equally important, scientific excellence would replace expediency as the principal criterion in planning and evaluating programs.

One accumulates capital by foregoing present consumption, and the capital subsequently provides for greater future consumption. Given limited resources, EPA must forgo certain short-term research activity in order to build understanding which in the long run is necessary to accomplish its mission. The accumulation of this intellectual capital through research creates opportunities for progress which would not otherwise exist. An NAS study of the interaction of research and engineering pictures research as "a well of knowledge from which engineering can drink to satisfy defined needs, rather than as a geyser which floods the engineer with solutions to present problems and with clear opportunities for exploitation" (*19*). Typically, past accumulation of intellectual capital has not been planned in a utilitarian way, but today pressures exist to force the accumulation and to predict utilitarian ends of basic research.

Does this help us with the question of how best to manage research in EPA? Consider the tension between the research scientist and the program official (policy-maker) needing usable information. There is often a confusion of ends and means here, as described by W. O. Baker, past chairman of the board of Bell Telephone Laboratories, who argues that in our management we must acknowledge not only the public purposes of science, such as providing usable information, but also the private purposes. Baker says, "The public investment in research and development must encourage this combination of public and private purposes, including some so private that they involve the inner and soulful aspirations of human beings. These personal aims are to understand, to know, to perceive" (*20*). He goes on to describe the personal drives for the "satisfaction of understanding and discovery which are a component of artistic experience." For the policy-maker, the desired end is usable information, while for the scientist the end is in part the satisfaction of understanding. Thus although the very successes of research have tended "to make the public and its representatives in government impatient with the deeper mechanisms of science and research," in order to generate the desired usable information provision must be made for this fundamental scientific priority which often puts understanding (intellectual capital) above application (consumption). Neither wishful thinking nor zero-base budgeting will change the nature of science and scientists. The drive to understand must be tapped and stimulated, not frustrated. For example, if the agency were to dedicate certain funding to the support of peer-reviewed, unsolicited proposals from universities, a great deal of talent and enthusiasm could be tapped. In the 1950's and 1960's major contributions came from the university community by that route, but the situation has changed (*21*).

A second management problem is inherent in EPA's mission: the pressure of numbers—the thousands of chemicals to be tested for toxicity, the hundreds of rivers to be monitored, the numerous enforcement actions demanding attention of various kinds—in short, the massive load of difficult but routine testing. Fulfillment of this mission might seem hopeless under Civil Service regulations with budget and staff restraints and government procurement procedures, but the difficulties point to the solution. The enormous load of routine testing and the concomitant demand for results (data) mean that EPA must have a high-quality source of intellectual capital, a sound research program unencumbered by crisis-oriented management.

First ultimate goals and then specific objectives must be set. The Clean Air Act, for example, requires states to develop implementation plans, and this leads in a fairly straightforward way to specific research objectives. Thus an implementation plan for photochemical oxidants requires an atmospheric chemistry model. Because many unique local airsheds must be controlled, what is needed is a thorough understanding of photochemistry; this is the proper research objective, and it is the intellectual capital needed for achieving the ultimate abatement goal. Hendrik Bode has described most eloquently "some of the usual canons of quality of scientific research" (*22*):

> Novelty is certainly one; the importance of discovering a new and unpredicted phenomenon requires no argument. Beyond this, the principal criterion can perhaps be described as a sort of intellectual efficiency in getting a great understanding of, and command over, nature for a small price. The generality of result and the perspective it sheds on a wide range of situations, in other words, are important indices. The fascinating feature of Newton's Law of Gravitation was the fact that it applied to every particle of the universe. Such a formulation does not rule out the systematic experimental work that constitutes the backbone of science, but it is almost the opposite of defining research as the mere satisfaction of idle curiosity or the indiscriminate heaping up of disjointed facts without pattern or purpose.

Bode's two canons of quality, novelty and intellectual efficiency, represent two important criteria for EPA. Novelty means, among other things, that good scientists do not like to duplicate one another's work unnecessarily. This makes a manager's job somewhat easier, because the higher the quality of the program the more the staff will eschew duplication. Intellectual efficiency and generality of results are important because EPA is in danger of being smothered by the burden of routine testing.

Having a research program of high quality could pay off for EPA also by enabling it to work with other agencies as a leader—not as a "lead agency" in the way OMB uses that term, but as a scientific leader. EPA should become such a leading agency and should engage in cooperative (not dictatorial) research with other agencies. This would require EPA to specify the understanding needed, perhaps to fund special costs, and to solicit interest from other agencies, all the while providing an example of high quality in its own scientific work.

Since essentially all EPA research is intended to support regulatory needs, it is all strongly guided by program officials, who seem not to understand its nature (*23*). What is needed are criteria for use by the agency managers in developing the research program necessary to deal with the intractable problems they face, problems seemingly larger than the scientific resources available. The lack of dose-response information for large numbers of chemicals is an example; one can easily imagine all the nation's scientific resources absorbed in routine toxicity testing. Luckily, competing demands will deny us the luxury of such an intellectually lazy, brute-force approach. Innovation and creativity are needed, and that means EPA must support the best minds to think and experiment on new approaches—to build the intellectual capital.

EPA must build up the best and most creative environmental research program possible, and it must support and encourage such work outside the agency with information, funds, shared equipment, symposia, and so on. The kinds of specific steps that would have to be taken to develop such a program are known (*6–8*). Congress has asked EPA, without success, to take some such steps. For example, the agency has been asked to devote at least 15 percent of its budget to long-term research (*24*); to increase its in-house health effects research staff (*25*); and to use peer review in planning its research (*26*). For various reasons, some beyond the agency's control, none of these steps has been taken. The list of rejected congressional initiatives could be expanded. The lesson we take from this history is that before specific steps are discussed a consensus must be established about the kind of research program needed.

Our recommendation is for rigorous, uncompromised research, part of it done in-house to assure that EPA has the competence to judge its contract research. Agency scientists should not be allowed to wander into irrelevant studies, but the definition of relevance should be broadened to include all the research that must be done. Research must be clearly defined in terms of the agency's mission, and programs must be well focused and yet creative, if necessary, in execution. This latter demand will not make life easy for the agency's managers, but it is a problem faced in many research organizations, and perhaps EPA can learn from others. Striking the balance between mission direction and creative freedom will be difficult, but the managers are working hard now on the fruitless task of trying to solve fundamental problems with limited analytical tools. "Officials—and other executives as well—tend to work to the point of exhaustion as one indication that they have done all that could be asked" (*17*). Surely, striving for the high-quality program recommended here would be more rewarding.

Epilogue

The authors would like to add a more personal note which is not directly related to the achievement of EPA's statutory goals. If EPA's activities can be seen as a small part of a larger effort to make the world a better place for man, then this is our attempt to relate EPA's part to the larger effort. Bronowski (*27*) has said it: "Why is it the business of no one in particular to stop fitting science for death and to begin fitting it to our lives?" It is clear what science can do when it is well supported. Many of the best scientific minds of the world were recruited to work on the atomic and hydrogen bombs, and they made wonderful "progress" for us. But now progress in other directions is needed. We need to realize fully the potential of "research and seeking of knowledge . . . as major civilizing influences, and elements of a free society" (*20*). Specifically, we need to recruit the best scientists into research programs such as EPA's. It is unfortunate that at present the agency has such difficulty in providing a comfortable institutional home for science. It would be a service to mankind to reverse this state of affairs.

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2. *Air Quality Trends in the South Coast Air Basin* (South Coast Air Quality Management District, El Monte, Calif., June 1979). In a letter transmitting a copy of this report to President Carter, dated 6 July 1979, A. A. McCandless, chairman of the South Coast District Board, says, "Despite a multi-billion dollar effort by government, industry, and the public, there has been no net progress in oxidant control in the past six years.

The latest data for 1978 suggests that the situation may, in fact, be deteriorating. . . . we may be losing the war on air pollution."
3. For examples see House Subcommittee on the Environment and the Atmosphere, *The Environmental Protection Agency's Research Program with Primary Emphasis on the Community Health and Environmental Surveillance System (CHESS): An Investigative Report* (Government Printing Office, Washington, D.C., 1976), especially chapters 4 to 6.
4. Without trying to be entirely rigorous, we will use an NSF definition: "Basic research is that type of research which is directed toward increase of knowledge in science. It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study, rather than a practical application thereof." This was given by A. T. Waterman, then director of NSF, in *Symposium on Basic Research*, D. Wolfle, Ed. (American Association for the Advancement of Science, Washington, D.C., 1959), p. 20.
5. For example, the EPA administrator, D. Costle, in a letter dated 12 June 1978 to Senator William Proxmire, chairman of the HUD-Independent Agencies Subcommittee of the Senate Appropriations Committee, said concerning environmental research, "I've had to make too many billion dollar decisions over the last year without the critical information this sort of investment, made five years ago, would have provided."
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10. *Research Outlook, 1980* (EPA-600/9-80-006, Environmental Protection Agency, Washington, D.C., 1980). This presents the agency's 5-year environmental research plan in response to statutory requirement. The plan is updated and a new report issued annually.
11. Many examples of the environmental problems EPA faces are reported in *Environmental Outlook 1980* (EPA 600/8-80-003, Environmental Protection Agency, Washington, D.C., 1980).
12. At the beginning of the 94th Congress (January 1975) the Committee on Science and Technology received jurisdiction over "environmental research" as a result of several changes in the rules of the House of Representatives. The Subcommittee on the Environment and the Atmosphere was formed to handle this jurisdiction and for 4 years (two Congresses), with Congressman Brown as chairman, had responsibility for ORD. In January 1979, as a result of reorganization within the Committee on Science and Technology, Congressman Brown moved to the chair of the Subcommittee on Science, Research, and Technology. The Subcommittee on the Environment and Atmosphere was renamed Subcommittee on Natural Resources and Environment and given some additional jurisdiction.
13. *Environmental Protection Agency Research and Development Issues: 1978*, hearings before the House Subcommittee on the Environment and the Atmosphere, 19 July and 13 and 14 September 1978 (Government Printing Office, Washington, D.C., 1979).
14. In making funding decisions, the agency uses a zero-base budgeting (ZBB) process in which programs are approved by a consensus of the administrator and the six assistant administrators. In the ZBB process, the ORD has only about one of six votes, and thus research programs are vulnerable to a great deal of influence from the program offices. Because they play such a substantial role in defining the program of research ultimately conducted by ORD, the administrator and all assistant administrators were asked to testify on what they expect from that office.
15. *Special Urban Air Pollution Problems: Denver and Houston*, hearings before the House Subcommittee on the Environment and the Atmosphere, 19 and 21 November 1977 (Government Printing Office, Washington, D.C., 1978).
16. *Long-Term Environmental Research in the Environmental Protection Agency*, hearings before the House Subcommittee on the Environment and the Atmosphere, 30 June 1977 (Government Printing Office, Washington, D.C., 1978). See the testimony of R. L. Sansom, especially his supplemental statement, p. 52.
17. H. Kissinger, *The Reporter*, 5 March 1959, p. 30.
18. For example, the agency has instituted a new system of research grants putatively aimed at bringing new work of high quality into its program. Despite this aim the published solicitation for grant proposals does not explicitly and unambiguously state that funding decisions will be based on scientific quality. Instead the following appears: "Scientific merit and relevance of proposals will be significant and balanced factors in the evaluation procedures since all projects must be in concert with the Agency's budget appropriations." In other words, it seems that work on highly relevant matters might be funded even if of poor quality. [See *EPA and the Academic Community* (EPA-600/8-80-010, Environmental Protection Agency, Cincinnati, Ohio, 1980), p. 2.]
19. National Academy of Sciences, Materials Advisory Board, *Report of the Ad Hoc Committee on Principles of Research-Engineering Interaction* (National Academy of Sciences, Washington, D.C., 1966), p. 16.
20. W. O. Baker, in House Committee on Science and Technology, *Seminar on Research, Productivity, and the National Economy, 18 June 1980* (Government Printing Office, Washington, D.C., 1980).
21. Testimony of J. N. Pitts, in *1980 Authorization for the Office of Research and Development, Environmental Protection Agency*, hearings before the House Subcommittee on Science and Technology, 13 and 15 February 1979 (Government Printing Office, Washington, D.C., 1979).
22. H. W. Bode, in *Basic Research and National Goals*, a report to the House Committee on Science and Astronautics (National Academy of Sciences, Washington, D.C., 1965), p. 74.
23. At present the program offices guide EPA's research not only through the ZBB process but also through the mechanism of 13 research committees. These committees translate program office needs into "research strategy documents" which guide all EPA research (*10*).
24. This provision is contained in section 6 of Public Law 95-155, the FY 1978 authorization act for ORD. For explanation of congressional intent see *Conference Report to Accompany H.R. 5101*, 95th Congress, Report No. 95-722 (Government Printing Office, Washington, D.C., 1977).
25. This provision is contained in section 11 of Public Law 95-155. For explanation see the report cited in (*10*), and also *Report to Accompany H.R. 5101*, 95th Congress, Report No. 95-157 (Government Printing Office, Washington, D.C., 1977).
26. This was contained in section 4(a) of H.R. 7099, the House version of the FY 1981 authorization bill. The provision was deleted from the final version of the bill at least in part because the agency strenuously (if informally) opposed it and succeeded in having it removed from the Senate-passed version of the bill. For explanation of intent, see *Report to Accompany H.R. 7099*, 96th Congress, Report No. 96-959 (Government Printing Office, Washington, D.C., 1980).
27. J. Bronowski, *The Common Sense of Science* (Harvard Univ. Press, Cambridge, Mass., 1978), p. 143.
28. We thank A. V. Applegate for substantial assistance in the preparation of this paper.

The Evolution of Cooperation

Robert Axelrod and William D. Hamilton

The theory of evolution is based on the struggle for life and the survival of the fittest. Yet cooperation is common between members of the same species and even between members of different species. Before about 1960, accounts of the evolutionary process largely dismissed cooperative phenomena as not requiring special attention. This position followed from a misreading of theory that assigned most adaptation to selection at the level of populations or whole species. As a result of such misreading, cooperation was always considered adaptive. Recent reviews of the evolutionary process, however, have shown no sound basis for a pervasive group-benefit view of selection; at the level of a species or a population, the processes of selection are weak. The original individualistic emphasis of Darwin's theory is more valid (*1, 2*).

To account for the manifest existence of cooperation and related group behavior, such as altruism and restraint in competition, evolutionary theory has recently acquired two kinds of extension. These extensions are, broadly, genetical kinship theory (*3*) and reciprocation theory (*4, 5*). Most of the recent activity, both in field work and in further developments of theory, has been on the side of kinship. Formal approaches have varied, but kinship theory has increasingly taken a gene's-eye view of natural selection (*6*). A gene, in effect, looks beyond its mortal bearer to interests of the potentially immortal set of its replicas existing in other related individuals. If interactants are sufficiently closely related, al-

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