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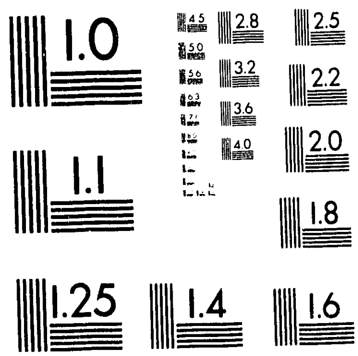

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GLOBAL CHANGE RESEARCH: SCIENCE AND POLICY

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MASTER

Mr. Chairman, members of the committee, thank you for this opportunity to speak with you this morning on the issue of science and policy priorities in global change research. This hearing is particularly timely, as the USGCRP is currently undergoing significant restructuring and the committee has the opportunity to influence that process.

I shall begin my remarks by characterizing certain aspects of the Global Change Research Program of the U.S. Government, and its relevance to the short and medium term needs of policy makers in the public and private sectors. I shall address some of the difficulties inherent in the science and policy interface on the issues of global change. Finally, I shall offer some proposals for improving the science for policy process in the context of global environmental change.

Global Change and the USGCRP

Global environmental change is a broad topic. It encompasses global commons issues where the action of one nation may directly affect the welfare of all, for example, greenhouse gas emissions or ocean pollution. It also may include the cumulative effect of activities that have become so widespread that they must be viewed as components of a global system that influences the way we live on our planet. Examples of this class of global environmental change may include such diverse activities as international trade in several kinds of waste and contamination of water resources by agricultural chemicals. Also, it is generally agreed that, in addition to large spatial distribution, global change issues have time scales ranging from decades to centuries.

A comprehensive global change research program would be concerned with this whole range of issues, exploring their interconnections within a framework of sustainable development. An irtegrated approach is important because global changes are occurring, not in isolation, but as a result of worldwide economic and social development. In contrast with this vision, it is fair to say that to date, the USGCRP has focused almost exclusively on changes in the earth's atmosphere and their potential consequences.

Furthermore, the USGCRP is overwhelmingly a natural science program focussed on basic earth systems processes. The Economics Program is an afterthought, admitted to the program largely because the previous Administration was concerned to emphasize the potential costs of measures to reduce greenhouse gas emissions.

The USGCRP program summary "Our Changing Planet," places considerable emphasis on policy relevance and supporting the needs of decision makers.

> The U.S. Global Change Research Program (USGCRP) was conceived and developed to be policy-relevant, and hence, to support the needs of the United States and other nations to address significant uncertainties in

> knowledge concerning the natural and human-induced changes now occurring in the Earth's life-sustaining environmental envelope.[2]

However, the resource allocations provided in the document indicate that the USGCRP is really a basic science program (Figure 1). This judgment is likely to be strengthened when the actual agency expenditures and research products are scrutinized. Many components of the USGCRP are high-quality projects that may substantially advance the state of the art in various scientific fields. It is equally clear that these studies have had only a tenuous connection to the present needs of public and private decision makers. Indeed, apart from a small pilot study conducted by my fellow panel member Chris Bernabo/Hadi Dowlatabati and a tiny number of detailed interviews conducted by Granger Morgan and his associates, there has been no systematic effort even to discern the real needs policy makers have for information from a global change research program.

The most significant policy impact of the USGCRP to date has been one that was not intended by the scientific authors of the program. Their very proper expressions of significant scientific uncertainty have been invoked frequently to argue for a "fools rush in" approach to climate policy rather than a "stitch in time" approach. But what can policy makers really expect of science in even a decade? Recently re-reading Svante Arrhenius' original paper from 1896,[3] it struck me that our understanding of the big picture facts about the greenhouse effect has not changed substantially in 93 years. It is not reasonable to expect a basic research program such as the USGCRP to make direct contributions to the formulation and evaluation of policy responses to global change in the short to the medium term, by which I mean anything up to two or three decades.

Perhaps it is a mistake for us to expect the natural sciences to provide the sort of certainty that would eliminate political controversy. For example, the National Acid Precipitation Assessment Program (NAPAP) dealt with a set of issues that were admittedly elusive, but far more tractable than global change. After a decade of research, NAPAP delivered a technical smorgasbord from which various concerned parties could select the science to support their existing policy preferences.

The best science-driven analysis may not provide policy makers with the type of information they need to inform decision making, or the information may require extensive interpretation that permits scientists to introduce their own implicit policy preferences. Similarly, the kind of information required by policy makers may seem to a scientist to lack rigor. To take but one illustration, the results of most climate impact assessments have not been expressed in terms that are useful to decision makers, but rather in the traditional biophysical units that are used in scientific disciplines --

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[2]*Our Changing Planet: The FY 1993 U.S. Global Change Research Program*, p.3

[3]"On the Influence of Carbonic Acid in the Air upon the Temperature of the Ground", *Philosophical Magazine*, S.5, Vol. 41, No. 251, April 1896.

changes in forest biomass per unit area, changes in crop yield, and changes in stream runoff. Such information may be of interest to a technical audience, but decision makers may find it difficult to relate this to decisions they must make about whether to grant zoning variances, fund infrastructure, or subsidize agricultural prices.

Scientists often have difficulty accepting that the requirements of useable knowledge for policy frequently are different (not necessarily worse or better) from the criteria of excellence in pure science, just as the canons of legal and scientific proof differ. On the other hand, policy makers often have difficulty articulating research goals for scientists that are both realistic and useful. In addition, policy makers often do not understand the limitations of the present knowledge base, or that research to reduce some kinds of uncertainty probably could not be completed before the climate has changed.

The underlying philosophy of the USGCRP has been "getting the science right," on the assumption (stated or unstated) that we cannot develop sound policy without substantially reducing scientific uncertainty about basic earth system science processes. This idea has been elaborated as the "cascade of uncertainty," the notion that uncertainties inherent in our understanding of basic earth systems are exacerbated by uncertainties over emissions (see Figure 2). In turn, this situation makes anticipation of impacts even more uncertain, especially in the context of global socioeconomic uncertainty. By the time these uncertainties are included in the consideration of policy responses, uncertainty is so great that rational action becomes almost impossible.

The logic of this view is that ideally each area of concern would be investigated sequentially so as to provide a sound foundation for our understanding of the next. If our goal is driven by the basic science motivation to perfect our knowledge, then this logic of uncertainty reduction has some merit. However, from a policy perspective we are concerned with *managing* uncertainty. *Reducing* uncertainty is only one of several alternative strategies for management, perhaps an unappealing strategy if the stakes are perceived as high and the reduction time as long-term.

In any case, it is a mistake to suppose that the reduction of scientific uncertainty will necessarily resolve conflicts over policy. Figure 3 illustrates the relationship between scientific uncertainty and societal decision stakes as factors shaping social decision strategies. Global change issues tend to cluster in the top right of this figure, the realm of political decision making. Reducing scientific uncertainty without simultaneously reducing the decision stakes (addressing winners and losers across a broad range of monetary and non-monetary benefits and costs) still leaves us in the domain of political controversy based on stakeholder interests and world views about the relative vulnerability of economies and ecosystems. This figure also warns us that we do not move out of the realm of political decision making merely by reducing specific uncertainties, for there will always be new uncertainties to which the basic debate over alternative paths for human development and environmental protection will be transferred.

Furthermore, it is misleading to suppose that basic knowledge of natural systems is a necessary precondition of sound stewardship policies. If I were traveling on a bus that was operating in an unsafe manner, the first thing I would do is to check out the driver. Improving my understanding of the internal combustion engine and hydraulic braking systems would be a longer term priority. Yet the logic of "getting the science right" would have me reach for the service manual before checking the health or fitness of the driver.

Global environmental change is not so much a problem to be solved as an evolving context in which life's decisions must be made. Global change is a matter of such complexity that we should not expect unambiguous guidance from science about the kinds of tradeoffs of human and environmental welfare that we will have to make. There will be no technological magic bullet for sustainable development to get us off the hook; we are in this for the long haul. Adjustment of policy makers' expectations of what science can tell us in a foreseeable timeframe may be as important to developing usable science for policy as the design of any research program.

The notion that global change is not a problem to be conquered by science but a condition under which we must make decisions may be a hard pill for us to swallow. Science in American political culture has always been viewed as a problem-solving activity that can be harnessed to politically defined goals. Science has scored some spectacular successes. Smallpox has been eradicated, The U.S. has put a man on the moon. In other cases, progress has been slow and success elusive; we are still fighting the war on cancer declared by President Nixon. If we want to make the best use of the information that natural science can offer, we need to build the social science component of the USGCRP.

Even an ideal natural science program can only assist policy makers to the extent of setting goals, such as stabilizing the earth's atmosphere at a certain level of greenhouse gas concentrations. The natural scientists can tell us nothing about how societies achieve those goals or about the tradeoffs we may choose or be forced to make between those goals and other societal objectives. This is the domain of social science research, which is represented in the USGCRP solely by an Economics Program budgeted at about $20 million over the last four years.[4] This sounds like a lot of money until it is contrasted with the $629 million budgeted for basic earth systems research in 1993 alone. Even allowing that social science technologies and instruments tend to be much cheaper than those of natural science, the fact that natural science research cannot, even in principle, resolve the societal issues of climate change without significantly improved understanding of the human side of the equation speaks for a reexamination of our research priorities and a greatly increased focus on the human dimensions of global change.

Policy-Driven Research

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[4] **The *Human Interactions* component of the program is largely a misnomer as it primarily designates the biochemical components of agricultural activity and such.**

If global change is to be a fact of life, we need to supplement the basic research approach with a policy-driven program aimed explicitly at realizable societal goals. The question is not, "Should we do science or should we do policy?" but how to create an activity we might call "science for policy" and how to define its relationship to basic science research on the one hand and to the business of actually making policy decisions on the other.

Science for policy requires a reflexive approach to both science and policy making that is entirely missing from the USGCRP. Studies would focus on issues such as the contrasting motivations of scientists and policy makers discussed above. Science for policy studies also would examine preferences and criteria for selecting theories, models, and data, as well as the way in which science and policy inquiries are shaped by institutions. The tangible benefits of such studies would be to clarify for scientists the expectations of policy makers and vice versa.

Science and policy studies have the potential to help devise selection criteria to focus research where it can provide useable knowledge for decision makers. Such selectivity is already exercised in current research, but not consistently. Although policy makers may select the gross fields of study (e.g., agricultural impacts or sea level rise), scientific preferences rather than policy needs tend to determine the choice of specific studies within these fields.

The development of selection criteria should be consistent with the information needed for investment and operating decisions in both the public and private sectors, and also should recognize the capabilities and limitations of current scientific data, methods, and theories. When applied, the criteria should give higher priority to research that has a strong likelihood of improving information critical to decisions that are moderately sensitive to environmental change (see Figure 4). If a decision is shown to be robust in the face of climate perturbations, then significant reductions in uncertainty about climate impacts are unlikely to affect the quality of the decision, and the value of the research for policy making (although not necessarily for science) is correspondingly lower.

If a decision is expected to be highly sensitive to uncertain global environmental changes, then extensive scientific efforts during the period in which the decision must be made are unlikely to improve it, especially if it involves very high stakes. Such investments are likely to be avoided on the basis of the precautionary principle or, if they cannot be avoided, are properly made as explicit political choices rather than technical, scientific, or economic decisions. The decision sciences are faced with the task of devising socially resilient strategies for such sensitive decisions that must be made where information is unavailable or highly uncertain.

In these ways, science for policy studies would provide a capability for the continuous evaluation of the USGCRP from both basic science and policy science objectives. Studies such as these are an integral part of other countries' global change research programs, including those of the United Kingdom and

Sweden. In the U.S., these issues have been all but ignored, even by those responsible for identifying a human dimensions of global change agenda.[5]

An authentic policy-driven program for global change research would start from a decision maker's perspective. It is important to recognize that we must be concerned with a wide range of decision makers, not just those responsible for federal policy making. For example, it is possible that we will experience patterns of climate change during the next 30 to 50 years, a time span comparable to the duration of many public and private decisions. Investment decisions for power plants and transportation infrastructure are perhaps the most obvious examples of long-lived decisions, but local zoning decisions also have long-lived impacts. Many decisions of private firms to invest in facilities to extract or harvest natural resources are long-lived. Similarly, decisions that various levels of government make today about how to deliver services and what kind of activities to encourage or discourage through tax, trade, research, or other policy will have long-term implications.
Global environmental change is caused by literally innumerable decisions made at local and regional levels. Yet, research into these decisions that are human driving forces of global change is missing from the USGCRP. Fundamental issues of human needs and wants, demand for goods and services, the relative importance of population size and expectations of welfare improvements, and economic and institutional barriers to the diffusion and adoption of technological innovation would all be priority topics for an authentic policy-driven research program.

The decisions that will ameliorate change or help cope with impacts also will have to be implemented at local or regional levels. As illustrated by Figure 5, very few people have the luxury of thinking about long time scales and geographic reach. A critical component of a policy-driven approach will be to identify the kinds of information that might change decisions at the local and regional level. In this way we can encourage people to act globally even while thinking locally. The elicitation of information needs requires sophisticated social science methods that should be incorporated into the program.

The importance of identifying such a broad range of decision makers is emphasized by Table 1, which identifies examples of two types of social and environmental vulnerability. The first list consists of possible, but highly uncertain long-term secular changes that are widely discussed in the climate impacts literature. At this stage, narrowing uncertainty about the probability and extent of such changes probably should be part of a basic research program in global change. The second list consists of various emergency-type events we may realistically expect to see as much shorter term precursors of climate change. Note that many of these emergencies will hit nongovernmental decision makers first.

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[5] **See, for example, the Global Environmental Change, Understanding the Human Dimensions report of the National Research Council (Washington D.C., 1992 and my review of that report in Environment Vol. 34, No. 7: 25-28, September 1992.**

Interestingly, we do not have to narrow the uncertainties about the probability and extent of the second class of impacts to justify research on how to cope with them. They are happening all over the world right now, largely as a consequence of policies conceived and executed in isolation from one another. Examples include the growth of megacities and the relocation of populations to vulnerable regions through government programs in Amazonia, the Indonesian Coast, and the Sahel. Bangladesh already loses hundreds of thousands of lives due to periodic storm events comparable in force to Hurricane Andrew. A policy-driven research program would directly address the combination of engineering, natural science, economic, and social science that is required to reduce current misery while equipping humanity to resolve future problems.

Essentially, global change is an issue, not of life on the planet, but of the quality of that life. For the geographically exposed poor, the issue is more likely to involve sickness, destitution, and death (e.g., Bangladesh). However, even the world's wealthy are liable to suffer loss of home, community, and investment in infrastructure (e.g., Hurricane Andrew). There is an urgent need for research on the means to lighten the stresses humans place on both their environment and their society, just as we would lighten the load on a shaky but indispensable bridge while conducting a long-term inspection to detect and rectify its structural faults. Neither closing the bridge completely while we await the inspection report nor ignoring the warning signs and proceeding at full capacity are attractive options. Yet comparable options are characteristic of the present debate precisely because of the way both sides focus on uncertainties in the basic science as the justification for their contrasting policy positions.

An Example: Policy-Driven Impact Analysis (PDIA)

Studies of vulnerability and of human drivers can be brought together in PDIA, a framework for decision makers to examine contingencies in their investment and operations decisions. PDIA begins with decision makers' problems and seeks to elucidate the sensitivity of their *decisions* to a range of potential impacts. *Policy-driven* emphasizes providing information useful for policy makers (e.g., government officials, planners, citizen groups). The phrase also stresses the need for the science of global change to collect data and create new knowledge that is also useful for policy makers, rather than being driven by questions that are of interest to scientific disciplines and subdisciplines but may have little relevance in policy analyses. *Impact analysis* refers to the need for policy analysis to evaluate the effects of events upon various socioeconomic systems and the subsequent effects of human interventions. With respect to global climate change, this requires that impacts of changes in climate be related directly to impacts upon socioeconomic systems that policy makers deem important, and that policy makers have some potential to prevent, ameliorate, control, or otherwise plan for these impacts.

The starting point for a policy-driven impact analysis is the decision making unit that originates and/or implements the policy under consideration. Initially we can identify at least three categories of decision makers for whom PDIA would be useful:

- The federal government, especially the Departments of Energy, Commerce, Agriculture, Interior, Transportation, and Housing and Urban Development, plus the Federal Energy Regulatory Commission, and the Congressional Committees having oversight for these agencies.

- State and local government, including state governors, state counterparts of the federal agencies listed above, public utility commissions, county commissions, and mayors or city managers.

- At least six major types of activity in the private sector: public utilities (particularly electricity, water and sewage, and natural gas); transportation; agriculture and forestry; manufacturing (through major industrial associations); oil and minerals; and residential and commercial buildings.

Each category participates in planning for investments and operations over long planning horizons. The time scales can vary even within a single industry according to the decision involved, the organizational culture of the decision maker, and national regulatory environment in which it operates. For example, some U.S. oil companies now regard five years as a long-term horizon, while some European oil companies such as the Shell group build scenarios of the global economy that extend well into the 21st century.

All analysts and decision makers base their recommendations and actions on expectations of the future. Most naively, they may simply extrapolate from current trends, much as electric utility systems planners once projected future load growth from current consumption. Alternatively, many activities use more sophisticated models. These models commonly make assumptions about changes in demographic patterns (size, composition, and distribution of population), socioeconomic transformations (technology, spending power, composition of economic sectors, international comparative advantage in resources), and government policies (regulation, taxation, international trade policy.)

Historically, all such modeling activities have assumed continuity in the large-scale natural environment. Even those that consider seasonal or other periodic fluctuations assume no changes in the underlying period or its effects. To the extent that analysts have considered changing environmental conditions, they have treated them as outcomes of implementing planned activities, rather than as constraints or opportunities impinging upon the original conditions considered by the planner. This is the province of environmental impact analysis.

A key element of PDIA is to rely upon methods now used in sectoral decision making, but to add environmental conditions to the variables such as capital, technological change, demographics, and regulatory constraints that must be specified to conduct the analysis. By making assumptions about the natural environment an explicit part of the planning process, we can examine how changes in the environmental variables might affect investment or operating decisions. These analyses would be informed by a range of plausible climate scenarios for any given geographical location and time scale. The analyses would not be predictions of the future, but sensitivity analyses to determine whether decisions may be robust or vulnerable to potential climate impacts over the lifetime of a given investment. Similarly, by making assumptions about the environment explicit, the analyst or decision maker could alter other variables to see how changing the decision might compensate for climate variability. Examples might include overbuilding infrastructure in anticipation of sea level rise, or changing patterns of water impoundment and release in existing hydroelectric and irrigation systems to respond to changes in precipitation.

For example, because power plants are expensive, take several years to build, and have long operation lifetimes (four to five decades), utilities use a variety of sophisticated planning models. These models include load forecasting, capacity expansion, production costing, and financial analysis.

The load forecasting models estimate the future demands for electric energy and the peak demands likely to be imposed on the utility. Annual electricity (GWh) and peak demand (MW) are usually computed for individual customer classes (e.g., residential, commercial, and industrial) and sometimes for individual end uses (e.g., lighting, space conditioning).The capacity expansion models help utility planners decide on the type, size, and timing of new power plant additions. These models are often optimization models that automatically identify the least-cost mix of power plants. These models identify capacity additions that best fit the existing power-supply system and the temporal patterns of customer electricity use, assessing plants with different capital and operating costs. Recent models, in particular the EGEAS model developed for the Electric Power Research Institute by the Massachusetts Institute of Technology, incorporate demand-side management programs into the capacity expansion algorithm.

Production costing models simulate the hour-by-hour dispatch of a utility's mix of power plants. These models are used to identify the most economical way to operate plants to meet the time-varying demands for electricity during a year. These models also show the outputs from individual power plants at different levels of temporal aggregation.

Finally, the financial planning models take outputs from the other models and compute income statements, balance sheets, and other financial information for the utility. These models help planners decide on the financial feasibility of different expansion plans. These results are also important inputs to rate making (setting electricity prices).

Ideally, these models should be sensitive to changes in the climate and show how the utility's operations affect the global environment. Many models can readily handle the second issue; it is straightforward to convert estimates of fossil-fuel consumption from the production costing model into estimates of emissions of carbon dioxide. However, the models generally do not incorporate variables that show how changes in the climate would affect decisions to acquire generating resources or to operate a utility system. For example, utility models do not account for the effects of climate change on:

- effects of changes in summer and winter temperatures on the demand for electricity for air conditioning and for space heating,

- output from hydroelectric facilities, which would be affected by precipitation regime, or

- heat rates and outage rates for power plants that use lake and river waters for condenser cooling, which would be affected by temperature of surface waters.

Similar kinds of models exist for decision making in transportation, town and regional planning, macroeconomic decision making, and other activities.

The analyses to be performed in PDIAs will be specific to the type of investment or operating decision to be made, the role of the decision maker in the political or economic system as a whole, and the region in which the decision is implemented. However, many decisions must be made within each region, and the development of background information about climate, possible changes, and possible responses of resources to climate will be needed for each analysis. Integrated regional resource assessments would be a key component of a system of policy-driven impact assessment; they would provide a context of climate change and resource response in which to conduct sectorally- and temporally-specific analyses using the investment and operation decision making models discussed above. However, the state-of-the-art in integrated regional assessments is limited by some of the methodological and other shortcomings noted earlier for resource response modeling. Improvements are needed especially in the following:

- linking models of different, but interconnected resource sectors that may operate on different temporal and spatial scales

- applying the results of models across temporal and spatial scales

- representing the mechanisms by which resource sectors respond to climate change

- making the best use of available climate models to provide plausible ranges of inputs

- accounting for conscious adaptation by human populations as they experience climate change

- representing the mechanisms by which a region responds to changes in other regions that are not part of the assessment.

Eventually, PDIA could be conducted using a decision support system (DSS) that combines sectorally-specific decision models and integrated regional assessments (see Figure 6). The DSS would provide automated support to policy makers at various levels of government and within different kinds of major institutions such as utilities, to assess the impacts of global climate change and to create and evaluate policy options to meet these challenges. The purpose of such a system would not be to predict the future or to automate the decision making process; rather, it would be to facilitate human decision making by displaying ranges of options, costs, and consequences under carrying assumptions about the future, including great uncertainty about its climate.

Research concepts like PDIA provide opportunities to bring together regional economic and environmental impact analysis with decision theoretic concepts in an integrated machine environment using advanced visualization for presentation of information in a form that actually relates to real decisions of real people.

Institutional Issues for Implementing a Policy-Driven Program

I have identified some characteristics and priorities for a policy-driven research program in global change that would bring the science and policy worlds closer together in answering the real needs of decision makers at many levels of society. One obvious institutional question is where should such a program be located? This is a delicate problem. If the policy-driven program is to evaluate the basic program, should both be incorporated under the USGCRP or should they be independent? On balance, I would favor location of both programs under a single USGCRP umbrella, provided certain changes are made in the USGCRP. These would have to include an explicit mandate and sufficient resources for the policy and decision sciences to participate as credible partners in the policy-driven program. If this condition is not met, it is inevitable that the program will continue to provide natural science answers to policy questions.

It is possible that the new Assessments Working Group of the USGCRP could fulfill this function. However, a condition of its success in doing so depends on its ability to shed the legacy of the Mitigation and Response Strategies (MARS) Working Group to which it is a successor. MARS was condemned to inaction by both external and internal failings, including an ill-defined mandate, absence of support from the Administration, lack of support from the basic science components of the USGCRP, lack of financial resources, inability to define a vision for itself, and lack of leadership.

I cannot express enthusiasm for completely new institutions such as the proposed National Institutes for the Environment. There is no reason to suppose that an NIE would be any less prone to the dominance of big science than the USGCRP has been. Also, such drastic institutional innovations often end up as a shell game in which scarce resources that could support research get lost in the bureaucratic shuffle.

In considering the future role of the National Laboratories, the Committee may wish to explore the opportunities that exist in the broad mission Labs. Oak Ridge, Pacific Northwest Laboratory, Lawrence Berkeley, and Argonne have the interdisciplinary traditions and the culture of policy-driven research that would be required to make the vision of policy-driven research I have described here a reality. These Labs have significant social science capabilities in anthropology, agricultural economics, economics, epidemiology, geography, information science, law, political science, phychology, resource and land-use planning, sociology, and transportation planning, in addition to the natural science capabilities they currently contribute to the USGCRP. National Lab scholars in these fields also have the benefit of extensive experience in interdisciplinary research on energy and environment issues. Many have published or contributed to significant research on aspects of global environmental change as diverse as energy/land-use emissions modeling, international negotiations, policy making and implementation, land-use change, energy technology penetration, regional and global development, human health impacts of elevated UVB, and global environmental perception. What the National Labs and the agencies that fund their research clearly lack at present is a collective institutional memory that facilitates long-term learning and reduces redundancy in all aspects of their policy-relevant research, not just on global change.

It is equally clear that the Labs alone do not begin to approach the specialized capabilities universities have in many of the areas of natural and policy science that would be required. While the Labs are consummate interdisciplinary weavers, they also depend on university scholars to spin the disciplinary threads, especially in social sciences. Clearly, efforts to develop an authentic policy-driven research program for global change would benefit from removal of the many financial and institutional obstacles that currently impede effective collaboration between universities and National Laboratories in the social sciences, where budgets tend to be smaller and time frames for research much shorter than in big science.

Finally, it must be conceded that in many areas, the social sciences require a basic research counterpart to a policy-driven program, just as the natural sciences will continue to support a policy-driven program with a basic science effort. Support for university-based social scientists to develop methodological capabilities and to expand the knowledge base of social science aspects of global change would be an essential ingredient of a well-rounded program. This could be achieved through expansion of the human dimensions of global change program at NSF. Overall, the goal should be to strengthen existing institutions in what they already do well and improve communication and collaboration among them through a policy-driven research program.

Thank you, Mr. Chairman, I would be pleased to answer questions.

Figure 1. U.S. Global Change Research Program Budget by Science Element

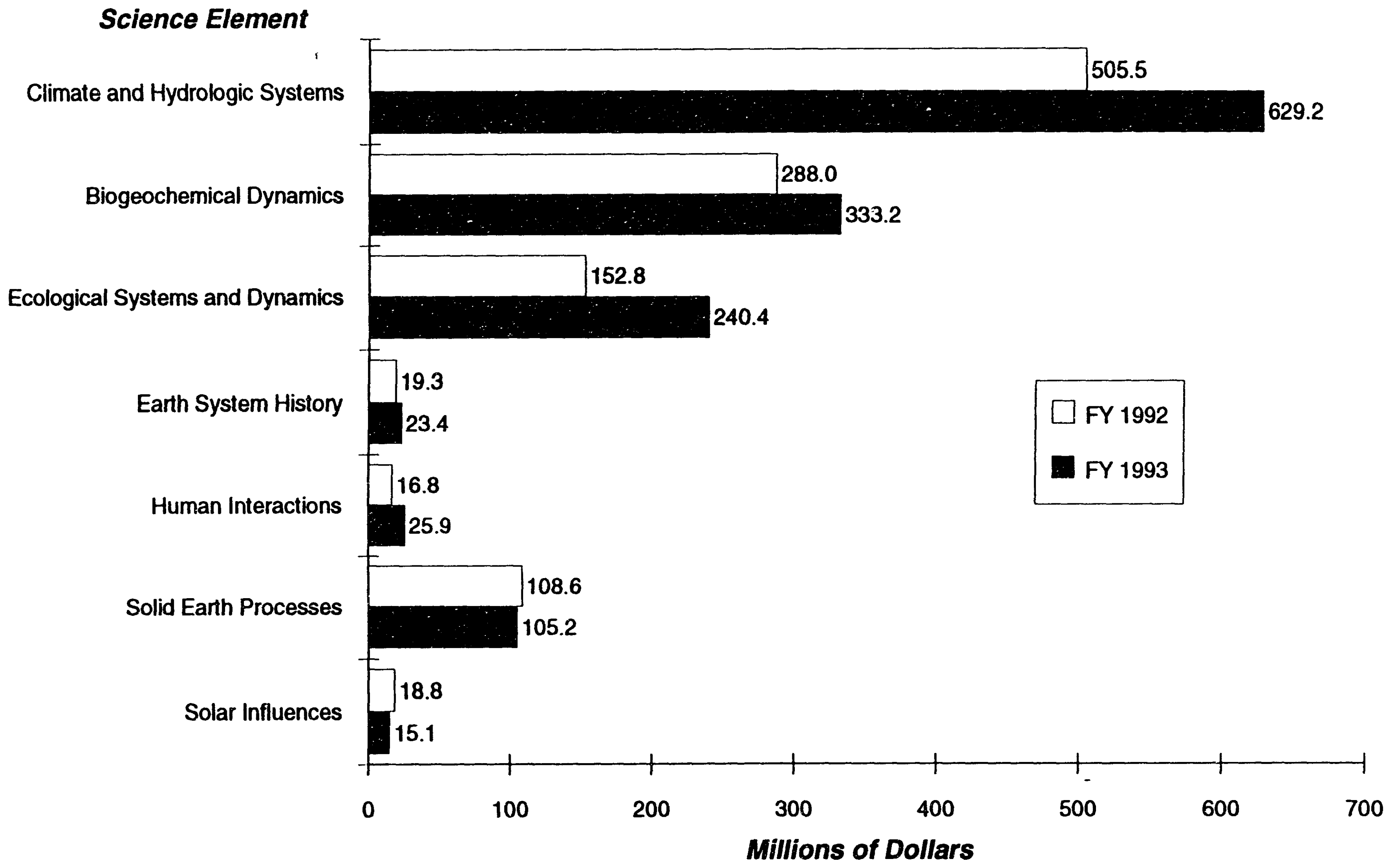

Figure 2. The Cascade of Uncertainty

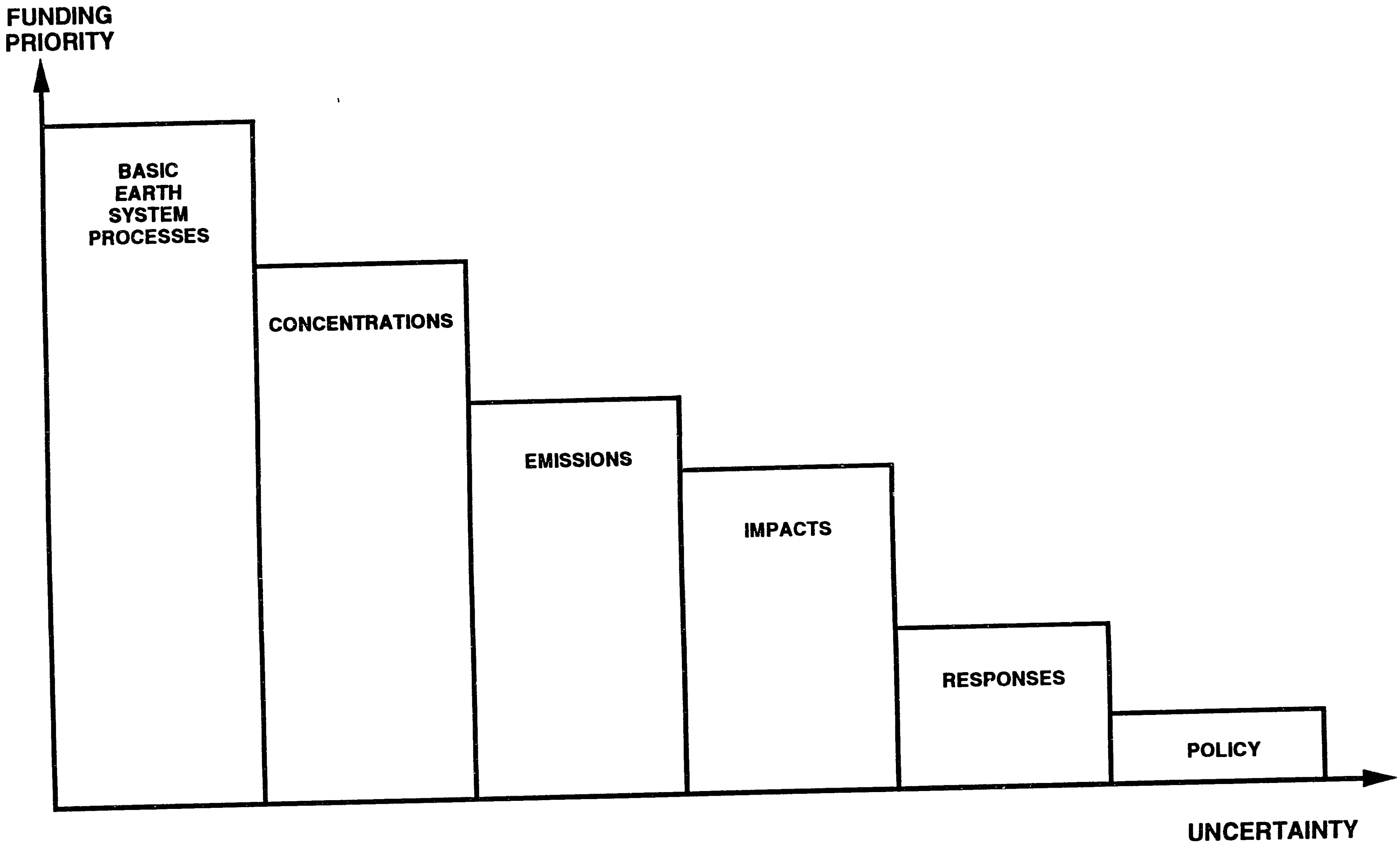

Figure 3. Three Kinds of Science

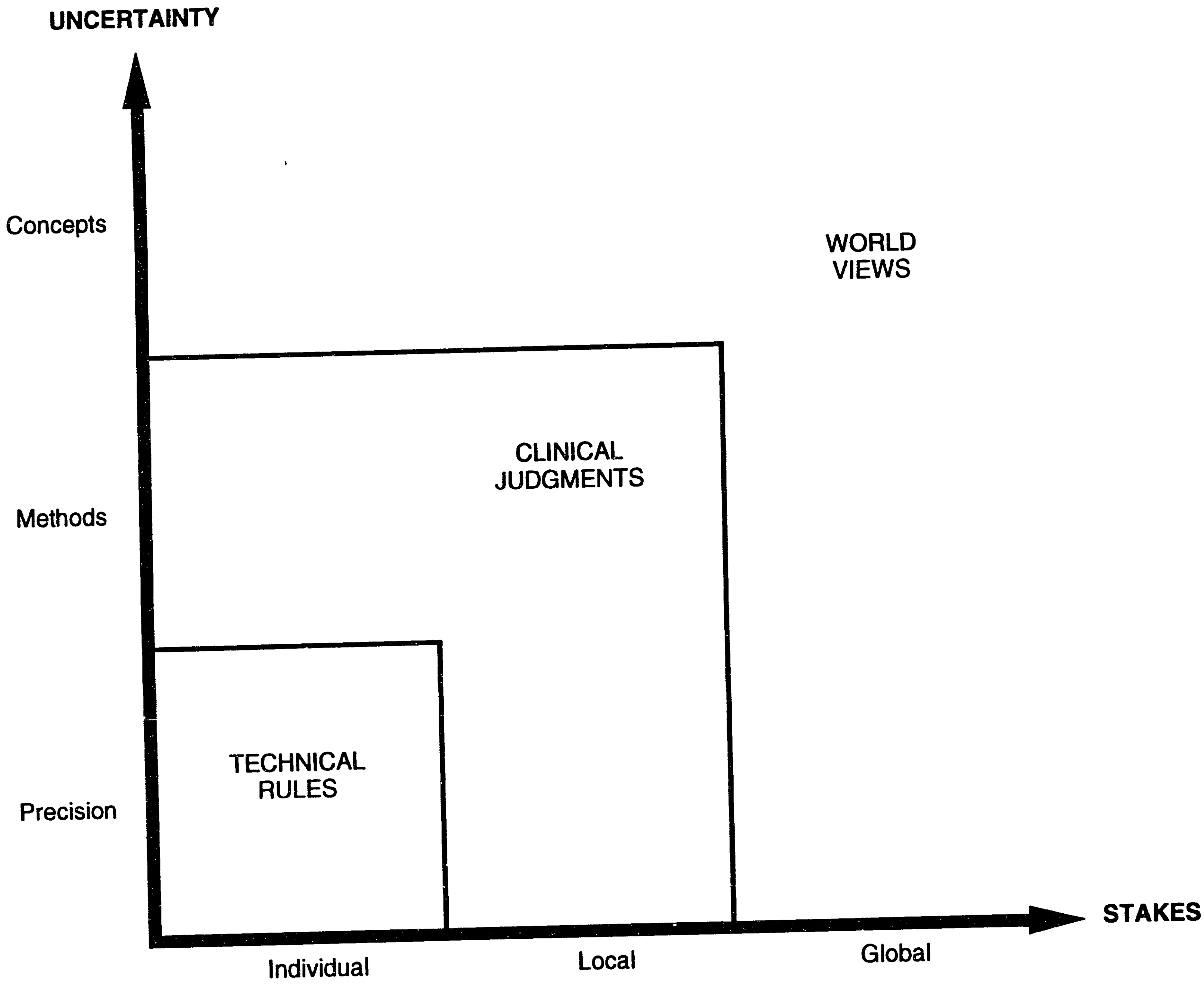

Figure 4. Risk Razor for Delivering Policy-Driven Research Priorities

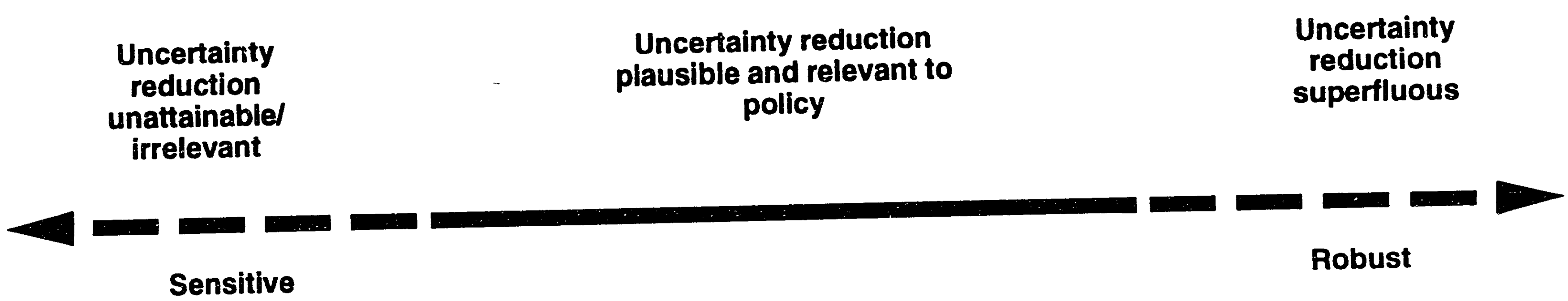

Figure 5. Human Perspectives On Time and Space

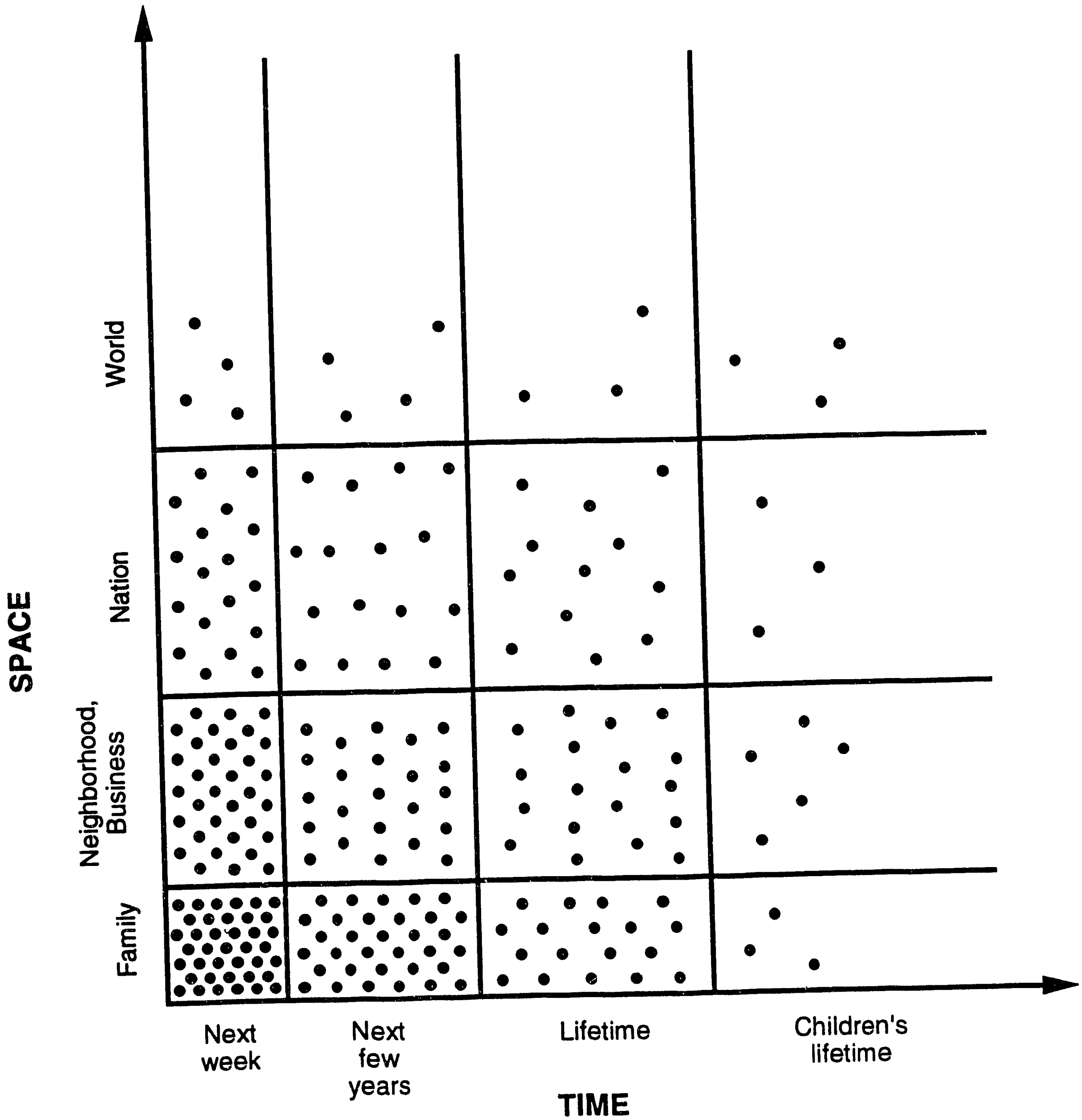

Figure 6. PDIA Modules

Decision Makers	Regions			
	Pacific Northwest	Southeast	Midwest	Etc.
Utility System Planners				
City Managers				
Watershed Managers				
Transportation Infrastructure Planners				
Etc.				

Table 1. Impacts On Resources

Resource	Impacts: Long-Term Change	Impacts: Emergencies
Agriculture	• Crop productivity (increase/decrease) • Salinization • Locations of crop regions	• Increased crop failures and losses • Famine
Water Resources	• Salinization • Management and timing of run-off • Water supply • Flood management	• Drought • Flood • Salinization
Population & Health	• Infectious and parasitic diseases (incidence, range) • Nutrition and sanitation • Air pollution	• Epidemics • Respiratory and cardiac stress • Refugees
Energy	• Hydropower resource changes • Changes in electricity demand	• Supply system failure (e.g., loss of hydropower services, storm damage to supply system)
Forestry, Fisheries, Ecosystems	• Shift in ecological zones • Species composition • Salinization • Productivity change	• Fires • Pests and diseases • Flooding

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