On the Social Science Contribution to Governmental Decision-Making

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It is often said that a major impediment to effective government is the lack of research knowledge underlying the development, implementation, and assessment of federal policies and programs. An examination is made here of the causes of (i) some continuing failures in integrating research information with decision-making and (ii) some notable successes of the past two decades in matching research capabilities with policy needs. Six recommendations are made to facilitate and further the progress that has recently been achieved.

As the world moves into the 21st century with the long-heralded advent of the "global village," even relatively insignificant government decisions can be expected to affect sizable populations, either directly or indirectly. Already, "small" decisions in the U.S. Department of Defense furnish examples of how this works: closing an army base, say, or delaying a single procurement program, or executing a minor reduction in force can have nontrivial economic, social, or political impacts or a combination thereof. Thus, considerations of the size of an effect alone—quite apart from issues of values or justice—make it critical that governmental decision-makers benefit from the best possible information they can get, relevant to the actions they contemplate.

This is hardly a new idea; philosophers, historians, and social scientists have been talking about it for at least 200 years (*1*). But even by the year 1900, the use of empirical information was still not a regular part of governmental decision-making, and scholars, when they served government at all, tended to give their advice on a purely ad hoc basis.

There are many reasons for this slow progress; two reasons, of course, are that research support is not necessarily useful for all decisions and decision-makers have not always been aware of how or when research might be useful to them (*2*). Moreover, decision-makers in the past have shown some reluctance to call upon researchers for help, because it meant acknowledging uncertainty, and worse, giving up some power and discretion. But even had decision-makers been willing to do so, research support for decision-making was little more than an idea in 1900: modes of application and implementation were not yet developed, and decision-makers still had to be convinced that the results would be worth the costs involved.

Since then, however, numerous scholarly attempts have emerged from many different fields of study to create the means for bringing systematic information to bear, in a focused way, on the major questions and uncertainties underlying those governmental decisions that research can usefully illuminate. The efforts of these scholars to understand how decisions are made and demonstrate the potential of objective information to support those decisions have made possible the improved relation of research to public decision-making that exists today (*3*).

Indeed, progress has been made in terms of both the needed intellectual and technical development and the willingness of government decision-makers to take advantage of the new possibilities. But what about the quality of the decisions themselves? Now that this period of learning has taken place, is there reason to believe that today's decisions are better than they used to be?

I argue that, first, among those decisions that clearly call for a research contribution, some cannot be better because a mismatch of some kind still exists between the research and the decision-making; second, some other types of decisions do appear to be favorably affected today by research findings; and third, a few things could be done that would greatly enhance both the use of research and the effectiveness of tomorrow's decisions, at least in some areas.

What then are the kinds of mismatches that continue to prevent research from supporting decision needs appropriately? I see at least three: (i) when political requirements are so overwhelming that information simply will not be sought; (ii) when information is sought, but contextual or resource constraints on the analysis impede researchers from actually producing the information needed for the decision; and (iii) when "state-of-the-art" research problems allow only inconclusive answers to decision-makers' questions. In any of these situations, there are constraints—either on the decision or on the research—that tend to prevent a match between the two from occurring.

What Research Cannot Do for Decision-Making

At first blush, it seems almost a truism that any important policy decision would be better with stronger information behind it. Experience has shown that it is generally useful to decision-makers for researchers to ask and answer their traditional exploratory questions (for example, what is known and what is unknown about a problem to be addressed by a decision? What policies or programs have already addressed it and how successful were they?). Two considerable advantages can be derived in this way: first, the decision can be based on past experience and knowledge, and second, the fact of ignorance can be confronted because it has been explicitly recognized. This second advantage is especially important because not knowing something in a key area normally means that a policy needs to be cautious and flexible (maybe even reversible)

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with respect to that unknown and that a program flowing from the policy could usefully be small scale and specifically designed to capture the knowledge that is needed.

However, from the experience of the last 25 years or so, it seems that past failures and lack of research knowledge do not always trigger decisions that take them into account. In certain cases, missing information may not have been recognized as missing, so it was not sought, or it may have actually been developed and then disregarded. Often two different and not always congenial rationalities are at work here: that of the researcher and that of the decision-maker. A researcher expects to find areas of ignorance in looking for evidence to support a decision and defines the decision itself as a conclusion resulting from a set of objectively derived premises. But a decision-maker often wants to look only at selected premises and may rule out even the consideration of other information to keep it from getting in the way of an important political purpose. When these two rationalities collide—typically in a critical, time-pressured policy arena—it is, of course, the rationality of politics that dominates the decision.

The Political Realities of Governmental Decision-Making

Political constraints on decisions in the past have sometimes meant that important data were left ungathered, or, when gathered, ignored. Consider Tocqueville's account of warning signs before the French Revolution of 1848 (*4*). According to Tocqueville, Louis-Philippe and his ministers appeared quite unaware, in 1847, of worsening economic conditions in France and growing popular anger about them. Yet neither the considerable efforts made by Tocqueville to inform the ministers nor the data he presented on the floor of the Chamber were heeded. For Tocqueville, the ministers were unaware because they wanted to be unaware: doing something about hunger and unemployment among the poor, he wrote, simply was not consistent with their political objective of governing "for the sole benefit" of the middle class. The problem was clearly not lack of information, but rather failure to use the data that were there.

Similarly, Max Weber, consulted by German military leaders during World War I, advised against initiating a policy of unlimited submarine warfare. The analysis he had made of prior U.S. policy, he said, showed that such warfare would provoke American intervention, followed by what he thought would be a "sure catastrophe" for Germany. Again, the decision-makers had the information but not the political room to consider an objective set of observations and a contrary view of likely outcomes (*5*).

Would things go any better today in the same kind of decision environment? It is not likely. Many may remember Hannah Arendt remaking Tocqueville's point in 1972 as she wondered about the "truly amazing" ignorance of U.S. decision-makers on Vietnam (*6*, p. 21).

> What caused the disastrous defeat of American policies and armed intervention was no quagmire but the willful, deliberate disregard of all facts, historical, political, geographical, for more than twenty-five years.

This "deliberate disregard" of information is all the more conspicuous because there was certainly no lack of competent researchers, analysts, and historians contributing to American policy on Vietnam. Indeed, in the case of the "Pentagon Papers," the analysts, like Max Weber, had been asked by decision-makers to report their findings and conclusions. But in the Washington of the 1960s and 1970s—as in the Paris of 1848 and the Berlin of 1916—no one in government really had the effective political option of listening to what research had to say.

This is not, of course, to argue that research is not needed or should be avoided in politically tight situations in which objective analysis has to compete with extremely strong, probably impregnable, political goals such as power, image projection, or face-saving. For one thing, some research is important to do for its own sake. For another, the checks-and-balances relation between executive and legislative branches sometimes causes objective information that the executive branch refuses to consider to find its way into the public consciousness through legislative or press debate. Furthermore, political situations do change, and findings that are unpalatable to one set of decision-makers may seem quite digestible to their successors.

Contextual Constraints on Research

In other situations, researchers may be prevented, again for reasons beyond their control, from giving decision-makers the information they need. The newness of a particular topic, for example, the immaturity of the research field, and the insufficiency of resources constitute contextual constraints on the work that may preclude the development of a useful research product.

When a policy-maker asks a social scientist for help in planning a response to a problem—President Kennedy, for example, telling Walter Heller in 1962 that he wanted to "do something" about the problems of inner-city youth and asking for "facts and figures" to support a program (*7*)—it makes a great deal of difference how long researchers have been working on the problem and whether they have arrived at a strong enough understanding of it to make a useful policy or program possible. In 1962, juvenile delinquency was only beginning to emerge as a serious national problem with the advent of gang wars and teenage street crime, and various theories about its causes and its relation to poverty had recently come into prominence; some of these conflicted with each other, and most were still hypothetical. But in structuring their effort to resolve these problems, program developers built on one of these still unproven theories as the basis for their program.

When the program later failed—for a wide variety of reasons—the lack of underlying knowledge came in for heavy criticism (*8*). Yet there simply was not enough knowledge available on which to base a program likely to be effective. This hardly means that no program should have gone forward in such a case, only that it should have been small scale and designed to fill the knowledge gaps that existed. But such a program would also have been sadly mismatched to President Kennedy's political aim to do something immediate, big, and imaginative, to somehow respond heroically to a king-sized problem.

Again today, for social problems such as homelessness or teenage pregnancy, the needed basic research has not been completed. Given that knowledge is lacking on, among other things, the predictors and varied characteristics of homelessness and the longer term outcomes of teenage pregnancy, it will be necessary to target federal resources appropriately—that is, by considering and trying to resolve what is unknown—in choosing a program intervention or in coordinating a number of interventions.

The immaturity of a field is as much a problem for determining the effectiveness of an ongoing program as it is for policy or program development. For example, legislative committees that have to decide whether a program should be reauthorized may ask for an evaluation of the program's effectiveness. But if the field is one in which little research has been done, the evaluators soon discover that the program may have little theoretical or empirical foundation, that it may not even have been systematically described, and that neither validated performance measures nor a comparison base have been developed. In such a situation, the evaluators can speak only narrowly, if at all, to the program's effectiveness. Yet to begin doing exploratory studies when a congressional committee is waiting for answers makes little sense: the eventual findings on effectiveness would be delivered to the committee long after its decision had been made.

Thus, research is cumulative; it builds on what has been done earlier, and, if the preliminary work has not been done, it cannot easily be improvised. The result is a gap between the research that is immediately feasible and the information needed for a decision.

Resource constraints act in the same way, depending on the type of question posed, because they may force researchers to rely on extant data and inexpensive study designs and methods. For some decision-makers' questions that can be answered in a few months on the basis of past research, the curtailed funds may well be adequate. Other questions, however, will be less quickly and inexpensively answered if they call for the collection of original data through, say, a sample survey design, a set of cumulative case studies, or an impact evaluation. In this sense, the particular question posed by a decision-maker has pivotal importance for the ability of researchers to support decisions well, because it determines the kind of study that will be done, the time that will be needed, and the cost. Sometimes, to accommodate a particular decision's time frame, it may be possible to couch a question in terms of description rather than causation, depending always on the decision-maker's needs and the research that has already been done. But in many cases the question that most needs a response is precisely the one that researchers cannot answer. Finally, there is another general situation in which there is a mismatch between research and decision-making: when the needed research capability does not yet exist.

State-of-the-Art Research Problems

Despite the progress made over the past 50 years in applying research techniques to decision-making, a number of areas remain weak (for example, counting "hidden" populations, understanding and measuring "quality," dealing with concepts such as risk, and so on). One problem in particular has thus far been proof against the most persistent efforts to resolve it: the ability of research to understand and account for context in a nonlinear, dynamic way. Researchers have learned a great deal about how to measure specific events (and even processes) retrospectively, when there is a trail of occurrences to describe and document and the outcome is known. Even in prospective areas, researchers have developed methods of short-term forecasting that seem to work quite well, as long as all variables other than the one (or ones) being studied can be held constant, and as long as there are enough data points going back far enough in the past to support a mathematical projection. What research does best, in sum, is analyze variables or processes that are discrete enough to be amenable to study.

Research is less successful in understanding the larger context within which those discrete variables and processes presently occur; the future context is little more than a mystery. Yet without a good understanding of how present and future contexts compare, it is difficult to tell a decision-maker how a particular finding from current research might apply to a program coming on-line 5 to 10 years hence.

This is not to say that estimable efforts are not being made to model the future world, taking into consideration a bewildering array of variables. But some things are hard to predict. For example, the Bureau of Labor Statistics 1970 forecast of the 1980 U.S. economy entirely missed the massive entry of women into the labor force that was to occur between 1970 and 1980 (*9*). Similarly, the 1980 *Global 2000 Report to the President* (*10*) on future population, natural resources, and the environment failed to recognize the great changes in the origin and distribution of immigration worldwide that had started in the 1970s and gained momentum through the 1980s. In the same way, most research agendas of the early 1980s did not pick up the acquired immunodeficiency syndrome (AIDS) as an important future global issue, nor could they have noted the coming of "glasnost" or "perestroika" and their likely political and economic impacts worldwide. In addition, the nationwide change in American behavior of the 1980s with regard to smoking, drinking, eating, and exercise was entirely overlooked in earlier forecasts.

The point is that such unpredictable but major developments as these shape the world in which the research findings of the past will be applied. But how useful can predictions about future births and deaths be to decision-makers if their assumptions exclude consideration of the impacts of women in the work force, reductions in smoking, or AIDS? It seems that the farther research tries to peer into the future, the more vulnerable it becomes to the basic problem of knowing which variables will change and how.

In sum, with regard to those decisions that cannot, for one reason or another, achieve a good fit with the research needed to support them, it seems that today's decision-makers are not likely to be much better off than those of the past. On the other hand, in those decision areas where political agendas are not inflamed, the research field is mature, resources are reasonably adequate, and it is unnecessary to predict effects far into the future, progress has been truly considerable.

Decision Areas Where Strong Research Support Can Be Obtained

Decision-makers in the United States can now count on support from research that greatly enhances decision quality along the entire spectrum of the policy and program process. These decision-makers may be executive or legislative branch policy-makers at state or national levels; they may be federal, state, or local managers of programs in any topical area; they may be policy-makers or staffers in central agencies responsible for budgetary functions.

What kinds of decisions have researchers typically been asked to support? Essentially, they seem to be of four kinds: those needed for (i) policy development, (ii) program development, (iii) policy and program monitoring, and (iv) policy and program evaluation (*11*).

Policy development. These decisions cover a fair number of activities, from needs assessments and agenda- or priority-setting to the formulation of a specific approach for addressing almost any public problem (for example, global warming, health care costs, unemployment, and crime control). The researcher's role here typically involves answering the kinds of questions mentioned above, with the purpose of bringing the best available information to the decision-maker on what is known about the problem, how it is changing, what the results of past efforts to deal with it have been, and what needs to be known.

Whatever the issue, the analysis of meaningfulness (how real is the problem?), feasibility (how susceptible is it to solution or mitigation?), cost (what kinds of present and future public expenditures are involved?), and institutional arrangements (what is the policy mechanism best suited to reducing the problem?) are almost always important. Decision-makers have recently used research very well in measuring changes in the welfare of disadvantaged children (the Congress) (*12*), setting a research agenda for transportation policy (the Department of Transportation) (*13*), achieving more precise local counts of homeless populations (the Department of Housing and Urban Development) (*14*), and forecasting the likely impacts of new immigration policy (the Congress) (*15*).

Program development. This may involve many of the same research activities as policy development but especially emphasizes program design. The main research efforts are to ensure a logical fit between the assumptions underlying the program and the program's objectives and activities, use past program experience, or a pilot test, as a basis for setting program objectives, make sure the program's implementation takes practical realities into account and is not so complex as to make it unrealizable, and build into the program (or its pilot test) plans for filling the most important gaps in knowledge and for evaluating both the implementation and the effectiveness of the program. Other forms of program development involve designing demonstrations, experiments, and developmental or operational system tests.

In all of these cases, the researcher's chief role is to design interventions such that the major issues needing resolution can in fact be resolved. Some examples in which program developers have used research well are the Kansas City Police Department's preventive patrol experiment; the Law Enforcement Assistance Administration's victimization surveys; and the National Institutes of Health's and the Food and Drug Administration's randomized clinical trials.

Monitoring. The research role in policy and program monitoring essentially involves the development and use of data systems to examine two things: first, the status of the problem addressed by the policy, and second, the status of the program. Here a decision-maker can use research to track the development of a problem, decide whether to modify a program addressing it, or monitor program targeting, cost growth, and a variety of other factors.

Program monitoring systems can now produce data of such quality, completeness, and consistency over time that they can often be used to answer questions about program effectiveness and problem or program status. Examples of excellent monitoring systems abound, both in the United States [one of good quality is the Fatal Accident Reporting System, developed and maintained by researchers at the National Highway Traffic Safety Administration; another was developed at the Department of Labor for the Comprehensive Employment and Training Act (CETA) program] and abroad (the comprehensive systems of Denmark and Sweden are well known; those of Malaysia and Thailand, among others, were developed in response to the monitoring and evaluation requirements of the World Bank).

Evaluation. Finally, executive branch decision-makers often request program or policy evaluations not only to support resource allocation decisions but also to improve program operations and acquire knowledge in a particular program area; legislative branch decision-makers request evaluations as part of their oversight, authorization, or appropriation functions. I noted above that the questions raised by the decision-maker determine the kind of evaluation to be done, but in any case, the research role is either to conduct the evaluation or critique an evaluation done by others. Many methods have been developed for performing evaluations, but, because all of them present both advantages and disadvantages, one of the most salient characteristics of today's studies is the prevalence of multimethod designs that use the strengths of one method to bolster the weaknesses of another. Such designs may combine qualitative analysis with survey research, for example, or reinforce a quasi-experimental design with a process evaluation.

Here again, examples of strong program studies are numerous, including multiple evaluations of the Headstart program sponsored by the National Institute of Education, the Career Criminals program by the National Institute of Justice, state welfare programs by the Manpower Development Research Corporation (Department of Labor), the Women's, Infants', and Children's nutrition program by the U.S. Department of Agriculture, and health care delivery systems by the Rand Corporation. Policy evaluations are somewhat rarer. Examples are the General Accounting Office's studies of U.S. chemical warfare policy (*16*), the National Highway Traffic Safety Administration's evaluations of minimum drinking age laws (*17*), the Department of Education's studies exploring the potential of a public-private educational policy (*18*), the National Institute of Justice's examinations of deterrence and incapacitation policy (*19*), and evaluations sponsored by the Bureau of the Census and various congressional committees treating the effects of welfare policy on the poor (*20*).

We have learned from these research contributions to today's policy processes that when the analysis is competent and the match between decision and research is good, many knowledge gaps or policy uncertainties can be (and have been) dramatically reduced. Furthermore, the experience of having designed different types of policy mechanisms over the past 30 years—for example, service and demonstration programs, experiments, tests, pilot programs, block grants, tax credits, subsidies, regulation, and public-private partnerships—means that many more options and institutional possibilities are available to decision-makers today than to those in the past. How might this have improved earlier decision-making?

Returning to the Tocqueville example, we find it hard to believe that François Mitterrand could make the same mistake in 1991 that Louis-Philippe did in 1848. Consider the situation (*21*, p. 12):

> The cereal harvest of 1846 had been poor; food prices rose by 100 to 150 percent. The crisis soon reached the textile, mining, and steel industries and workers' salaries fell by 30 percent. Unemployment began to spread. High prices passed over France like a flood, and, like a flood that subsides, left behind a ruined population whose savings were totally wiped out. Often people had to pawn their furniture. The crisis finally shook the banks, and railroad construction was stopped, along with other public works. But this only added to the problem by taking away more employment: 500 million days of work were gone, at 2 francs a day. It is this crisis that was the precipitating factor in the revolution (of 1848).

Today, pretensions of ignorance in such a case would be infeasible for at least three reasons. First, too many empirical studies of unemployment (not to mention periodic data) would have been published in too many research journals (and eventually in the public media) to allow the secret of the problem's existence to be kept; second, survey research and media polls would have left the government in little doubt about the realities of public opinion; and third, there is just too much institutionalized knowledge today about how to administer disaster relief, stimulate the economy, and target work and training programs for an administration not to realize it would have to do something if it wanted to stay in office.

Again, one would not expect any national government of the 1990s to implement an "anti-alcohol" policy that did not even consider the health impacts of drinking. But Finland did just that in the early years of this century (*22*). Similarly, it seems impossible to imagine legislative decision-makers looking to eugenics as a formal basis for immigration policy (which is what the U.S. Congress did in 1911) (*23*).

Decision-makers today can use research readily for a number of purposes—say, to understand public opinion, focus on problem causes, compare policy alternatives, determine which populations need priority help, and target services—in a flexible manner that makes much better decisions possible. However, the constraints discussed above that have always affected the ability of researchers to inform decision-makers are still serious and widespread enough to obviate any inordinately optimistic view that current decisions are unambiguously better than those of a century ago. Furthermore, the relation between researchers and decision-makers remains one of inherently imperfect understanding, based as it is on the uneasy juxtaposition of different kinds of rationality and the dominance of politics over scientific logic in democratic societies. Still, as the contributions of researchers cumulate and decision-makers use those contributions successfully, it seems reasonable to expect that mutual confidence will grow as well. In the meantime, several fairly obvious things could be done to reinforce both trust and understanding between the two.

What Can Be Done to Help?

It is important to channel research findings directly into public decision-making because, quite apart from the decision itself, the very process of using research seems to have a beneficial effect on the bureaucratic structures with which democracies govern. Recognizing and confronting areas of ignorance tends to reduce rigidity. Making policies and programs smaller scale, more iterative, and more dependent on the acquisition of knowledge induces modesty rather than grandiloquence. And incorporating the evaluation mechanisms that allow policies, programs, and performance to be assessed promotes prudence and responsiveness on the part of agency officials. So using research tends to push decision-makers toward moderation but also toward policies and programs that are more likely to work. This said, there are a number of measures that could be taken right now to help ease some of the current problems by bringing research and decision-making closer together.

First, the Office of Management and Budget ought to take the lead in ensuring that more policy research is done, especially in the topical areas highest on President Bush's agenda (for example, drug use, international competitiveness, crime control, education, and so forth) (*24*). This research is important even if political constraints make it impossible for research to inform all of the decisions. Currently, the future of the administration's drug program—proposed in 1989 under quasi-crisis conditions—is once again in question. Because the past program does not appear to have benefited from serious research support, it is today almost impossible to say either what it accomplished or even what may have been learned from it. At very least, the next iteration should carefully consider some of the crucial lessons from the antipoverty efforts of the 1960s: among others, not to implement a costly program nationally that is based on shaky theoretical underpinnings and little practical evidence of likely success.

Second, regulatory agencies should be required to evaluate the results of their policies and programs. Currently, regulatory policy-makers seem to channel more of their efforts toward getting public acceptance for a particular regulation than toward finding out, once the regulation has gone into effect, whether it did any good. For example, it would be useful to learn periodically what the health effects of the Environmental Protection Agency's anti–air or –water pollution efforts have been and how well the Food and Drug Administration's "early warning system" for medical devices has worked in protecting the public against safety hazards. Such evaluations are few and far between, but the costs of some regulatory programs are very high, and it is logical that more research should be devoted to determining their effectiveness.

Third, decision-makers should engage the researchers working with them in a continuing dialogue. Special attention should be paid to the research questions to be addressed and the strength of the design proposed for answering them. Some questions cannot be answered—or answered well enough—by research, and it is up to the decision-maker and the researchers together to determine whether the study design proposed will bring useful enough information to make a costly effort worthwhile. The more a decision-maker is involved in the research, the more likely it is that the study will be helpful and the findings used appropriately.

Fourth, legislative decision-makers should use research more in their oversight function. Congressional debate—and the press coverage that attends it—is one of the best ways of ensuring that executive branch decision-making does not disregard, distort, or obscure objective information that challenges it.

Fifth, universities should take the lead in proposing applied research training—and especially modern statistical methods—for prospective decision-makers in public administration programs. Many of these programs offer little quantitative training, but the public would surely benefit if the next wave of American administrators knew how to use research well, no matter what the branch or level of their administrative decision-making.

Sixth, researchers in government should make more use of outside research help with their work. A "second opinion" that is both independent and expert always strengthens the work, improves its legitimacy, and eventually enhances the likelihood of its use.

There are good reasons to believe in the importance of the partnership between research and public decision-making. Government, unlike business, has no natural basis for making choices, no profit-and-loss statement to show that a program or policy is outmoded or unproductive. An election mandate is typically unspecific. So a strong and skeptical research function is needed to help provide the conceptual foundation for major public policies and programs and to monitor and evaluate their continuing effectiveness. Furthermore, parameters of time, place, and recent history may so constrain decision-makers that their alternative courses of action seem no better than a choice among evils. But choose they must, and researchers are at their best in trading off the subtle differences among available options.

Moreover, when there has been a reasonable match between research and decision-making, the experience has been very good indeed. The increased technical ability of researchers to address policy questions, the number of analyses and evaluations that have had major policy impacts, and the regular nature of these contributions all mean that decision-makers today receive and use more and better information than they did in the past. What is needed now is a climate in which what has been learned can be put into more extensive practice.

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2. The term "research" is used comprehensively in this paper to cover those forms (that is, "basic" and "applied" research) that are needed for decision-making and have been developed within the various social science (and other) disciplines.
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here is not to be exhaustive but rather to show that some research contributions are consistently demanded today, as opposed to the occasional, ad hoc efforts of the past. Thus, no mention is made of some notable research contributions to judicial decision-making, because the process is still sporadic rather than routine, as in certain decision-making processes of the legislative and executive branches.
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24. The General Accounting Office has in fact proposed this initiative to the Office of Management and Budget's director [see *Transition Series: Program Evaluation Issues* (GAO/OCG-89-8TR, November 1988) and *Improving Program Evaluation in the Executive Branch* (GAO/PEMD-90-19, May 1990)]; indeed, the latest federal budget (fiscal year 1992) shows some renewed emphasis being accorded to evaluation after a long period during which executive branch evaluation offices steadily lost both personnel and funds.
25. The views and opinions expressed by the author are her own and should not be construed to be the policy or position of the General Accounting Office.

Reactivity of Organic Compounds in Hot Water: Geochemical and Technological Implications

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Understanding of the reactivity of organic molecules in hot water is developing from studies aimed at explaining how organic matter (kerogen) forms in natural environments and then breaks down into energy source materials. In natural systems where kerogens are depolymerized, hot water is ubiquitous and usually contains salt and minerals. Reactions such as ionic condensation, cleavage, and hydrolysis are facilitated by changes in the chemical and physical properties of water as temperature increases. These changes make the solvent properties of water at high temperature similar to those of polar organic solvents at room temperature, thus facilitating reactions with organic compounds. An understanding of aqueous organic chemistry may lead to potential applications in areas as diverse as the recycling of plastics, the synthesis of chemicals, and coal liquefaction.

THIS ARTICLE DESCRIBES AN EMERGING AREA OF CHEMIStry: the transformations of organic compounds in hot water at elevated pressure. Although conventional wisdom holds that most organic compounds do not react with water under normal conditions, our overview demonstrates that water frequently participates as catalyst or reactant as well as solvent. Specifically, the behavior of compounds with functional groups and linkages corresponding to those found in coals and shale kerogens, and their precursors, implies that water has important effects on the conversion of plant and animal material into organic fuels under geologic conditions of time, heat, and pressure. These results are of broad interest to geologists and chemists and may provide a means for reducing pollution by organic wastes. The implications are beginning to be explored with respect to energy sources and the development of environmentally clean and safe chemistries for chemical synthesis and recycling.

Organic molecules that were previously considered to be unreactive in liquid water undergo many chemical reactions when the temperature is increased to 250° to 350°C; these reactions were previously expected only in the presence of strong acid or base. For example, ethers and esters, which are unreactive to heat alone,

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