PLANNING FOR THE MANAGEMENT OF TECHNOLOGICAL RISK

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ABSTRACT

This dissertation draws on case studies to describe how top engineers, scientists, and policy makers planned for the federal management of three kinds of hazards: dam failures, structural failures from earthquakes, and radioactivity from nuclear waste. Each case involves artifacts set into an unstable earth, where disasters can occur from a combination of a minor organizational mishap, such as a missing part of a machine, and inadequate knowledge by scientists and engineers.

The planners, working far from the physical and social reality in which hazards arise, operated with mainstream conceptual models of organization, engineering, science, and society as a whole. These general models place technical rationality at the top to achieve predictability and control, and discount the knowledge of all below. But these planners encountered uncertainties in knowledge and surprises in the institutional setting and responded by imposing more stringent controls that may actually have exacerbated risks.

On the other hand, the cases revealed other kinds of knowledge: "intimate knowledge" of particular conditions acquired over time, skillful workmen's "feel" for phenomena that cannot be directly observed, "critical knowledge" of technical matters by those outside specialized fields, and the "aggregate knowledge" of social groups, more comprehensive than the analytical knowledge of science. Competent technical professionals also display such knowledge, which is essential for planning, design, construction, and monitoring of such artifacts but which is suppressed under mainstream models of knowledge and institutional arrangements.

Current remedies do not capture what is needed to cope with these hazards. The dissertation explores kinds of social arrangements within bureaucracy and at local levels outside, which transcend rules and cut across disciplinary and institutional boundaries. It recommends a kind of planning that is informal, flexible, and responsive to diversity and to irreducible uncertainty in the physical and social reality.

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PREFACE AND ACKNOWLEDGEMENTS

I am grateful to Ted Greenwood for his suggestion in 1980 of the title and case studies for this dissertation. Ted had been working at the Office of Science and Technology Policy in the Executive Office of President Jimmy Carter, preparing a policy for the management of nuclear waste. He suggested that this project was often linked with a project on federal dam safety and one on earthquake hazard reduction, and all three became distinguished from other activities of that office under the label of "planning for the management of technological risk."

More than seven years have elapsed as I struggled to make sense of these diverse topics. After being a sociologist and city planner, I focussed my doctoral work in planning and policy studies on the fields of bureaucracy, professionalism, and science and public policy. As I waded through dozens of boxes of files in the National Archives and as many file drawers at the Federal Emergency Management Agency, I became so intrigued with the technical aspects and institutional contexts of each project that I produced four separate papers, each the size of a normal dissertation, one each on dams and nuclear waste, and two on earthquake hazard reduction, one focusing on seismic engineering and the other on the science of seismology.

The theme of knowledge was obviously central, first in the emphasis on "art" in engineering by the outside consultants on dam safety, then in the struggles of seismologists to obtain knowledge sufficiently reliable to make predictions, and finally in the focus on the status of geologic knowledge for mined repositories for nuclear waste. As I read through the nine cartons of Ted's personal files on the nuclear waste project that he kindly lent me, I found adequate documentation of the problems of knowledge from the earth sciences. This became a theme to link all three.

Professor Martin Rein's suggestion that I compare and note changes in texts of documents prepared over time was useful. On dam safety this method revealed the underlying debate on risk analysis, which Frank Perkins, Provost at M.I.T., confirmed as a central issue. He also provided insight into implicit rules underlying many kinds of engineering.

Interviews with the principal staff members on the dam safety and earthquake hazard reduction projects were invaluable. Bruce Tschantz, William Bivens, and Homer B. Willis of the Corps of Army Engineers offered useful insights.

Charles Thiel's richly descriptive stories about the earthquake hazard reduction program and the lessons learned became central for the prescriptive part of this dissertation. His understanding was corroborated by Robert Hamilton and Robert Wesson of the U.S. Geological Survey. My fascination with the outcome of "conspiracies" in planning led me to to see for myself what was happening in

California. To single out Karl Steinbrugge, Richard Andrews, Robert Olsen, Joseph Lang, and James Davis is somewhat arbitrary, and does not diminish my gratitude to a score of others who gave freely of their time and enhanced my understanding.

Frank Press and Philip Smith were helpful. Many at MIT deserve my thanks, including Nafi Toksoz and especially Anton Dainty who critically reviewed a lengthy treatise on seismology, here reduced to less than a dozen pages. I am grateful Gary Marx for encouragement when the tasks seemed overwhelming.

To Donald A. Schon, my faculty supervisor, my debt is unlimited. He patiently read innumerable drafts, including one of almost a thousand pages, and insightfully provoked me with his special genius to articulate underlying themes, and to reduce this dissertation to a more manageable size. Without his enthusiasm and sustained confidence in me, this thesis would never have been completed.

Last but not least, is my deep appreciation for unflagging support from Linda Greene, who patiently typed and retyped the material. Thanks also to Keith (Mrs. Frederick J.) Adams and Dr. James Deeny, whose moral support and encouragement kept me going. All those mentioned must be absolved from responsibility for lack of clarity and errors, which falls on me alone. Even now I feel that the articulation of this thesis has only just begun.

CONTENTS

CHAPTER I

LITERATURE REVIEW

Introduction

> . . . the problem is not new. The government has been struggling with the issues for 150 years [since it] asked . . . the American Philosophical Society to appraise the risks of steam boiler explosions around 1820 . . . a note of optimism [is] related to the fact that we collectively decided to restructure our society and are well along. The 1980's agenda is the rationalization of the process.[1]
>
> Philip M. Smith

This dissertation questions the optimism and expectations of rationalizations in federal efforts to deal with problems of technological risks, specifically those resulting from dam failures, earthquakes, and radioactive wastes. During the period 1978-80, planning for the management of hazards from these sources was carried out at the apex of what could be called national institutions for knowledge and collective action, in the Office of Science and Technology Policy (OSTP) within the Executive Office of the President during the administration of Jimmy Carter.

We contend that the rationalization touted by Smith is in fact part of the problem, not the solution. Our thesis is that generalized models of knowledge and institutionalized actions are inappropriate to deal with these technological risks because they do not address the nature of the physical and social reality. As Smith indicated, federal concern with technological risk was not new. What was new was increasing public concern about the negative effects of technology, arising out of the anti-establishment and environmental movements of the 1960s, and the demands that the federal government do something. The failure of a federal dam, the threat of a great earthquake in California, and public agitation about nuclear waste brought these particular issues onto the federal agenda in 1977.

In each case, the federal government faced a dilemma. For more than a century, it had created public works to control the flow of water over the land and had set an example for others at home and abroad in constructing great dams. It had subsidized public facilities and fostered urban development in areas prone to seismic activity. It had sponsored the concentration of radioactive materials for military ends and promoted their use for civilian needs. It had done these things in the public interest, and still does.

These federal actions were based on the fact that for years federal engineers had insisted that their dams were permanent and would not fail. Earth scientists had promised that they would soon predict great earthquakes and thus save lives. Nuclear scientists and engineers claimed that wastes could be safely disposed of in salt deposits deep in the earth. And many still do. On the other hand, failures, threats, and public agitation challenged the promises of the scientists and the hubris of the engineers and the credibility of a federal government perceived to be ultimately responsible for protecting public health and safety. This created a federal dilemma.

In response, OSTP, among its other activities, undertook three projects to plan for the management of technological risks, on Federal Dam Safety (FDS), for Earthquake Hazard Reduction (EHR), and on a policy for Nuclear Waste (NW). All were carried out under Frank Press, the President's science advisor, and directed by Philip M. Smith, an associate director of OSTP, with the aid of specialized consultants, leading scientists and engineers, and top officials, who are considered the planners in these cases.

To understand why the three projects were grouped together, it is important to recognize the sense in which the hazards of earthquakes are primarily technological. These natural events do not harm people in open areas and seldom damage simple wood frame structures. The dangers lie in the collapse of more elaborate structures and of such complex systems as gas lines and bridges.

To give the reader some background: in response to the failure of Teton dam in Idaho in 1976, OSTP began to prepare federal dam safety guidelines early in 1977 and in 1979 delegated their implementation to an office within the new Federal Emergency Management Agency (FEMA). Later in 1977, OSTP began planning for the use of funds to be allocated by Congress for a new federal role in overseeing programs for earthquake reduction at all levels of government. The next year, after the President did not approve grants for the states, funds went mainly to research in seismology and seismic engineering. In 1980, another new office in FEMA finally did persuade the President to approve a pilot program for Los Angeles. In 1978, OSTP began working with others on the problem of isolating nuclear waste; by the time a policy was announced during Carter's last year in office, Congress was pre-empting the administration's initiative and had subverted its intent.

A decade later, the planning seems to have resulted in little of practical value. Aging dams are seldom inspected and some hazardous ones remain unrepaired. The threat of a great earthquake grows as moderate ones shake Southern California, but the only earthquake predicted so far, in a rural area, will do little to save lives. Meanwhile people live below dams or in unstable structures in seismic areas, often unaware of the dangers, while the agencies that could manage these programs struggle for survival. Old nuclear wastes leach into the ground and new wastes accumulate, while plans for treatment and disposal are deferred.

As technological disasters, such as the explosion of the spaceship Challenger and the fire at Chernobyl, raise public anxieties, they also increase the importance of understanding what went wrong with these federal projects. How can we reduce the likelihood of such disasters, prepare for them, or mitigate their consequences?

Before searching for answers, we review some of the literature that informed our work, especially on technological risks and accidents. The next chapter provides some background on the institutional setting of OSTP, the methodology, and major themes. The body of this dissertation consists of three stories about the OSTP projects; the work concludes with an interpretation of these and a synthesis of lessons learned.

The Literature

Disasters resulting from the use of tools have occurred since the dawn of civilization. But the literature we are about to review is narrower, and may be divided into two basic parts. Traditionally, well-known misfortunes have been treated as matters to be handled in monetary terms by insurance. But more important is a growing body of new literature describing an analytical approach to rare or unprecedented accidents. The question of the acceptability of risks is set apart and thereby generates an expanding list of problems. At the core are problems in analyzing hazards and quantifying the probability of risks accompanied by problems of public perceptions of risks and leading to dilemmas for public decision makers. In using better procedures for public participation in decision making to solve these dilemmas, new global issues arise about threats to the basic institutions of government and markets. The second section of this review covers some fresh perspectives, first on cultural perceptions of risk, next on organizational strategies for handling uncertainty, then on how unprecedented accidents arise, and finally on a kind of knowledge radically different from that of science.

Insurance. An earlier effort to manage risks, in a quantitative and economic manner, can be traced back at least to the Middle Ages. During storms at sea, trading vessels were lightened by heaving cargo overboard, imposing disproportionate losses on some of the merchants. (One can imagine controversies must have arisen about the placement of goods in the hold.) When all of the merchants pooled funds in advance of a voyage to cover the losses expected on the basis of past experience, and entrepreneurs invested in these pools, marine insurance was born. Other forms of insurance followed. For instance, after London's Great Fire of 1666, fire insurance was introduced. In the 20th century, compulsory "social insurance" has been adopted against the risk of unemployment and the loss of income in retirement.[2]

In the insurance industry, the concept of risk was based on several conditions. Negative events must not be subject to human control but rather arise by chance. The risk must be unilateral, not arising out of bilateral marketplace transactions, where one man gains from another's losses (although the insurance industry has gained). Events producing losses must be measurable and quantifiable under theories of probability and chance and the law of large numbers, which states that the greater the number of instances, the closer the results approach the theoretical probability, or a normal frequency distribution. Risk can be calculated as a ratio between the number of units one expects to be harmed and the units exposed to the hazard, both assigned monetary values. For insurance to work, a population at risk must recognize and dread the hazard, and be willing to take responsibility for the economic consequences and pay its share. When the costs to a few are then distributed among all members of a group, insurance is said to substitute certainty for uncertainty in an economic sense.[3]

These conditions, particularly the law of large numbers, do not apply to many modern technological risks. Negative events, such as the failure of a large dam, may be so rare or unprecedented that they cannot be anticipated on the basis of past experience. The boundaries between who is and is not exposed may not be clear, as in the case of the expected losses from exposure to radioactivity, for instance, in the present or in future generations. The value of human life, once a matter for God, or considered cheap, or left up to individuals seeking insurance, is particularly contentious. Moreover, an exposed population may be unwilling to pay insurance costs or to take responsibility for the consequences of a natural disaster, such as a major quake. Corporations may treat risks as economic externalities, not their responsibility, or to be dealt with after the fact by compensating individuals harmed. For such reasons, government has intervened, requiring or underwriting insurance or legally regulating private activities.

More important, when professionals and managers claim to control technology, risks no longer appear to arise accidentally, as matters of chance. Someone is surely to blame. Since the 1960s, a new legal procedure, liability suits, has gained popularity to compensate people after the fact of losses. Insurance rates have soared to cover the prospect of such suits. Moreover, analysts and legislators are constantly trying to find new ways to reduce risks to economic terms.

An Analytical Approach to Risk

Since World War II, a new and highly technical definition of risk has emerged, based on the idea that rare or unprecedented events can be anticipated, their probability calculated, and the level of acceptable risk decided upon and controlled as a matter of public policy. Techniques have been designed to anticipate events differing from those observed or experienced. The Department of Defense and its contractors developed analytical methods, such as operations research, statistical decision theory, and systems analysis, for use in the aerospace and other industries and for planning military strategies.

In the late 1950s the Atomic Energy Commission began to assess risks from the nuclear generation of electric power. In the early 1970s, it elaborately analyzed the possibility of a power plant accident, using decision trees of faults or errors to arrange subsets of events and sequences of processes in scenarios that could lead to failure. By multiplying the low probabilities assigned to significant points of these scenarios, the Rasmussen Report, as it was called, concluded that a major disaster was unlikely.[4] After the report was criticized for limiting assumptions and for neglecting combinations of factors, the techniques were refined.

The difference between old and new ways of knowing about risks, through observation of past events or through logical mathematical constructs, is exemplified by national maps of seismic hazards. Earlier maps were based upon observation, local lore, and history, and presented the highest magnitude of tremors known since European settlement; they did not reflect the greater frequencies of earthquake in the west inactive faults that might soon again trigger quakes. Seismologists have since designed complex algorithms with parameters reflecting what is known theoretically about the causes and transmission of ground motion through crustal materials. Their mathematical models represent the probabilities that certain levels of intensity of tremors will not be exceeded during the next fifty years, as a more reliable basis for seismic standards.[5]

Now few except experts in seismology and statistics can understand or criticize the new maps, which rationalize seismic risk into a hypothetical concept inaccessible to ordinary people. They must accept on faith the numerical probabilities of unprecedented future events.

Problems from Subdividing Risk and Safety. Characterizing risks has become the focus of a new professional field, with a growing body of literature. The professionalization of risk has been justified by the growth in the scale of technology and its interdependence with the environment, which has "dwarfed the ability of individuals to estimate, appraise, and reduce their own risks." Moreover, the public expects scientists to solve the problems that the application of science in technology created in the first place. Thus, we need a rational and centralized determination of safety.[6]

Safety, once a quality taken for granted, has been turned on its head and neatly cleaved into two: "measuring risk, an objective but probabilistic pursuit, and judging the acceptability of risk (judging safety), a matter of personal and social value judgment." The technical definition is further subdivided into a three-part formula: risk equals the statistical probability of the occurrence of a negative event multiplied by the magnitude of its effects.[7] How to put the parts together is an implicit issue in the literature.

Many difficulties arise on the technical side: analyzing, modelling, and quantifying risks. The relationship between cause and effects may not be clear, as when adverse effects of radiation exposure are delayed. Cancers, for instance, may have multiple causes, posing problems of inference. Even after the fact, as we will see in the failure of the Teton Dam, the cause may be unclear. Scientists face difficulties in extrapolating, as from mice in the lab to humans or from given facts to different models, as from the known effects of exposures to sudden high dosages of radiation at Hiroshima to low doses over longer periods of time.

In its present state, the techniques are said to be useful at least for organizing information, shaping alternatives, and surfacing issues for discussion and further research. But the "art of risk analysis is so primitive that in debates, differing analyses can be played off against each other, supporting opinions arrived at by other means."[8] Experts expect the infant field to mature with better science.

However, many problems involving risks have a disturbing uncertainty about them or pose dilemmas, like Hardin's "tragedy of the commons," pitting the individual against social interests. When men are reluctant to make personal sacrifices for the general good if others do not, all may ultimately lose the benefits they shared in common. These have sometimes been called have called "transcientific problems," involving science but beyond its ability to resolve; these are matters for the public and its leaders to handle.[9]

The Problem of Public Perceptions. On the social side of the dichotomy is the judgment of how safe is safe enough, often considered a matter of personal or social values, a moral judgment of the sort that objective scientists should not make. Some experts have tried to pre-empt the public's role. In the 1960s, Chauncy Starr considered the risks of nuclear power plants as a kind of transactional matter, using the method of "revealed preferences," and tried to determine objectively and indirectly what risks people would accept in the marketplace in exchange for the benefits of technology.[10] He failed to recognize that people may voluntarily take higher risks than they will allow others to impose on them.

But research into public perceptions of risk and safety has concluded that the public is irrational, even absurd. Tversky and Kahnemann demonstrated that in matters of chance and probability, most people have little grasp of the rules of inference and instead use simplifying rules of thumb, or "heuristics," to make decisions.[11] Building on such findings, social psychologists have tried to design a theory about the public's "expressed preferences." Systematic sources of error arise from "availability" or "imageability," the ease with which instances of hazards can be brought to mind. The image of "mushroom clouds" may haunt the minds of anti-nuclear activists. Personal experiences play a part; if these are biased, one's perceptions will be too. The press also distorts perceptions of risks from disasters that take many lives at one time, such as airplane crashes, even though aircraft are less hazardous than cars. After a well publicized disaster, like a major quake, the public will clamor for something to be done but that demand will rapidly abate.[12]

People also seek to reduce uncertainty by denying that it exists. The seismic planners' version of "human nature" was that most people live in the "here and how," engrossed in personal day-to-day problems. They are not concerned about the consequences of low probability future disasters. They will tend to ignore or seek disconfirming evidence of scientific warnings, such as of an impending quake. Even experts may have untoward confidence in their own judgments, denying the validity of contrary evidence. "Such over-confidence can keep us from realizing how little we know," social psychologists warn.[13]

This "normalcy bias" has posed problems for disaster mitigation planners, who have concluded that programs "based on individual motivations for self-protection or the initiative of individuals and small groups" have failed because they ignored "the universal human tendency to assume that everything is all right until events clearly prove otherwise."[14] Thus government is justified in preparing programs to protect the public.

The Problem of Public Decision Making. When experts do not agree on causes or models or measurements of risks, and when the public appears irrational, what are public decision makers to do? They are caught in a dilemma of trying to find a rational solution to a public problem while maintaining their own credibility before an electorate that does not know its own mind. The new professionals emphasize that a risk-free world is impossible. Some would convince decision makers that the risks are small, urge them to get on with business as usual, and educate the public to the low probability of "real risks." Most would convince them to allocate funds to assess the risks.[15] But others, in the business of mitigating risks, including some seismologists and seismic engineers, would educate the public about higher levels of risk, to obtain public support for research and their cooperation with official controls.

Many professionals would transform risk and safety into economic terms to make a case against government regulations, or simply because making economic decisions seems easier than making moral ones. "Safety, like anything else can be bought at a price, but then we have less to spend on poverty and disease or things to make life worth living."[16] Others, in a liberal democratic mode, would devise new or better procedures for involving the public early on or more intimately in the decision process. This opens up more global problems.

The Procedural Problem. Dorothy Nelkin and Michael Pollock have analyzed types of procedures here and in Europe to handle public controversies over nuclear power projects, which they consider symbolic or prototypical of conflicts on other forms of technology. They have categorized the procedures in a matrix of four different types based on the general characteristics of being elitist or broadly participatory, advisory or only informational. No one type seems appropriate to all nations and situations.[17]

All types of public participation procedures aim to allow dissenting groups to articulate their views more effectively. This objective is best achieved by five general conditions: the appropriate involvement of all affected parties, a fair distribution of expertise, unbiased management of the procedures, an agenda giving due weight to social and political as well as technical concerns, and a real margin of choice for the participants. However, such debates still do not come to closure but only abate temporarily as public anxieties shift to other manifestations of risk. This conclusion supports the conviction of some that political consensus is impossible. Debates on risk result only in stalemate or authoritarianism and threaten the basic institutions of democracy.[18]

The Institutional Problem. Another theme in the literature on technological risk is the breakdown of an underlying consensus on societal values, such as the need for economic growth and the authority of government. Environmentalists had led the way by adopting a new "paradigm" incommensurable with the old. Those who would protect "spaceship earth" play by different rules from those who see wealth as the name of the game. What is reasonable and rational from one perspective is not so from another.[19] The remedies take two main forms: institutional reform or radical restructuring of society.

Reformers believe that government, though it must make a "mess" of managing risk, is all we have. Corporations externalize risks and ignore side effects while trading short-term rewards for longer term risks that we cannot begin to understand. Government bureaucracies do the same when they optimistically develop technologies, such as nuclear power, in a "hot house," without benefit of common sense or prudent trial and error.

Individually the American People are risk takers but collectively they are risk adverse and woefully ignorant about how technology works. They bump their concerns up to government, calling for new laws and regulations, and then confuse symbols with action, failing to notice that government intervention leads largely to more reams of paperwork. When a technology actually fails, the public calls upon political leaders to produce a scapegoat. The expert-bureaucrat search for acceptable risk becomes a game, as agencies madly search for standards, disburse money, and give the illusion of doing something. The remedy would be to create a new institution specifically for dealing with technological risk.[20]

Some European social philosophers and political economists take a more radical view and contend that the very idea that a capitalist society can manage technological risks is absurd. Most espouse the "critical theory" associated with Jurgen Habermas: twentieth-century society has reorganized its view of itself and its institutions on the model of technical reason. Science and technology have become ideology, legitimating capitalistic exploitation of individuals and society as a whole. Emancipation can only come through a neo-Marxian approach, the application of critical theory, which must unmask the ideology. Society must be radically transformed through free and open communication among its members. All must have equal power to start, to influence, and to criticize the dialogue. Only then will people come to a rational understanding of both the nature of ecological processes and of social reality and totally reconstruct society in the interest of survival.[21]

The radical critique offers insights into the interrelationships between knowledge, human interests, and institutions. In demanding dialogue, it calls for more than procedures that only allow dissenting groups to articulate their views. But it does little to address the problems of technological risk in an immediate or practical way. For fresh insights, we turn to anthropology and organizational theory.

Other Views of Risk and Reality

A Cultural Diagnosis. Mary Douglas, a cultural anthropologist, and Aaron Wildavsky, a political scientist, have devised a theory to explain why environmentalists differ from others in risk perceptions and see technology as a threat. The scientific and technological world is too complex for any of us to know or to cope with all the risks we face now or in the future. Even primitive societies select particular risks to attend to and construct ways to deal with hidden dangers within the cultural framework of how they see the world. In our own society, as well, different groups share beliefs or "cosmologies" that set boundaries on what is normal or moral and not, and that also suggest causal chains from actions to disasters, and establish who is to blame and what should be done.[22]

Science, with its ability through specialized knowledge to measure ever smaller things, has actually expanded the universe about which we cannot speak with confidence. Scientists now disagree on whether there are problems, what solutions to propose, and if interventions will make things better or worse. They tend to label whatever is not amenable to technical solutions as institutional or political problems. Risk assessment itself is biased, underlain by assumptions about the way the world is. The scientists' claim of looking at the "is" of a problem before rationally devising the "ought" ignores the prior editing of risks and the taken-for-granted moral way people view the world. Specialized risk assessments impoverish statements of human problems by removing risks from contexts, objectifying or "desocializing" them.

Our society can be divided into many "political cultures," each with its own "cosmology" through which individuals and social units identify particular risks in particular social contexts and have devised social institutions for managing them. "The social units that do the risk handling come in a variety of forms -- bounded groups, hierarchical organizations, competing personal networks..., atomized communities -- and they run the entire gamut from vast federal agencies to tiny self-help arrangements organized by nothing more formal than a shared sense of neighborliness."

Five general types of individuals can be characterized on a social map, at four corners and the center of a matrix of "groups" and "grids" or hierarchies. These range from tightly bounded groups to loose networks and from rigid hierarchical to egalitarian social settings; each has a special sense of time. "Sect members," typified by environmentalists, form tight groups outside of and attacking hierarchies. In the short term they only want to survive the pervasive hazards created by dominant institutions; in the long term they hope to find redemption and inherit the earth. "Castists," such as many federal officials, operate in tightly bounded groups isolated from day-to-day concerns within rigid bureaucratic hierarchies. These give them cautious optimism that the stable, complex collectivity will control risks in the long term.

At the other extreme are "entrepreneurs," working through loose networks in more egalitarian settings and disdaining hierarchy for the management of short-term risks, which they welcome as opportunities. They emphasize personal skills and judgment and tend to be expansive optimists, expecting "business as usual" in a discounted future, like a fourth group in the middle, the "hermits," who are independent of both hierarchies and groups. Some of our outside consultants may exemplify this type. At the bottom of hierarchies with little group support, the disadvantaged and powerless live from day to day and accept and absorbing risks as facts of life.

These authors do not deny that many risks are real, spilling over from one technology to another and from the physical to the social world. In their concern with what the lack of consensus on socially constructed risks may do to basic social institutions, their intent is to create a theory that policy makers can use in accommodating differences.

This diagnosis points up that institutions come in many forms, with informal norms and taken-for-granted customs as well as formal rules and roles. It directs attention to outsiders and those at the fringe and the bottom and tends to equalize their cognitive claims with those of technical experts, both embedded in moral views of the world but played out in different social settings. It is good description and diagnosis but fails to prescribe what can be done outside the domain of formal decision making. In order to find clues to remedies, we must look more closely at organizations.

Organizing around Uncertainty. The social psychologist Karl Weick does not discuss technological risks and accidents directly but writes of processes of organizing to handle "equivocality," ambiguous signals and novel situations Uncertainties trigger social interactions within groups; they organize in order to select interpretations or causal maps and recipes for handling these. Such maps, much like Wildavsky's cosmologies, and recipes, like institutionally given ways of handling risks, are usually drawn from those retained in organizational memory.[23]

People interact to make sense of the plethora of stimuli and the flow of experience in everyday life. Their experiences are bracketed or bounded, and parts edited out or rejected, to fit common conceptual models. When the appropriate behavior or response is not immediately clear, when conflicts and controversy are rife, people fall back on diagnoses that worked in the past, selecting models of situations and recipes or remedies most readily available in the institutional setting, as was the remedy of new regulations for dam safety.

Reality is always constructed after the fact, in Weick's view. We know what we think after we hear or see what we say or do. Thus retained causal maps, selected interpretations, and organizational interactions are closely coupled. This view is captured by Clifford Geertz: "Man is an animal suspended in webs of significance that he himself has spun," and in the metaphor that organizations paint their own scenery, observe it through binoculars, and then try to find paths through it.[24] But how should we construct the reality of technological failures?

Normal Accidents. Charles Perrow, an organizational theorist, offers new maps to explain how accidents occur. He starts with fine-scale descriptions of accidents in the past and works forward to describe the characteristics of systems that make accidents so likely that they must be considered normal. He would classify all human systems, both social and physical, in a matrix with two dimensions: the extent to which system components are loosely or tightly coupled and the extent to which the components interact in simple and linear or more complex ways. A nuclear power plant, such as Three Mile Island, is a tightly coupled, complex system, in which components may have multiple functions or depend closely upon one another and interact often invisibly in unforeseen ways. The failure of two or more tightly coupled components, although infrequent, can proliferate rapidly to a total system collapse.[25]

In a loosely coupled complex system, such as a university, a combination of unexpected, untoward events can usually be contained without a system failure. In a loosely coupled linear system, like an assembly process in manufacturing, the worst that may happen is that parts will back up or form queues. Perrow placed dams in the remaining category of tightly coupled linear systems, largely based on the accident at Teton (we will disagree with this classification), in order to complete his model:

> But by and large, dam failures appear to be due to rather prosaic matters, in particular, ineptitude and deliberate risk taking. It was important for us to consider dam accidents because we needed an example of tight coupling without interactive complexity.[26]

Perrow identifies six types of components of systems: design, equipment, procedures, operators, supplies and materials, and lastly the environment. Too many accidents are blamed on "operator error," overlooking other groups such as management and designers. Frequently, as in mine accidents, operators as "first party victims" are blamed. Normal accidents can affect other types of victims; second party individuals without influence in the system, such as users and suppliers, innocent third party bystanders, and fourth, future generations, as in accidents involving radiation or biogenetic research.

In complex, tightly coupled systems, designers often believe that they can reduce the likelihood of accidents by incorporating greater redundancy or more automation. But each additional unit in such a system may only increase the complexity; the risk may rise exponentially with the greater number of potential interactions. With automation, operator confusion may increase in situations not covered by orders from above or operating manuals, compounding the problem. Even experienced people tend to make de minimus assumptions and deny that the worst is happening. They may construct the safest model of reality, one perfectly reasonable based on past experience. This explains why seasoned ship captains have misread lights ahead in the dark and steered into oncoming vessels.

On the other hand, operators can contribute to a recovery from the errors of others or from unexpected environmental conditions, as the Apollo space crew did after it directly experienced the jolt of an explosion and saw a vapor trail. Ground based managers and designers, confident in safety devices and redundancy, misinterpreted instrument data and searched for small explanations, determined to maintain operations. Perrow agonized with others over the dilemma of greater or lesser operator control.

Perrow concluded that some systems should be simplified by decoupling components. He feels that others, like dams, are essential, cannot be simplified, and should be allowed to continue but under tighter safety regulations. A technology that could fail with irreversible long-range consequences but is not yet in use, or for which substitutes are available, as with nuclear power, should be abandoned or phased out.

Perrow also contributes an interesting new concept of decision making, that of "social rationality." This is distinct from two familiar forms: economic or absolute rationality, which is narrow, quantitative, and precise, and bounded rationality, which emphasizes limits in our thinking capacities and in our ability to achieve absolute rationality. Bounded rationalists despair of the public's ability to make sound choices on risks. On the other hand, social rationalists consider that cognitive limits have positive consequences. They emphasize the diversity in cognitive abilities, such as in counting, verbalization, or visualization. This diversity brings people with different types of skills together to address complex problems and leads to interdependence and social bonding.[27]

Moreover, cognitive differences promote new perspectives and solutions that no one person will have. Indeed, even those who seem irrational about new technologies may have something to contribute: their feeling of "dread" about unprecedented disasters, based on a broad understanding of the context in which lesser accidents arose in the past.

Like Clifford Geertz's "thick description,"[28] social rationality recognizes cultural values and subjective dimensions of reality and accepts scepticism about man-made systems and institutions. Given the tentative, ambiguous nature of experience and the unanticipatable, unrecognizable interactions from which failures occur, social rationality may be vital. But what is this subjective dimension of human understanding? A clue comes from one small source.

Bottom-up knowledge in organizations, as described by Ralph Hummel, seems linked to the collective feeling of public "dread." Both are ways of knowing about what is unique or unprecedented in experience and both differ radically from what is expected of scientific knowledge. Much like the "feel for the hole" that we discover among grouters at Teton, Hummel identified a special kind of knowledge of phenomena among craftsmen in their mundane understanding of their tools and materials.[29]

People who work directly with their hands on materials may acquire a "feel for" the object of their work, coming to know it "in its own terms." To a sensitive workman who lacks preconceptions of a priori notions, the object "shows itself" and imparts an understanding of its particular nature. The worker comes to know or apprehend its unique qualities and learns "how it wants to be handled." Workers attuned to qualitative phenomena that seem to emanate from an object thereby overcome the object/subject dichotomy typical of the scientific attitude.

Underlying this kind of knowledge is an assumption that reality is more than what is known through analysis and in relationships based on intentions to control objects. This richer view of reality opens up opportunities for improving the quality of work in organizations. It involves attention to more than blueprints or plans or numbers. Instead it requires workmen to understand the context of their work and the larger purposes of the organization and to synthesize such understandings with their feel for and receptiveness to the back talk of objects and the opportunities these present.

Conclusions. This chapter has taken us from an earlier approach to risk, as a response to past experience, to an analytical approach to unprecedented future hazards. Technical professionals break complex matters into simpler parts, after sloughing off what is not amenable to analysis and quantification and relegating such matters to centralized political control, leaving the public to accept experts' conclusions on faith.

On the other hand, if accidents arise from the close coupling of components in poorly understood complex systems, physical or social, public feelings of dread may be rational. Something more than the analytical knowledge of science is needed. This literature also suggests two very different views of physical and social reality. On one hand is what can be called "technical rationality," and on the other hand, one we will call "social rationality." Each is dependent on different ontological and epistemological assumptions. We may characterize or caricature the two views as follows:

Technical rationality assumes a reality apart from people, subject to eternal, universal physical laws. These are understood by deducing hypotheses from general theories and testing these with analytical methods, yielding certain knowledge for prediction and control.

An alternate view is that humans cannot know all of the qualitative richness of reality, except perhaps provisionally in direct interactions at the smallest scale. At a larger scale, they perceive phenomena differently from place to place and over time. The best they may do is to arrive at tentative understandings negotiated in social groups in particular social settings. The opposition and tension between these two views of reality permeate our case studies. But first to more substantive matters.

NOTES

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17. Dorothy Nelkin and Michael Pollock, "Problems and Procedures in the Regulation of Technological Risk." in Richard C. Schwing and Walter A. Albers, Jr., eds. Societal Risk Assessment: How safe is safe enough? (New York: Plenum Press, 1980), 233-247.

18. Eugene B. Skolnikoff, "Implementation: Action or Stalemate?" unpublished paper presented to Conference on Public Policy Issues In Nuclear Waste Management, October 29, 1976.

19. Steven Cotgrove, "Risk, Value Conflict and Political Legitimacy," in Richard F. Griffiths.

20. Joseph F. Coates, " Why Government Must Make a Mess of Technological Risk Management" in Christoph Hohannemser and Jeanne X. Kasperson, eds. Risk in the Technological Society. (New York: Praeger, 1982), 21-34.

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24. Ibid. 140.

25. Charles Perrow, Normal Accidents: Living with High-Risk Technologies, (New York: Basic Books, 1984), passim.

26. Ibid, 354.

27. Ibid, 321-328.

28. Clifford Geertz, An Interpretation of Culture (New York: Basic Books, 1973), 3-32.

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CHAPTER II

BACKGROUND

This chapter briefly describes the institutional setting of OSTP and each of the three projects. It then considers ways that these could have been interpreted and treated by quantitative analysis or under various theories of the social sciences. It next describes the methodology used in preparing this dissertation and concludes by describing how major themes emerged to shape the thesis and intertwined in the stories.

Institutional Setting

Although OSTP was only created by Congress in 1976 under P.L. 94-282, the National Science and Technology Policy Act, scientific and technical advice in the executive office of the President was hardly new. Scientists had been valued in the White House during and after World War II when Vannevar Bush declared that science was America's new frontier.[1]

Support for basic or theoretical research in the physical sciences soon became an accepted federal function, necessary since it was beyond the capability of private organizations and was good for the nation as a whole. The mainstream belief is that theoretical knowledge must be advanced to replenish a pool of knowledge from which general principles would flow down like water into new technology for the domestic public welfare and to strengthen the nation's competitive position abroad, economically and militarily.

The surprise of the Russian spacecraft launching led President Eisenhower to choose a special science advisor; so did every elected president thereafter. An Office of Science and Technology was established by executive order in 1962. President Nixon, however, transferred its functions to the National Science Foundation (NSF) in 1971, after policy conflicts with his advisors. The technical community quickly turned to Congress and with support from the office of President Ford, succeed in obtaining legislation to institutionalize this function. A skeleton staff from NSF served Ford and included Smith, who alone would remain under Frank Press, President Carter's Science Advisor, to work with the new and technically sophisticated Carter.

Segments of the technical community have argued that the top government scientist should have cabinet status or a department or at least an agency of his own with the authority and budget to operate programs.[2] On the other hand, the science advisors have had direct access to the President and to the Office of Management and Budget, where Smith had worked for a time before moving to NSF. The science advisor's influence inside the White House has depended largely on his personal relations with the president and his staff and on his powers of persuasion; OSTP adequately demonstrated such powers and also the ability to use the clout of the executive office from time to time to bring the agencies into line. Under Press, OSTP did not lack adequate power.[3]

One difficulty in the formal institutional setting, obvious from our cases, was that divisions among federal branches and state governments, set up as checks and balances against overcentralization of power, often meant that responsibility was divided and unclear for protecting citizens against hazards. Congress has ultimate authority for making policy on matters of deep national concern, as it would do on nuclear waste, and intended to do about earthquake hazards until the president overrode its intent. The states have limited control against hazards imposed by the government itself, of federal dams or nuclear waste disposal. Thus in the larger institutional framework, as in the bureaucratic arrangements for dams, authority is divided and the locus of responsibility often moot, providing one sense in which institutional arrangements are inappropriate. To orient the reader, we now summarize the three cases, each initially treated by OSTP as a separate and independent problem.

The Federal Dam Safety Program (FDS) After a dam newly built by the Bureau of Reclamation collapsed in 1976, Congress urged the executive branch to improve its decisionmaking procedures on dams. OSTP was asked to survey the safety practices of more than a dozen agencies dealing with dams and to direct the preparation of guidelines for federal dam safety, with advice from a panel of outside experts. Three major problems were identified: there was no communication among the agencies, no one knew all about nor was in charge of each large project, and agency officials were reluctant even to talk about risk and failure.

OSTP carried out its tasks and saw to it that a new lead agency was established in the Federal Emergency Management Agency (FEMA), to coordinate new dam safety offices set up at the top of each agency to implement administrative guidelines. Almost immediately, difficulties were reported, such as limited staff, funds, or legislative authority. Over the years the Bureau of Reclamation did not appreciably change its practices and other agencies did little but extra paperwork.

The program at least survived a new administration and has continued to work with top officials at a reduced pace, largely to define consistent technical standards for dam engineering. It also tried to build a constituency for dam safety among the states, which are responsible for about 95% of the nation's large dams, but their interest waxes and wanes largely in reaction to periodic failures. Still, this may have been OSTP's most successful planning project.

The Earthquake Hazard Reduction Program (EHR) After years of conflict, scientists and engineers joined to persuade Congress to liberally fund a "whole" national program for EHR, both for building the scientific capability for prediction and for engineering and planning to reduce hazards said to threaten at least 39 states. Under Press, a leading seismologist, OSTP was given the task of planning for the use of the funds.

When planners initially produced only a long list of familiar issues, calling mainly for adding seismic provisions to existing programs, OSTP then pushed through a very general plan, centering around a program of planning grants to states. The Office of Management and Budget (OMB), fearful of setting a precedent for funding states to prepare against other natural hazards, persuaded Carter to veto the program. The funds appropriated by Congress for a whole program would be used largely for scientific and engineering research.

OSTP eventually set up a lead agency, again in FEMA, mainly to work on national codes and standards and to urge federal agencies to retrofit their buildings, but with little success. After Mount St. Helens erupted in 1980, Press did convince the president to re-examine the threat, primarily to defense installations. Finally, Carter reluctantly approved a small federal-state program for EHR in the Los Angeles region. FEMA has since prepared a logistical plan for "command and control" of an urban area after a quake, one that many in California deem inappropriate to unpredictable situations. In the

Reagan administration, the lead agency operated for many years without a director and barely survived, with little control over the Los Angeles program, which has in fact become the center of a successful movement of preparation in that state.

The seismologists continue with basic research and hope to make seismology a Big Science. A National Earthquake Prediction Evaluation Council (NEPEC), set up to certify the validity of prediction research, has issued only one prediction and for a rural area. Many believe that the prospects for a socially useful prediction are dimmer now than a decade ago.

<u>The Nuclear Waste Policy</u> (NW) As public agitation complicated administrative policy making in other areas, President Carter established an Interagency Review Group (IRG), chaired by the new Department of Energy (DOE) as lead agency, to formulate a policy and program for safely isolating all forms of nuclear waste.

OSTP played a central role, analyzing alternative technical strategies and problems of knowledge of disposal in mined repositories. It advocated a cautious "stepwise" process of extensive scientific research on multiple sites in many geologic media, followed by public review and licensing before the first repository, primarily for spent reactor fuel, was constructed. DOE preferred a more rapid program including a small licensed experiment in burying spent fuel with military wastes in New Mexico, an ongoing project that OSTP generally opposed. As time passed, issues continued to surface and consensus dwindled within the IRG.

By the time the president was persuaded to support the more conservative approach, DOE had backed off and obtained help from Congress to place military waste at the New Mexico project without licensing, a project that Carter had no power to block. His policy statement at the end of his term had little public or programmatic impact. Congress later incorporated the site selection procedures into legislation but under a rigorous timetable, negating OSTP's intent. President Reagan and Congress have cut back on the search for sites, reducing the limited victory for the policy.

In December 1987, as this dissertation was being submitted, water was flowing into the New Mexico repository, probably making it useless. Congress had also abandoned its procedures for studying many sites and had effectively chosen only one at which to bury spent fuel in Nevada.[4] In essence this, the most elaborate of OSTP's three projects, which might have been judged a limited success, has now been totally undone. We now address the question of how to treat all three cases together.

Interpretations

How should these programs be evaluated as a group when each is so unique and assessment of the outcomes varies with time and with the perspective taken and even now is changing? Chapter I mentioned some reasons why the outcomes seem disappointing. On the positive side, the planners could be said to have responded adequately to the definitions of the problems and their mandates as given, but they encountered surprises and unexpected obstacles, such as presidential disapproval of state planning grants or DOE's end run to Congress. But there were some accomplishments. For example, engineers now admit that dams do fail, some old nuclear waste has been stabilized, and the program in Los Angeles is successful. But these seem minor or far less than expected, or were accomplished in spite of federal efforts.

Some might also claim that the risks are quantifiably lower than they would have been without OSTP's intervention. To argue that case would entail estimating the levels of risks before 1977, projecting them, comparing these hypothetical values to present levels, and attributing some positive difference to OSTP, a highly speculative venture.

The present approach is that of the more descriptive social sciences, but what field can supply an adequately comprehensive framework for analysis? Organizational theory, for example, provides ample literature on the problems of bureaucracy to account for difficulties in FDS and particularly for the subsequent inertia in implementation, but has little bearing on what happened in EHR.[5] The concept of political games might be used to explain what went on in making policy for NW, but is less applicable to the other two programs. Other concepts from political science might be useful, as in the literature on problems of implementation. For instance, a permanent "fixer" in the Executive Office might have made the NW policy work.[6] There may be a simpler explanation: a new political party came to power and the political agenda changed; the planners cannot be blamed for events beyond their control. But failures, threats of disasters, and public concerns about risks still haunt elected officials.

Indeed, using several mainstream views in the social sciences, one could plausibly and partially explain why these programs were not more effective, or even argue that they succeeded. The conclusions would depend largely on the frame of reference and time, on what is chosen as relevant evidence, and how it is marshalled in argument. But our intent is more practical than simply to support, refine, or rebut a particular theory.

Methodology

Our approach does not derive from a single theory but is more empirical and somewhat interdisciplinary. To a case study approach was allied a method akin to hermeneutics. This involved close study of fine details of texts and then shifting attention to larger contexts and then back to details, repeatedly, in an effort to organize and make sense of the parts in relation to the whole and to grasp the whole by apprehending the configuration of parts. Closure comes with the convergence of understanding. This method required attention especially to changes in key documents and omissions in subsequent versions, as clues to underlying conflicts that were suppressed. The remarks of the main participants were analyzed closely for an understanding of their particular perspectives. Making sense of anomalies necessitated tracing concepts back in time and exploring tangential areas.

The initial sources of data were public documents produced by these projects, supplemented by earlier drafts, memos, correspondence, and other material on file in lead agencies or the National Archives. An invaluable part of the process was the series of interviews with the OSTP consultants and other participants, particularly in the first two cases. The consultant on nuclear waste policy made available his complete files in nine large cartons. This research was supplemented by a field trip to California to directly observe the outcome of the EHR program. A review of materials put out for the general public, such as Science magazine or conference reports of technical societies, offered the basis for understanding technical matters, verified through discussions with specialized professionals.

Every social scientist is obliged to make explicit his personal views and biases to the extent he is aware of them. I confess to an initial distaste for the hubris of many scientists and engineers, a prejudice rapidly dissipated by the honest humility of many of my informants, who openly shared a critical understanding of their fields. A second bias, if so it be, is distaste for those who belittle the knowledge of ordinary people, all the more troubling in a democracy. This research has only reinforced my feelings on that. But such feelings were not adequate to structure the research.

Common Themes

A persistent question in the early research was what is common to all three projects? One answer was clear: all are about artifacts in unique sites in geologic settings and closely interacting over time with a none-too-stable earth. This distinguished them from technological artifacts such as nuclear power or chemical plants, where failures usually arise from events independent of their sites. It also sets them apart from totally mobile objects, such as space crafts, where other types of environmental factors contribute to failures. This fact makes issues of knowledge in the earth sciences, particularly for engineering, germane to all three cases. But institutional issues were also a common thread; so obvious as to be easily overlooked is that all three projects were carried out at the top of the federal bureaucracy and involved a number of different agencies. For such reasons, we began to look at models of knowledge and at institutional actions. The three major themes of the dissertation become the nature of science and engineering in relation to one another and to the federal bureaucracy.

It became apparent that "generalized models of knowledge" lay behind the work of scientists, engineers, and bureaucrats -- knowledge derived through the methods of science and refined analytical techniques, knowledge embedded in abstract theories and general principles and expected to flow down into engineering and technology to enable men to overcome uncertainty and to predict and control the physical and social world.

Also common were institutionalized actions not only in the structure of the federal system, but even more in rules and procedures for applying generalized knowledge: in general guidelines and technical standards, as formal criteria for decision making, in the processes of policy analysis and planning. These institutionalized aspects were all formulated at the top and imposed down through the hierarchical organizations of government and for science and engineering.

In contrast to these common general aspects, two rather anomalous points stood out, not common to all three programs but unique, one emphasized in OSTP's program on FDS and the other by planners for EHR, and both largely overlooked, ignored, or suppressed in the other federal programs. Neither was clearly recognized by the planners in NW. The first applies to the nature of engineering knowledge, the second to the role of individuals and groups in decisions or actions necessary to deal with technological risks.

In FDS, the outside consultant and panel of experts repeatedly stated that in practice dam engineering is not simply the application of scientific principles. Practitioners must contend with irreducible uncertainty in understanding sites and material conditions and must recognize their limited control over construction processes and the destructive forces of man and nature. Designers must accommodate and synthesize many non-technical considerations into their designs. For such reasons, dam engineering is more like an art.

In EHR, the planners emphasized that at virtually every level of society, individuals and groups make decisions that affect environmental safety. Therefore responsibility for mitigating earthquake hazards must be <u>shared</u> throughout the public and private sectors.

These principles, that engineering relies on more than science and in practice is like an art and that responsibility for safety must be shared, seem to come from a model of the physical and social reality other than the model of technical rationality described in Chapter I. In that mainstream view, engineering is simply the application of scientific principles and those at the top assume full responsibility and are ultimately held accountable.

But the outside dam engineers failed to completely understand artistry; they attributed it almost exclusively to themselves, inadequately recognizing the knowledge and skills that we will discover in construction, such as the grouter's mysterious "feel" for unobservable conditions below the surface into which he injects material. The EHR planners largely ignored the artistry, for instance, of local building officials coping with seismic risk in difficult social settings and the ability of ordinary people to understand earthquake hazards and be willing to take precautionary actions. Instead, particularly in EHR, the federal government fell back on a bureaucratic model of command and control to protect the public from harm.

In NW, the policy called for the federal government to assume full responsibility for isolating nuclear waste, ignoring the shared responsibility for its generation. The public would be allowed to express its views primarily through licensing procedures. These planners also sought better scientific knowledge and elaborate procedures to inform site selection and unprecedented engineering, under the model of technical rationality.

In contrast, what is happening about nuclear waste illustrates our thesis. The unexpected water at the New Mexico site reflects the inadequate knowledge of scientists and engineers and may well combine with DOE's poor management, particularly in its relations with New Mexicans, to halt that project. On the other hand, although Congressional representatives are accused of politics, they seem to have exercised sound judgment in choosing a site for spent fuel in Nevada. As with engineering artistry in design, this choice reflects an understanding of the social and physical reality and a synthesis of technical and non-technical factors which make this site preferrable to contending ones in Texas and at Hanford, Washington. Like Hanford, it offers the political advantage of state tolerant of nuclear activities, the demographic and physical advantages of being in an unpopulated area already contaminated by nearby nuclear testing, and the legal one of federal ownership of the land. Economically, it is the cheapest of the three to mine. Technically, the site is in a dry area above the water table and unlikely to contaminate the water supply, as feared on the basis of extensive studies of geohydrology at the other two. Although not a perfect choice, it reflects a kind of shared responsibility and "social rationality" or artistry in decision making.

Why did the planners ignore the principles of artistry in engineering and shared responsibility for safety? The answer seems to lie in part in the institutional setting. The planners took for granted mainstream views of knowledge and of appropriate forms of action embedded in the dominant institutions of science, engineering, and the federal bureaucracy. In interacting at the apex of these, the planners simply did not see what goes on far below or far away from the center at the periphery or outside of institutional boundaries, in diverse and ever changing local realities. The thesis arising from these observations is that generalized models of knowledge and institutionalized actions are inappropriate to deal with technological risks because they inadequately reflect the social and physical reality.

To give a solid foundation for this generalization, we first describe the cases narratively, as stories. These ostensibly describe interactions among top officials and leading scientists and engineers and interweave the themes of bureaucracy, science, and engineering. Engineers dominate the first story with outsiders pitted against bureaucrats. In the story on EHR, outside engineers are pitted against scientists, who are insiders in the sense that they depend on federal support. In the final case, bureaucratic officials and engineers are pitted against scientists as policy makers.

Minor stories are linked to the major ones. One strong central in FDS is about the evolution of Teton dam. Others tell about inspection and construction, revealing alternative kinds of knowledge. EHR takes us back in time to the evolution of knowledge and institutional arrangements of seismic engineering and seismology. It also describes seismic code enforcement in a local social reality similar to the physical one at Teton. The tale of the subsequent program in FEMA reveals effective planning in bureaucracy. Several minor stories complete this one, about recent earthquake preparation and programs to enable predictions in California. Finally there is a story from China, where the preparation and prediction were successfully combined. The story on NW is supplemented by a sketch of earlier federal efforts and ends with a description of a local movement, again revealing kinds of knowledge and actions that helped upset the federal policy.

A final comment: these stories are not "whodonits" revealing a villain at the end to be blamed for the crimes; little is achieved by placing blame. These are more like classic tragedies in which the heroes themselves become victims, not of blind chance, but of their own blindness. Indeed our lack of awareness of institutional arrangements and of limited knowledge make us all potential victims of technological risks.

NOTES

1. See James Everett Katz, Presidential Politics and Science Policy, (New York: Praeger, 1978), for a full account of the history of scientific advice to the President.

2. The case is made by William H. Shapley in "Executive Leadership for Science and Technology", in William T. Golden, Science Advice to the President, (New York: Pergamon, 1980), 165-174.

3. Frank Press, "Science and Technology in the White House, 1977-1980," Science 211 (9 and 16 January 1981): 139-145 and 249-256.

4. Susan L. Ravsky, "Accord is Reached on Nuclear Waste Dump," New York Times, December 18, 1978, 34.

5. Nicos P. Mouzelis, Organization and Bureaucracy: An Analysis of Modern Theories, (Chicago: Aldine, 1978).

6. See for instance, Eugene Bardach, The Implementation Game: What Happens After a Bill Becomes Law, (Cambridge: MIT Press, 1977.)

7. Isaac Winograd, 'Padwaste Disposal in a Thick Unsaturated Zone." Science 212 (26 June 1981): 1457-64.

CHAPTER III

FEDERAL DAM SAFETY

What follows is a story in several parts. It begins with a history of Teton, the dam that failed, followed by investigations into that failure. The central part is the response in the Office of Science and Technology Policy (OSTP), followed by a description of a continuing program in the Federal Emergency Management Administration (FEMA). It then describes the National Dam Inspection Program, revealing a useful but untapped kind of knowledge. A close look at construction of Teton reveals three more useful kinds of knowledge.

The Story of Teton Dam

At 11:57 on the morning of June 5, 1975 an earthfilled dam over 300 feet high, nearly completed by the Bureau of Reclamation on the Teton River above Idaho Falls in southeast Idaho, abruptly collapsed. The water in an almost filled reservoir spewed fourth, killing 11 people, damaging over 3,000 homes, destroying 16,000 head of cattle, inundating 100,000 acres of farmland, and causing property damage estimated at more than one billion dollars.[1]

This event marked the first time that a large dam constructed by a major federal agency had totally failed. It shook the Federal establishment and the nation. It also abruptly ended a project that had been evolving for over seventy years.

Although Teton is a "worst case," the process by which it evolved may be typical of many public works or even large private projects. The first part of the story has been pieced together from statements scattered through sections of reports on subsequent investigations that seem to have been of minor interest to engineers. The latter part is much as the Bureau might have told it, had everything worked out as planned.[2]

The idea of a dam on the Teton River was first considered in 1904, only two years after Congress had created the Bureau. No doubt the local Bureau people were attracted by the sheer volume of water that poured down through the steep-walled canyon of the Teton River, draining an area of over 1000 square miles, capped by the Grand Teton Mountain rising 23,766 feet above sea level to the east. The precipitation in that watershed, which falls mostly as snow, is 12 to 15 inches a year. These waters then flow into the Snake River, which has laid a rich alluvial plain across southern Idaho. There, in contrast, the average rainfall is only about 8 inches per year, making it a naturally semi-arid region. [Panel 10-11]

After the turn of the century, settlers in the Snake River Valley began building a private, cooperative system of canals, which dramatically expanded irrigation projects and agricultural production. Both the Bureau and the Army Corps of Engineers constructed public works to aid the farmers. By mid-century, 90% of Idaho's economy depended upon agriculture, primarily from the southern part of the State.[3]

In these first five decades, the United States Geological Survey assisted the two agencies with geologic and hydrologic studies of the Teton and its tributaries. A 1934 USGS report described the first specific site investigated, for a limited water storage project.

But selecting a suitable site remained difficult. In 1946 the Bureau rejected two sites on a tributary of the Teton, largely because too much reservoir water could seep into the highly fractured volcanic rocks of the canyon walls through which the river cut. [IRG 3,6] Gradually the concept of a multipurpose project evolved, no doubt to offset the costs of construction under the difficult site conditions. The project would control all drainage from the watershed and achieve the maximum benefits of water storage, flood control, irrigation, and hydroelectric power. [DOI 3]

From 1955 to 1960, the Corps and Bureau jointly conducted a survey of developmental opportunities in the Upper Snake River Basin [GAO-11]. In 1957, the Corps bored 285 feet through alluvium to fairly impermeable rock and decided that this would be acceptable; it would eventually become the site of Teton dam. [Panel 4-3] That same year, the Bureau drilled to solid bedrock at a site downstream but rejected it because the low banks there could contain only a small reservoir. It sought instead a site upstream with higher banks but still far enough downstream to capture the maximum runoff.

About the same time, the Bureau and Corps formed a joint committee to subdivide the territory in which both agencies had an interest. The Bureau was awarded jurisdiction over the Upper Snake River. [IRG 6] The Corps went on to build Ririe Dam on a tributary of the Snake, some 30 miles below the Teton site. [GAO 20] In 1959, the Bureau began a reconnaissance survey for a site on the Teton.

Public pressure for a project began to increase in the irrigation district around the junction of Snake and Teton Rivers, an area of 110,000 acres. Bureau staff met repeatedly with representatives of agricultural groups and encouraged them to urge their Congressmen to support a Teton project The water-users also mobilized Chambers of Commerce and other community groups in the area. At a public hearing in Idaho Falls in December, 1960, a resolution was unanimously adopted in favor of the project. In January, 1961, the Idaho State Legislature passed a "Memorial" petitioning Congress and the President to give early consideration to construction of the Lower Teton Reservoir. [CGO 5]

Over the next year, natural events seemed to conspire with user needs. That summer a drought emergency was declared along the Upper

Snake River. In February of 1962, cropland along the Snake River tributaries and on its plain were declared a disaster area from severe flooding. [CGO 4] In March, at the request of the Bureau's Commissioner in Washington, both its Engineering and Research Center (E&R) and the Idaho District Office approved a special report based only on preliminary reconnaissance data, urging early Congressional authorization for a multipurpose project estimated to cost $52 million but yielding benefits worth $93 million. Customarily, such a proposal should have awaited completion of a more detailed feasibility study. Meanwhile, a Bureau geologist visually inspected four new sites but supported the particular site that the Corps had earlier found acceptable. [GAO 11]

The project was to be built in two stages: first a dam, a hydroelectric plant, and a canal to a nearby irrigation district; the second stage would extend irrigation into new areas. The only objections to that site came from a local wheatgrowers' association, which argued that the first phase of the project would only stabilize and expand the yield of existing acreage; it wanted a site farther upstream to irrigate additional land in its area. [Panel 4-3]

Without debate and with little discussion, Congress's project was approved on September 7, 1964. It now seemed essential to control chronic natural disasters as well as to meet expanding agricultural needs. [CGO 5]

The Bureau then closely followed its formal sequence of routine procedures.[4] It made an intensive geologic study of the site, drilling over 100 deep holes to explore the foundation and to assess local sources of sand, gravel, and silt to be used for construction. The many holes were necessary because alluvial material had been deposited rather randomly in the riverbed, often in the form of lenses (thick in the center, tapering out at the edges), posing difficulties in extrapolating the nature of subsurface conditions from one core to another. The local materials were not considered ideal but were no worse than materials used successfully at other dam sites. [GAO 29]

The Bureau used sophisticated equipment to determine the extent of fracturing. It also conducted one of the most thorough pilot grouting programs in its history, inserting water and concrete into the rocks to determine how best to seal the extensive cracks. By the end of 1969, it was confident that it had sufficient data on the geologic problems at this particular site and that these were no greater a challenge than at sites of successful Bureau dams. [GAO 31]

The E&R designed a five-zoned earthen structure more than 1600 feet deep from its upstream to downstream "toes" (the base of the structure in the stream-bed) and extending 3100 feet in width across its crest. Ninety-percent of its bulk was an "impervious" clay core of silt compacted to retard the seepage that would inevitably occur. On the downstream side a drainage layer of coarse sand and gravel would carry the seepage that did occur harmlessly down the face of the structure to the riverbed below. Other zones or layers supported and protected the structure. Such a zoned design was common practice.

The core was to be set on a wide-angled "cutoff trench" dug about 150 feet below and across the riverbed between the banks. To prevent water tunneling under this would be a "grout curtain," a row of closely spaced holes drilled as deep as 300 feet below the trench, each filled with a cement mixture under pressure to seal all subsurface cracks and create a kind of subsurface wall. Such a curtain was frequently installed when foundation rock was not ideal.

To compensate for the special problems of the site, the Bureau devised several somewhat innovative techniques. Since a single grout curtain might not adequately seal the fractures in the rocks, the designers added two parallel rows of holes to contain and reinforce the central curtain. It had learned the necessity of this a decade earlier at a similar site of the Fontenelle dam, when water began to tunnel under the dam through the single curtain.[5]

Since the canyon walls were also cracked and covered deeply with loose material, the designers added "key trenches," steep walled excavations up both banks of the canyon, also to be underlain by a triple grout curtain and filled with compacted silt like the cutoff trench. Thirdly, to control the spread of the fluid cement mix through the crevices around the grout hole and to hasten its setting closely into a compact curtain, the designers called for calcium chloride, or salt, as needed, to be added to the grout mixture.

These technical modifications gave the Bureau confidence in its design. The drawings and specifications were put out for bid in July of 1971. The contract was let in December, and work began at the site in February, 1972.

Construction went smoothly. The only major change in the original plans was the use of about three times the amount of grout originally anticipated. Congress granted additional funds for this without question in one of its annual appropriations. Before the dam failed, these totalled more than $70,000,000. [CGO 6-8]

The only major hitch in scheduling was a delay in the delivery of a piece of equipment for the main river outlet, so that only the auxiliary outlet was operating when the reservoir began to be filled in October of 1975. The filling was slightly ahead of the original schedule but was necessary so that the contractor could test the turbines in the power plant the following spring. The reservoir began to rise by one foot per day, the rate the designers had specified. [GAO-58]

Then an unexpected natural event occurred. The snowpack in the mountains above the dam was almost twice as heavy as the average over the past 20 years, and was melting rapidly. [Panel 10-11] The runoff began to exceed the capacity of the auxiliary outlet to release excess water. In March of 1976, the Project Construction Engineer requested permission to fill the reservoir more rapidly. He cited the advantages of water for that summer irrigation and for recreational use of the reservoir. Permission was granted. The rate of filling increased and on several days was more than four times the originally specified rate. But the Project Engineer and Bureau officials obviously had no choice. [Panel 10-12]

When the reservoir was almost full, the crew noticed fresh springs and seepage below and at the base of the dam. But the water ran clear and the volume was small; such incidents are not considered unusual nor a cause for alarm.

Then at 9 a.m. on the morning of June 6 leaks appeared high on the dam. Within an hour, a tunnel opened up, spewing out over 6,000 gallons of water per minute. The Project Engineer notified sheriffs of the two counties below but conveyed little sense of urgency to at least one of them. That sheriff requested the local radio station to broadcast a warning, but the station allowed its taped program to conclude before making an announcement.

By 11 a.m., a whirlpool had developed in the reservoir. At 11:20, two bulldozers, which had been used to try to fill the hole on the face of the dam, fell into it. Erosion progressed up the face until at 11:57 the dam collapsed.

It was said that many people who had lived through natural floods failed to understand the seriousness of the situation. Indeed, no one had ever thought that the dam would fail. [GAO 67]

This story offers few clues that the project would end in a catastrophe. The idea of the project evolved slowly, until the agency responded in a timely way to public needs and natural disasters. It took exceptional care in studying the site. It built upon experiences elsewhere and modified the design with defensive measures in a conservative manner to accommodate specific site conditions. Construction followed specifications, except in increasing the grout to create a more solid foundation and in filling the reservoir more rapidly in response to an unexpected combination of man-made and natural events.

Background on Building Dams. Before considering how the federal government responded to the failure, a bit of background is in order. Structures to stem the flow of water over the land are almost taken-for-granted around the world. This country contains more than 67,000 large dams, 25 feet or more in height or impounding 50 or more acre-feet of water, in addition to several million smaller ones.[6]

Such water impoundments are no modern phenomena; men have piled earth across stream beds for over 4,000 years.[7] Structures of hewn stone, "gravity dams," date back to early Mesopotamian and ancient Greece. Early settlers in New England even built dams of wood. Within this century engineers have created arch dams of reinforced concrete, sometimes exceeding 700 feet in height, but these constitute only a small percentage of all dams. The majority of large dams are still earthen structures, the highest, designed by the Russians, to rise over 1000 feet.

In 1842 the first engineered dam in this country was built for public water storage at Old Croton, N.Y.[8] During this century the rate of dam construction accelerated from an average of 205 large structures completed each year from 1900 to 1930, to 570 in the period 1930 to 1950, to approximately 1610 a year, or an average of four of five per day, during the 1960s. [FCCSET11] Since then the rate of construction of large dams has dropped due to rising costs and a shortage of suitable sites. Like Teton, large modern dams usually serve many functions. In the 1970s, most new ones were of moderate size and largely for recreational or aesthetic purposes.[9]

Dam failure is such a rare event that the average annual death rate attributed to the failure of dams, large and small, is insignificant; more people drown each year in reservoirs and impoundments.[10] The most memorable disasters, as that which caused 2,200 deaths in the Johnstown flood in 1898, were of structures built without the benefit of modern engineering and then poorly maintained. In fact, by the middle of this century, well-engineered dams were considered "permanent structures."[11]

The federal government has pioneered in dam engineering, shaping waterways since the early 19th century. By 1976, 18 federal agencies were authorized to carry out responsibilities related to dams, notably the Corps of Army Engineers, the Bureau of Reclamation, and the Tennessee Valley Authority. The government owned and operated almost five per cent of the larger dams in this country; the remainder fell under the jurisdiction of the States. The major federal agencies[12] occupy a position of leadership in dam construction far greater than these numbers imply, and were particularly proud of their record of no major failures, until the collapse of Teton.

Investigations

Immediately after the failure -- pictured on the cover of Time -- a slew of special investigations began. Both houses of Congress, the General Accounting Office (GAO), the Department of the Interior, the Idaho Governor's Office, and the National Academy of Sciences (NAS), among others, set up groups to study and report on the incident.

Like Monday morning quarterbacks, each group tried to reconstruct what had happened and to identify the causes of failure, each in light of its particular interests and the facts it attended to. Presented here are highlights of four investigations that influenced the OSTP agenda. Of interest is how two groups with different diagnoses converged on a similar set of remedies while two others starting from identical facts diverged in their conclusions.

The House Subcommittee Investigation. Two weeks after the disaster, Congressman Leo J. Ryan, Chairman of the Subcommittee on Conservation, Energy, and Natural Resources of the House Committee on Government Operations (henceforth referred to as the Subcommittee) inspected the ruins and talked with officials and victims in Idaho. His subcommittee then began a series of hearings that continued for almost a year.

Ryan recognized that the site was a poor one but considered this a "man-made disaster." [CGO 8] Early in the investigations, a Bureau official was overheard to say that the agency had nothing to learn from the failure. To this, Ryan commented, "... bureaucratic infallibility is an idea whose time has long since passed." [3] At the opening session of the hearings on August 4, he articulated a "theory of momentum" under which he diagnosed the problem. [14-16] The Bureau and other federal agencies were wont to continue projects, once begun, in spite of hazards detected and warnings given.

Ryan focused his attack on the bureaucratic "momentum to construct," fueled by engineering hubris. This was evident in the belief within the agencies that they could engineer a solution to any problem, based on "a general principle of modern engineering" enunciated by a top officer in the Corps that "almost any site can be used for construction if the owner can stand the price."[13]

Initially the Bureau's official position was that what had happened at Teton was "impossible." Its officials were so confident in their engineering ability that they maintained that the site was adequate and that nothing was wrong with the design, engineering, or construction. [24] And yet it had happened.

The Subcommittee supported its theory by citing four instances in which significant warnings went unheeded. One occurred before excavation began when a team of USGS field geologists, mapping the Snake River Basin, concluded that the area was seismically active. Yet the Bureau's geologic reports had acknowledged neither seismic hazards nor the recent discovery that newly filled reservoirs can trigger quakes and cause dam failures. Thus, "in the spirit of cooperation," the geologists sent a lengthy and urgent handwritten memorandum to their home office to be typed and relayed promptly to the Bureau's Site Engineer.

But over the next six months their superiors in Washington revised the material, deleting all sense of urgency and transforming the memo into a more "objective" scientific paper. The Bureau never formally acknowledged receiving it. [16-22] One of the team later said, "... the emotional -- the feelings we had about the thing really could not be documented scientifically." [19]

Another warning cited was when the Bureau's Regional geologist first met with the Project Construction Engineer at the site and told him that this was not a good place on which to build a dam. His professional opinion was never relayed, orally or in writing, to Bureau officials in

Denver. The Subcommittee concluded that both men were "intimidated" and "silenced" by "the force of the momentum." [22]

More evidence of warnings unheeded emerged during excavation and grouting. The crew uncovered cracks in the highly fractured rock far in excess of the maximum size of 1.7 inches anticipated. Instead they found caves large enough for a man to walk in. One hundred feet down one narrow passage was a cavity containing a rock the size of a pick-up truck. [14] At another point, grouters, filling a hole in the side of the canyon, were forced to flee from an emerging swarm of bats. [23]

But the Project Engineer had been confident that grouting could adequately seal these holes. The Subcommittee concluded that the Engineer's exaggerated overconfidence was typical of Bureau officials, "bordering on arrogance." The Bureau's own grouting expert later testified that "certainty is never possible"; grouting is not an exact science but more like an art, necessitating a certain "feel" for the work. [24]

Finally, the Subcommittee decided that the Bureau took an unwarranted risk in filling the reservoir so rapidly. This is the time when a dam is put to the "acid test". Bureau officials had granted permission for rapid filling contingent on findings from a "superior" program of instruments in 19 wells to monitor changes in groundwater around the site. But three of the instruments malfunctioned; data from a fourth, when later analyzed under a "steady state" model, indicated that groundwater had travelled under pressure at the alarming rate of 1,000 times what was anticipated. But the Engineer had relayed such data routinely in his monthly report, which the Bureau's Denver office received only after the dam had failed. The ERC later used a different, "transient," model to analyze the data and found that it did not indicate a "pressure response" and was thus presumably less alarming. The Project Engineer would not comment on this to the Subcommittee, disclaiming expertise as a groundwater geologist. [24-27] Nor did he mention that the Bureau had no choice in the rate of filling.

However, in its eagerness to justify its theory, the House Subcommittee neglected to distinguish the different positions and roles of its informants in the bureaucracy. The USGS was a separate agency from the Bureau. Contractors came from the private sector, outside the bureaucracy. The Bureau officials in Denver, who approved the rapid filling, ranked high above those at the site. Yet under the theory all were treated simply as parts of a monolithic body equally subject to the abstract force of "momentum."

Nor did the Subcommittee reflect on the role that Congress itself had played to initiate and maintain that momentum by appropriating funds until the dam failed, ignoring public protests. Voices were raised not in the executive branch nor directly to Congress, but in the courts; they were not about safety and thus apparently not germane to the Subcommittee's argument.

For instance, in 1971, environmentalists, concerned that the 17-mile long reservoir would destroy scenic areas and wildlife habitat upstream (but apparently unconcerned about the hazard to human habitat downstream), sought to stop the project through an injunction requested from a Federal District Court. Their grounds were technical and procedural: the Bureau had overstated the economic justification for the project and had filed an inadequate environmental impact statement. A higher court halted this legal action in 1974. [9]

In 1971, the Idaho Water Resources Board also protested to the federal Council on Environmental Quality that the benefit-cost ratio was been incorrectly calculated and that the project was not economically justified, to no avail. By 1969 the Bureau had lowered the preliminary 1961 ratio of 1.79:1 on which the project had been authorized, to 1.55:1 and reduced the estimated cost from $52 million to $48.5 million. No procedures required revision of that estimate again, once Congress had begun to appropriate funds routinely.

Instead, the Subcommittee focussed on matters within its scope of authority, to oversee operations of the Executive branch. Its prescription followed from its theory and was consistent with its metaphor of bureaucratic momentum. The Secretary of the Interior should apply "appropriate brakes on the momentum to build," or else legislation might be passed to reorganize federal dam operations. The Bureau and others should establish new administrative procedures to improve their decision-making, instituting procedures for setting a mid-point for reappraising projects under construction and for halting construction, should any significant safety hazards appear. [33] In essence rules were needed to slow momentum and, if necessary, to stop a project.

Technical Investigations. Shortly after the disaster, the Governor of Idaho and the Secretary of the Interior, Cecil B. Andrus, convened a group of international technical experts from outside the federal government as an Independent Panel to review the cause of the Teton Dam failure. [Panel] Its aim was to reconstruct the sequence of physical events within the structure leading up to the breaching. But it faced a problem common after engineering failures, that much of the evidence has been destroyed, in this case, literally washed downstream.

The Department of the Interior also created an Interagency Review Group (IRG) with a similar mission and made up of representatives from five agencies: USGS, the Soil Conservation Service (SCS), TVA, the Corps, and the Bureau. The two groups shared the results of field investigations and laboratory tests, but independently analyzed the data and the design,specifications, and testimony from many involved. (The term "experts" will distinguish the Panel from the officials of the IRG.)

Neither group could reconstruct with any precision or certainty what had happened. Both rejected the hypotheses that seismicity was involved or that differential settlement had cracked the structure, and neither considered the rapid rate of filling significant. Both agreed on a very general explanation, that the dam had failed due to internal erosion (piping) through the core. Water had entered openings in inadequately sealed rock joints and tunneled through the core before it burst through other zones high on the right abutment.

The experts proferred several tentative explanations for parts of some complex sequence of events. A theory of "hydraulic fracturing" was one: the geometry of the steep-walled key trench had forced the fill, expanded with water, to arch, causing cracks in the core above. In 1976, this theory was relatively new, more like an hypothesis, still to be tested, and unknown at the time the Bureau designed the dam. [Panel App. I]

Neither group found anything amiss in the site selection procedures and geologic studies. Documents indicated that the site was "the best of the available sites for the purposes of the project." [IRG III] Like the Bureau, investigators did not question those purposes nor whether men should have tried to stem the flow of that river; all seemed to take for granted the basic principle that a permanent dam could be build on almost any site. They accepted their immediate problem as given, to identify the physical causes of failure, how the dam should have been designed.

The geologic studies had been "appropriate and extensive." [Panel v] The pilot grouting program was exceptionally thorough and forecast many of the difficulties. The design followed Bureau practices developed through years of experience. Construction conformed to the actual design, to the plans and specifications, in all significant respects except scheduling. [Panel vii]

The Bureau had been aware that the silt in the core and trenches was brittle and erodible, and also that the downstream drainage layer was rather impervious, but it had used such materials on earlier dams, but apparently not both together. In combination, the drainage layer had prevented the exceptional seepage from flowing harmlessly down the face of the dam. [Panel 7-11] If that layer had functioned as intended, the weakness in the structure might have been evident earlier and allowed the population downstream to flee before the dam was breached. The experts suggested that adequate instrumentation in the structure (not just in the banks) might also have provided early warnings, but the Bureau was so confident that it could predict problems in its earthen dams that it did not install this expensive equipment. [IRG D-4]

The experts casually mentioned one decision that seems to have contributed significantly to the failure. Late in 1970, when environmentalists were challenging the project, the designers decided not to take sand and gravel from the riverbed downstream "apparently to avoid damaging the downstream environment," but to use as much material as possible from upstream, although it contained more silt. They correspondingly reduced the sand and gravel requirements in the specifications for the core. [8-1]

One can imagine that economic reasons bolstered this decision made in a political context. The reservoir would eventually conceal the upstream "borrow pits," as these excavations were called, but the Bureau would have had to grade over unsightly holes downstream, at some expense. The experts did not explore reasons for this decision, perhaps because both political and economic matters were outside their mandate and expertise.

Instead, they found the cause of failure in the design and faulted designers, but primarily for omissions. More defensive measures should have been used against the possibility of piping, such as filters over the core, transition zones under it, grouting on the sides of the trenches and a drainage system in the abutments. [8-1]

The officials also recognized these omissions but defended them with a kind of psychological explanation: "the designers' reliance on the grout curtain 'inhibited' their adopting other measures..." [103] Apparently design engineers were so satisfied with their innovative solutions that they forgot to attend to other potential problems. A main criticism was that they did not document how they "logically" had arrived at their decisions. [87]

More important, even though "windows" were found in the grout curtain and fill was often insufficiently packed under overhanging rocks, both groups absolved construction personnel from responsibility or blame, and instead commended the workers at the site. The project engineer "did a good job." [Panel 11-18]. Supervisory personnel were "knowledgable" and "interested" and "faithfully carried out all aspects of quality control." [9-6] The contractors were "competent" and "administered their formal contracts in accordance with well-accepted practices." [IRG v] The field staff seemed interested in the quality of their work and "determined to achieve, or exceed, the desired results." [IRG C-5]

Field workers obviously took initiatives. For instance, "to allay their concern for compaction of Zone 1 (core) material over large voids," they developed "surface treatment procedures," filling cracks and laying slurry grout in the bottom of the trenches. They only ceased this practice high up in the key trenches, where piping later occurred, "on orders from above." [IRG D-6]

The experts said that questions about the quality of construction should not deal with its execution as much as with the "exercise of judgment in matters more related to fundamentals of conceptual design." [9-66] The Bureau had not considered sufficiently the unique and unusually difficult geologic conditions at the site nor recognized that every embankment has "its own personality." It concluded that building a dam at that site called for "the best judgment and experience of the engineering profession." [ix]

We may never know the final answer to the specific cause of failure, the Panel concluded. It may have been from some combination of geologic details, the geometry of the key trench, variations in compaction or stress conditions, but which precise combination of factors initiated the failure "is of course unknown and, moreover, not relevant." [12-18]

> "The failure was caused not because of some unforeseeable fatal combination, but because (1) the many combinations of unfavorable circumstances inherent in the situation were not *visualized*, and because (2) adequate defenses against these circumstances were not included in the design." [12-18, italics added]

In diagnosing the cause of failure as a lack of professional imagination and redundancy in the desig, the Panel did not consider that additional defenses would have clearly made the project economically unfeasible.

The officials generally agreed with the experts' conclusions, but took their diagnosis a step back into the organizational context. During their study, they had identified "areas where it appears that procedures and documentation, or lack of them, may have played a part in decisions that ultimately led to the failure of Teton Dam." [101] This led to four recommendations for new procedures requiring:
(1) independent boards to review the design and construction of each major project, (2) design decisions to be formally documented in writing; (3) design personnel should remain active and visit a project frequently during construction, and (4) instruments should be installed and data from them and promptly interpreted at all major dams. [107]

Although starting with identical data, investigators diverged, consistent with their institutional positions. Outsiders, independent consultants, called for more of what they themselves possessed, the best professional engineering judgment and imagination. Like the House subcommittee, the bureaucratic officials stressed impersonal remedies, new administrative procedures, particularly to govern engineers.

The GAO Investigation. At Ryan's request, the General Accounting Office (GAO) investigated the procedures and practices of the Bureau and also of the Corps and several non-federal groups. Assured of no major problems in construction, it focussed on site investigation and design. In general, it supported the recommendations of the House subcommittee and the officials for consistent controls over design and administration processes.

The GAO seemed leary about the use of professional imagination and judgment. Although acknowledging that each site is unique, it descried the agencies' lack of explicit criteria for choosing "acceptable sites" and of specifications for "adequate" site investigation. Like the officials, it called for independent reviews of agency judgments on these, especially "due to the relative complexity of the remaining sites." [22]

The GAO urged designers to make their "intent" explicit and agencies to adopt procedures ensuring that the "design intent" is carried out. In the case of Teton, the intent has been so unclear that the staff had "misinterpreted" general instructions and allowed the crew to apply slurry grout at its own discretion. [42] The GAO obviously frowned upon imagination and discretion at all levels.

Moreover, noting that the theory of hydraulic fracturing had not been adequately tested, the GAO concluded that dam building "has not reached a point where designers can predict all problems." [36] Implicitly until that science advanced, multiple defenses were justifiable.

The GAO also chided the Bureau for not learning a lesson from a near failure during the filling of its Fontenelle dam, a failure averted by lowering the reservoir through oversized outlets; the Bureau should have made sure both outlets were usable before filling Teton. [65] The GAO seemed unaware that the Bureau had taken a different lesson from Fontenelle, addressing what it saw as the cause of near failure by adding two rows to the grout curtain at Teton.

The GAO also pointed out inconsistencies among agencies, for instance that none had formal criteria for rates of filling or lowering reservoirs. The Corps built its dams to withstand any rate of filling and the Bureau used one foot per day as an "unwritten rule of thumb." The Corps and Bureau could empty one in 90 to 120 days; the State of California expected to empty half a reservoir in 14 days. Agencies also differed on means to handle heavy flows the Corps and TVA depended mainly on spillways; California and other agencies used outlets.

[63-64] Each seemed to have its own "style" of engineering; the GAO apparently expected there to be one best way.

Both Corps and Bureau also had "procedural gaps" in programs for inspecting existing dams and in monitoring them with instruments. The use of instruments raised other problems. The anomalous data from Teton had "different meaning and significance" to different people, including designers. [51] Project staff were not qualified to recognize abnormal readings, and even ground water specialists had not known what ranges to expect. On the other hand, the GAO reported, park rangers and maintenance workers in California, "with little dam design and construction experience," read instruments on existing dams and transmitted data to the Corps district office, apparently quite satisfactorily. [55] The GAO did not realize that instrument data at a new site can be meaningless even to experts until patterns are recognized specific to that particular site.

The GAO discovered that the field staff at Teton had devised a "Self-Protection Plan" of instructions for internally reporting serious or unusual conditions, perhaps reflecting unspoken concerns of the construction crew. [71] In fact, none of the federal agencies had plans or procedures for emergencies, which obviously would not occur if dams were permanent, failsafe structures.

In a postscript to the formal investigations, the Bureau later claimed that contractor negligence was responsible for the failure.

Investigators had uncovered an "anomalous layer of cold, moist fill" horizontally across the core where the water had probably tunneled. The Bureau then suggested that late in the Fall, when contractors added water to the final layer of silt, it froze into a thin layer of ice which was sealed in with new fill in the Spring. The ice eventually melted into a plane pervious to seepage across the core. This discovery enabled the Bureau to deflect blame from itself and open legal actions against the contractor, and made designers aware of inhomogeneities in all earthen dam construction.[14].

Planning for Federal Dam Safety

In December of 1976, as investigations continued, top officials from about 14 federal agencies dams, met and decided to establish an Interagency Committee on Dam Safety (ICODS). Early in 1977 they adopted a formal charter with the purpose of preparing a set of guidelines outlining procedures to assure attention to dam safety in each of the constituent agencies.

To draft the guidelines, ICODS organized a small hierarchy; under a 5-member steering committee were three subcommittees, one on each step of the formal sequence of site investigation and design (SID), construction (CON), and operation and maintenance (OPM). Each was further subdivided into special task groups to prepare specific parts of the guidelines, with membership distributed among the agencies. [FCCSET Appendix B]

In March, after more House Subcommittee hearings, Congressman Ryan wrote to the President urging the administration to take action on dam safety. On April 23, 1976, Jimmy Carter issued a memorandum [FCCSET 1] to the Directors of OSTP and OMB and to the Secretaries of Interior, Agriculture, and the Army, the Commissioner of the U.S. Section of the International Boundary and Watery Commission (IBWC), and Chairmen of the Federal Power Commission (FPC), and TVA. They were directed to cooperate with OSTP in a three part program. Each agency should first survey practices affecting the integrity of dams under its authority. Second, OSTP should convene an interagency committee to propose guidelines for management procedures to ensure dam safety. Third, a panel of recognized outside experts should review agency regulations, procedures, practices, and the proposed guidelines.

Carter urged special attention to several aspects of dam safety. These included the effects of "cost-saving incentives" and also of earthquakes or earth movements (Frank Press's primary interest), the extent of in-house and outside interpretations of data in site selection and design, the use of "new technological methods," and the involvement of local communities in "identifying, analyzing, and solving dam safety questions." One item, "the degree to which probabilistic or risk-based analysis is incorporated into the process of site selection, design, construction, and operation" put the new technique of risk analysis, then poorly understood in OSTP, on its agenda.[15]

Philip Smith, one of three associate directors of OSTP, was put in charge. As technical consultant and staff, he brought in Bruce Tschantz, an engineer from the University of Tennessee. His recent testimony on State dam safety programs before the House Subcommittee had impressed the Congressmen,[16]

Smith converted the Steering Committe of ICODS into an ad hoc committee of the Federal Coordinating Committee on Science, Engineering and Technology (FCCSET) and in May convened it in an impressive conference room in the White House. A cadre of Army top-brass attended to represent the Corps. In heated discussions that ensued, one Corps engineer emphatically repeated the statement that with enough money the Corps could build a safe dam on any site. This arrogance irritated Smith.[17]

He also had strong reservations about the federal government even being in the business of building and operating dams, but admitted privately that as an avid whitewater canoeist he was probably "biased." Lacking expertise in dam engineering and pre-occupied with other duties, he left technical matters and much of the day-to-day work to Tschantz, whose first assignment was to "interpret the President's memo" and would then work closely with the 5-member steering committee.[18]

Tschantz modified the wording of the President's list to conform to the lexicon of dam engineering, adding items to cover all essential aspects of the conventional process from site investigation onward.

Initially he hoped that the agencies would evaluate their own procedures and insert their scores on a matrix of sixteen specific items. But the agencies objected that if the public learned that safety procedures were less than adequate, it might become unduly alarmed. Undoubtedly, low scoring agencies also feared losing face and funds at budget time in Congress.

So Tschantz asked them only to report on relevant procedures and practices in the sixteen areas and describe any other problems. At first, some did not seem to take the assignment seriously or protested that the checklist was not relevant to their operations. For instance, neither the TVA nor IBWC were constructing new dams. The Soil Conservation Service (SCS), and other agricultural agencies were concerned primarily with small structures. FPC was primarily interested in regulation of hydroelectric power projects. Most eager to cooperate was the representative of the Bureau, which had been humbled by the failure and forced by many inquiries to reflect on its practices. Tschantz also added William Bivens, a former classmate, to the Steering Committee to represent the Nuclear Regulatory Commission (NRC), which regulates impoundments of water from nuclear power plants and had pioneered in the use of risk analysis.

At the end of June, when Tschantz and Smith met with staff of OMB, he established his credentials in OSTP. The consultant so persuasively presented a paper on dam safety issues, especially on the deplorable status of State programs, that OMB agreed to seek funds for the Corps to inspect non-federal dams, a long neglected law, under P.L. 92-367, passed in 1972.[19] His success gained him "clout" among the agencies.

Over the summer, the Corps solicited comments on items in the survey district offices. DOI hired a consultant, Woodward-Clyde, to assess the procedures of the Bureau and its five other agencies. A committee of top officials representing six agencies compiled Agriculture's report. [27-28] In September, they submitted more than 2000 pages of material to OSTP. Most had catalogued copies of formal documents, lists of administrative and technical procedures and statements of policy intent under the survey headings to attest to safe practices. The USDA justifiably complained that time did not permit a field study but the others seemed to assume that these documents represented their actual field practices. Tschantz laboriously summarized this material for the FCCSET Report. [27-68]

Survey Findings

The most general item, the use of "Cost-Saving Incentives" seemed unproblematic, and all agencies claimed to achieve some balance in phrases such as TVA's: "Dams are designed and constructed with regard for economy. However, saving of cost is not permitted to compromise dam safety." [67]

Personnel qualifications seemed adequate. All cited criteria for formal education and experience in hiring and staffing. For instance, 15% of the Corps' professional employees held advanced degrees and two thirds of its engineers and scientists were registered. All listed programs for professional development, for "technology transfer", and for improving technical skills, such as in-house lectures and seminars or the availability of new publications, participation in professional societies, or on-the-job contacts with superiors.

But more was needed, the Corps volunteered, such as programs for training sub-professional operators and maintenance crews, who lacked the engineering background to recognize dam safety problems, and for rotating field and office engineers. Engineers were later said to resist field assignments, perhaps because these symbolized loss of status or forced them to live far from urban areas. [OSTP-24]

The Bureau brought out how bureaucratic matters impeded professional development and performance: "...engineers have little time to participate in activities which lead to technical growth and sometimes may not be able to give full technical attention to project requirements." It explained that "recent reductions in ceilings on employee levels" and increased workloads had diverted "substantial numbers of 'personnel slots'" from design and construction supervision to work required to conform with "social and environmental regulations," citing the Occupational Safety and Health Act, Privacy Act, Freedom of Information Act, and the National Environmental Policy Act (NEPA). [42]

Most agencies called upon the professional judgment of others within their agencies for "Internal Reviews" of projects, but brought in outside consultants where unusual problems were anticipated. Only TVA had regularly used independent boards for external reviews. Its internal reviews often took the form of informal consultations between persons involved earlier and later in projects, no doubt because TVA did not use contractors but hired and trained local people, maintaining a cadre of experienced personnel within the Tennessee valley. [GS-67] Such informal consulting might be more difficult when agency operations are geographically diffuse.

The Corps raised an issue that the survey had not addressed. Because each site is unique, with diverse conditions, initial geologic data is often inadequate. Designers often have difficulty interpreting data and appreciating foundation conditions before the site is fully excavated. The Corps seemed to fault USGS and State geologists for supplying inadequate information; it had to exercise "continuing vigilance" to assure that scientists under contract supplied information consistent with designers' needs. Since problems frequently develop at the dam/foundation interface, as they did at Teton, the Corps recommended that preconstruction assumptions be re-assessed during construction so that foundation treatment could be modified. [33-34]

The survey implicitly recognized such problems under "Assimilation of New Field Information" into the design. On the other hand, "Orientation of Construction Representatives" to the design intent seemed make construction supervisors responsible for identifying new information to be incorporated into the design. One message was clear: before construction practices could vary from the specifications, those at the site must send information up to the designers so that they could modify the design documents. These seemed to be assigned an almost sacred status.

The Corps also admitted difficulties with its "Construction Quality Control" (CQC) procedures under Army regulations; most other agencies used less elaborate procedures for materials testing under Federal Procurement Regulations. The Corps hired independent subcontractors specialized in CQC to monitor the prime contractor and had cut back on its own inspection staff, which now only checked that the subcontractor complied with his contractual obligations. But working through a "middleman," as it also did on site investigation, the Corps had encountered difficulties getting the prime contractor to meet the specifications and felt that the new arrangements had weakened its quality assurance program. [30]

The engineers' mistrust of those responsible for completed dams was obvious in another item, "Control against Improper Operation and Maintenance." Although some agencies had manuals for operating larger dams, most conveyed instructions informally and, by implication, failed to exercise sufficient control.

Most agencies also cited procedures for "Periodic Inspections" of existing dams, but few were probably better than the Corps: "Excellent in concept" but in practice district offices lacked "diligence" in carrying them out. [29] Part of the problem lay in obtaining funds for maintenance and non-emergency repairs. The Soil Conservation Service had no program to follow up on small dams prolifically constructed on its advice.

The Department of Agriculture did work with private individuals and groups on small projects. But in contrast to the broad role envisioned for citizens in the Carter memo, most agencies became involved with the "Non-Technical Community" only through NEPA procedures, which required public hearings on environmental impact statements. or through the licensing procedures of the NRC and FERC, both completed long before construction began. TVA with its system of dams completed continued to work with community groups in their operation.

The regulatory agencies alone required emergency plans, to be made by others, to evacuate people downstream from the threat of a dam failure. Conversely, under "Security Against Sabotage," the Corps, the NRC, and IBWC had procedures for protecting dams from the threat of people.

<u>"Risk-Based Analysis"</u> One surprise from the survey was the unity of the agencies, except the NRC, in opposition to what was called "risk-based analysis."[20] Most of these engineers were not opposed to

probabilistic methods per se, which were commonly used on aspects of design, as in calculating the maximum probable flood or the strength and tolerance of materials. Their conventional method for handling uncertainty was the "factor of safety approach," or allowing a "margin of safety" under a policy of "conservatism" in design and construction to give them assurance that their structures would not fail.

The arguments against risk-based analysis were diverse and often contradictory. The Corps, usually at the forefront in technical innovations,[21] had studied the new technique and concluded that it had "little to offer in its present state of development." The Bureau listed specific problems: its use would require assessing the probabilities of all factors in each phase of the process of building and operating dams, taking care that these factors were "all inclusive and mutually exclusive." Even if all factors could be classified to make them causally independent, the mathematical probabilities of failure of many factors could not be determined. Thus the Bureau concluded that risk analysis required "judgmental assessment," here presumably unreliable. [45] Conversely, USDA argued that these procedures, not yet proven to be valid, might lead to "the substitution of numerical values for professional judgment," [37] here presumably more reliable than a technique.

The NRC admitted that risk analysis applied to dams, in contrast to nuclear power plants, had "technical limitations" but alone continued to endorse its development and use. [59] The value of property and number of people at risk downstream (the magnitude of the consequence) could at least be used to set priorities for attention among high hazard dams. The technique could provide a "more explicit approach to the understanding of risk" and give theoretical guidance to engineers in allocating resources among safety measures. But even when theories of risk were fully developed, the techniques could augment but never substitute for the competence of professionals and constructors, and in spite of the intention to reduce the risk of failure to zero, some risk would remain and failures would occur. [OSTP 23]

Privately, the agencies argued vehemently and ad absurdum against the use of risk assessment. To discuss risk openly would undermine both public confidence in dams and Congressional support for the agencies. It could bring an end to new water resource projects so desperately needed to increase agricultural production and feed the world's burgeoning population. Use of the technique would result in mass starvation.

OSTP admitted that assigning a mathematical probability to the failure of an individual dam was not yet technically possible and that any number would be "very arbitrary." On the other hand, the "social cost of a low probability disaster (residual risk)" should be factored into benefit-cost analyses used by Congress in authorizing new dam construction and to alert the public of the potential hazard of new projects in order to prevent an "upward bias" for dams that are not "economically justified or socially acceptable." [FCCSET 75] This idea of modifying benefit-cost analyses was consistent with Smith's desire to get the federal government out of the business of dam building and apparently one of his major contributions to the FDS program.

In the results of the survey released in November, Smith wrote an executive summary directed mainly at the Corps. It should complete inspection of all high hazard non-federal dams. With other agencies, it should involve the non-technical community more than "passively" and beyond the initial stage of dam development. [8] A research program should also be funded to meet such common needs as objective criteria on filling rates, and improved techniques for calculating seismic forces and especially for analyzing risk.

Tschantz himself evaluated agency procedures on a matrix, adding items raised during the survey, a complex analytical task. Only the NRC and the Federal Energy Resources Commission (FERC and formerly FPC) had perfect scores. Two giants, the Corps and TVA, were inadequate on many counts. Agencies in Interior, except for the Bureau, which had embarked on reforms, lagged behind those of Agriculture. Of course, agency spokesmen still objected that simple symbols could not and should not be used to rate and compare diverse agencies.

Tschantz also summarized major points for the guidelines from rough drafts and outlines submitted by ICODS subcommittees and appended to the report. More important, he concluded that the review process itself had been valuable and extensive discussions had resulted in safer dams because hundreds of agency people were now aware of "management lessons" to be learned from Teton's failure. [79]

The OSTP project had also set in motion a framework for treating dam safety as an "identifiable component" in all phases of a project. Moreover, he expected the federal program to stimulate, by example and standards, programs in the States. [80] In essence, the project had transformed dam safety into a separate matter and created a new program expected to serve as a model down through the federal system and out to every State.

New Uncertainties. However, both Tschantz and later the Independent Panel emphasized the uncertainties in dam engineering. In an introduction to the FCCSET report, the consultant recalled major failures of the past. Moreover, because of "uncertainties and lack of understanding" of material behavior and of man-made and natural destructive forces, and because of "imperfect control of construction processes," no dam will ever be "failsafe." [9] Imperfect knowledge and control were basic issues.

His argument went further. Dam engineering may draw heavily on mathematical principles and physical laws but is not an exact science; it is "more like an art." In practice, engineers must exercise judgment, grounded in experience, especially in the face of limited knowledge of foundation conditions prior to excavation. Therefore, throughout and after construction, dams must constantly be "re-engineered." [10] This solution would put the professional continuously in charge.

Meanwhile all existing dams are "aging" and, reflecting the accelerating rate of dam construction since the beginning of the century, an ever greater proportion are becoming fifty years old or older. [12] Moreover, maintenance is costly but yields no quantifiable economic benefits beyond those originally calculated. (Implicitly, such costs could only be justified as a way of avoiding the cost of failure.) At the same time, the number of feasible sites for new projects is decreasing and, as construction costs rise, engineers are under pressure to construct new dams with more limited resources at ever poorer sites. These were the key issues.

About a year later, the Independent Review Panel published its report, [OSTP] supporting and expanding on these views. It too emphasized that dam building is an art. Practitioners may draw on basic principles of sciences, such as geology, hydrology and seismology, and on bodies of knowledge in civil and construction engineering and in project management. These principles have emerged over time, in part through the work of several federal agencies and as professionals have learned from failures. But experience and judgment are essential in applying these principles for the development of water resources projects in the context of economic, environmental, social, and political factors. [8-9]

An issue of "overriding importance" was dealing with uncertainty arising from geologic conditions at a site, variable material conditions that escaped detection even with CQC, and "extreme meterological events and operating conditions which can be forecast only in a statistical sense." Human behavior was particularly unpredictable, making it "difficult and perhaps even impossible to quantify" such sources of risk as "errors of judgment" and "the inevitable human shortcomings" of designers, contractors, and owners. [27] Formal risk analysis offered little solution to these uncertainties.

The Panel elaborated on factors in the physical context that jeopardize dams. Hydrological and other conditions upstream can change over time, subjecting them to increasing pressure and greater possibility of failure. For instance, developers may clear land, increasing runoff, or a municipality may import water from another watershed increasing the river's flow. Nature or men may alter a river's channel increasing the speed of that flow; it may carry more sediment to settle behind the dam and reduce the reservoir's capacity, contributing to overtopping. A series of dams on a watercourse, like steps on a stair, often built and operated by independent agencies, with different standards, also create a potential "domino effect" of simultaneous failures. [12-16]

Internally a structure and/or its foundation may weaken or age in undetectable ways. For instance, certain kinds of foundation material deteriorate rapidly. Clay shales may lose "shear strength" and permit a structure to slide; limestone may dissolve into a pattern of voids like a honeycomb, allowing water to tunnel beneath a structure. Water seeps not only through earthen structures but even through concrete, which can also deteriorate from physical weathering or chemical action.[22]

Instruments may not detect the gradual process of internal aging in the early stages. The most common instruments are piezometers, deep set pipes, to measure underground water pressure, but, like those in the banks at Teton, are not extremely reliable. They were installed on less than 10% of existing dams and are especially expensive to emplace after a dam is complete. Meanwhile, people settling on low-lying plains downstream increase the possible consequences of failure.

The panel also contributed insights into the organizational and institutional context of dam engineering, particularly in the federal government. The "size and complexity of dam projects and of the agencies engaged in their administration" and "the natural tendency of large organizations toward compartmentalization and specialization" could stifle communication and the necessary coordination. Other considerations -- environmental, political, etc. -- may also divert management attention from "the overall objectives of risk reduction and cost-effective operations." [26]

A key problem arising from the institutional context was "fixed appropriations." On both public and private projects, major funding is committed before construction begins, and contingency funds often underestimate the "real degree of uncertainty or the costs which may result." Moreover, the "differences in spending rates," slow during design and more rapid during construction, aggravates the situation. "Designers are not accustomed to working in an environment where time is crucial" and redesign may create "havoc with the logistics of construction, carefully scheduled for cost-effectiveness." [27]

Other formal arrangements impede communication and complicate the situation. A contractor may not understand the implications of construction problems and so not report them to the engineer. Under formal contractual arrangements, he and the engineer may attend less to "project requirements" than to meeting their "individual obligations," which in turn can force all parties into "adversarial roles." [28] Also, although quality control programs offer the benefits of expertise and efficient testing, inspectors tend to develop "a testing psychology," rigidly following contract specifications rather than adjusting their procedures to actual "test results" or to obvious site conditions that indicate the need for more frequent tests or even different ones, so that actual deficiencies go unidentified and uncorrected. [29]

The Panel also suspected that some agencies viewed procedures on inspecting and re-evaluating existing projects as "bureaucratic requirements to be met 'on paper,' rather than ... achieved in fact." [19] Some lacked legislative authority or policies or simply failed to request budgets to take corrective actions. [20] But panel members could not agree on whether procedures should explicitly require inspections at specific intervals or should allow the agencies the flexibility to determine their timing, based on specific factors at specific dams. [21] Here as on other matters, the panel showed itself to be divided on whether to treat bureaucratic problems with more formal requirements or less.

The Panel did recommend extensive peer review, by outside engineers, even of each agency's organizational structure and operating conditions. Plans should also be made at the start of each project to "forecast potential problems and create a rationale for dealing with them ... in a timely, efficient manner to minimize construction delays, added costs, or distress to the completed dam." But this would require "an organization that assures communication and continuity of thought process with feedback . . . throughout the life of the project." [27] The implicit remedy here was for an organization rather than professionals to be continuously in charge. But the Panel gave little specific advice on financing, contracts, and other institutional arrangements, matters that would carry it far beyond its mandate and expertise.

A major concern of the Panel, comprised of engineers largely from academia, was that agencies had not attracted top quality professionals, an impression based on informal sources. Many had attended second rate colleges or had not achieved high standing in their classes. Therefore, professionalism should be strengthened by using outside consultants on difficult projects and internally through professional development programs following the guidelines of the American Society of Civil Engineers (ASCE). [23-25]

But the greatest threat to dam safety lay within the culture of the agencies: the slow insidious development of attitudes that trivialized safety issues in small agencies, and in large ones reduced dam building to routines and allowed personnel to "become complacent, over-confident, and perhaps, even arrogant in the belief that they know with certainty how to build a dam that will not fail." The Panel emphasized that "*the most fundamental principle of dam safety is the recognition that every dam runs some risk of failure*." [8, italics in the original]. Thus changing personnel attitudes was a basic problem to be solved, but the question was how?

The Panel and Tschantz had become aware of an unwritten rule within the agencies forbidding even *talk* of risk and failure. The subject was *taboo*. The agencies had argued that should personnel even mention that a dam might fail and it later did, they might be legally liable for a structure they had "known all along" was unsafe. If anyone *knew* that a dam would be unsafe, he should speak up so problems could be corrected.

This argument suggests that a fundamental objection to risk analysis lay in the fear of liability, which could destroy the credibility of a professional or the reputation of an agency. This reasoning and references to knowing are revealing. Obviously no one can know with certainty during design and construction that a dam will fail; one can only speculate, raise questions, or express intuitive feelings. The story of Teton refutes the assumption that an agency would accept that kind of knowledge, unsupported by scientific arguments or technical data, especially from mid-level or lowly people or outsiders. There is evidence that early concerns expressed by the chief designer of Teton were even ignored.[23] In practice, speculation, imagination and criticism were taboo.

The Panel's main response to the lack of professional competence and taboo on talk of risk was to recommend a dam safety office be established at the top of each agency implicitly to make risk and failure discussable. Although it recognized that "in some sense safety is everyone's responsibility, [11] these academicians thought that some one person, like a teacher, must lead the discussion and show by example that such talk was permissible. This permission was expected to filter down through all levels of the bureaucracy enabling everyone to voice concerns and attend to safety issues.

Such offices at the top would also be free of competing pressures and act as independent "champions" of dam safety. [10] They would provide peer review on major projects, to counteract the impression that heretofore no one knew all about nor was accountable for large projects. To overcome a lack of communication among the agencies, the dam safety officers should report to, advise, and work together with a lead agency, possibly in the President's Office.

The guidelines should be completed as soon as possible, but again Panel members could not agree, this time on whether the guidelines should cover technical as well as administrative matters and in how much detail. Detailed technical guidelines could convey current practice and provide minimum standards for smaller agencies to compensate for the poor quality of professionalism. They would also stimulate improvements in non-federal practice. The very process of "personal interactions" to reach agreements (echoing Tschantz in his conclusions to the survey report) on technical standards might generate improvements in federal practice.

On the other hand, technical guidelines would be burdensome to keep current and might lag behind the best practices or even "freeze" them in the inertia of institutional processes. Moreover, they would "tend to become cookbooks that are followed uncritically and without real thought," making practices that follow the book "appear safe, whether they are or not." [31-33] This was their dilemma.

Federal Guidelines for Dam Safety (ICODS)issued in June of 1979 dealt only with administrative matters and left technical matters to the discretion of professionals. Even then the authors tried to strike a balance between proscribing detailed procedures that could be burdensome, expensive, and unnecessary for smaller agencies, while avoiding "saying nothing." If the guidelines were too general, agencies could interpret them to mean that they did not have to change their ways.[24]

The guidelines contained few surprises. They called for dam safety offices directly under the direction of each agency. These officers should use reasonable judgment in obtaining compliance with the intent of the guidelines throughout their organizations. The general responsibilities of other roles and offices were listed. Written documentation often in "standardized format" was prescribed in considerable detail on every stage of a project, from the interpretation of even "suspected" site conditions in geotechnical records to the reasons for rejecting alternatives in design decisions. [14]

On the other hand, they emphasized teamwork and informal communication. For instance, management should encourage interdisciplinary teams of geologists, geophysicists, and engineers to use "intellectual curiosity and an inquisitive approach" in geotechnical investigations. [21] Designers should seek the advice of construction engineers on the "constructibility" of a project and the "ease of contract administration" prior to advertising for bids on construction. [13] The agencies should coordinate budget requests and give dam safety matters "visibility" throughout the federal government. [18]

A few analytical techniques were mentioned, such as for determining "maximum credible earthquakes," without specifying details. Designers were warned that "judgment and analytical expertise" were just as important as the mechanics of analysis in selecting data to represent the range and variation of foundation and material properties. [25] Analysis, too, appeared to be something of an art.

Detailed procedures were specified for operating and maintaining existing dams and new forms of analyses for emergency action planning. The agencies should evaluate "the possible modes of failure" and the "many degrees of failure" up to a final catastrophic stage, and identify precursory signs and a range of emergency actions to be taken for each of these. They should prepare "indundation maps" for all high hazard dams. [36]

The reluctant agencies were again instructed to develop and use "risk-based analytical techniques and methodologies" with their "high potential" as an aid to decision making. The authors acknowledged "the dual problems of uncertainty in analysis and the possibility of misinterpretation by the public," and also that "loss of lives can only be quantified, but not evaluated." [11] In other words, one can count dead bodies but not put a pricetag on lives.

Institutional problems were glossed over, except that "Safety related functions and features must not be sacrificed to reduce costs, improve project justification, or expedite time schedules," [9] and design and contractor "organizations should maintain the flexibility necessary to modify the design ... and construction specifications as conditions dictate ..." [30] Here "saying nothing" disposed of problems too intractable to deal with.

On construction, the guidelines were brief and general. For instance, construction supervisors must be aware of the assumptions and intent in "design philosophy" and have the authority to suspend work when conditions differed from those anticipated until design engineers could evaluate these and determine if design modifications were required. [28]

The role of the public was less than Carter envisioned or Smith had urged. Outside interest groups "should have the opportunity to voice their concerns" on public works projects at any time. However, their concerns "often represent constraints on technical decisions in the form of local or regional political interests, legislation, perceptions of risk and hazard, environmental factors, social conflicts, etc." Agencies should develop procedures to assimilate such views and resolve problems before beginning construction. [18]

With publication of the guidelines, OSTP's planning was complete. Smith's final act was to obtain a second Presidential memo calling for adoption of the guidelines, a new lead office in the Federal Emergency Management Administration (FEMA) and another process similar to the first: another survey, this time on implementation, and reviewed by an independent panel. [FEMA]

Smith's intent was to help Tschantz, as director of the FEMA office, and with a part time secretary and little budget, to establish clout and control over the agencies, who did not welcome another survey so soon after completing the guidelines. Four months later they did report that all agencies had added dam safety offices to their organizational charts, all except the Bureau adding these duties to individuals with other responsibilities. [7] On other points, as a new independent panel observed, the agency reports contained more promises than evidence of accomplishments. [Appendix A-19]

Tschantz re-organized ICODS to advise his office; its first task was to review the agency surveys. It found evidence of chronic bureaucratic complaints, of insufficient funds, staff, and of time, of course, to implement the guidelines. Implicitly shifting responsibility to Congress, it called for legislation to deal with gaps or overlaps in the authority among different agencies.

ICODS recognized that some agencies were making costly efforts, beyond what good sense would dictate, to comply with literal interpretations of the document. For example, some were carrying out costly seismic analyses in zones of minimal seismicity. It recommended that conditions be specified for granting exceptions to the guidelines, in essence, rules for interpreting the rules. [18]

The new panel also recommended that the lead office refine the guidelines. It suggested a long list of tasks that would obviously require a sizable staff or pre-occupy the overburdened dam safety officers from their agency work. The guidelines seemed to be taking on a life of their own, demanding special attention and effort.

Obviously not understanding the reasoning or intent of the earlier panel, the new one also questioned whether dam safety officers were pre-empting responsibilities for dam safety more appropriately distributed among designers and contractors. Nor did the guidelines provide practical advice on raising consciousness of safety among personnel in all phases of a project. [Appendix A, 32]

During the final six months of the Carter Administration, Tschantz convened ICODS monthly. The dozen members faithfully attended, partly because, it was said, each feared that in his absence other agencies would interpret the guidelines in a manner detrimental to his own agency's interests. Mistrust apparently still plagued interagency relations. At the end of the Carter Administration, Tschantz resigned, "burned out," and returned to the University of Tennessee. His duties went to William Bivens, who was still promoting risk analysis.

The FDS office had been put within FEMA's Office of Hazard Mitigation and Research, directed by Charles Thiel, an effective advocate for both federal dam safety and earthquake hazard reduction. When he resigned late in 1981, FEMA support for both programs waned. Bivens eventually found a superior without specialized knowledge of dam engineering and with many other duties but willing to help the program survive.[25]

He began to build a constituency for dam safety outside of FEMA, particularly among the States, by sponsoring conferences, publishing reports, and adopting a "logo" to give the program identity. He continued to convene ICODS but now only quarterly. The National Academy of Science created a Committee of the National Research Council (NRC) on the Safety of Non-Federal Dams, to study the proper role of the Federal government vis-a-vis the States.[26] In a second phase, it planned to address technical issues, beginning with standard definitions of heretofore inconsistent terms. For instance, "dam failure" could mean anything from excessive seepage, to overtopping, to total collapse.

FEMA's promotion of risk assessment as part of benefit-cost analysis was without success. Even the NRC realized that opposing parties, affected differently by any assessment, would use different criteria to determine costs and risk. But it hoped the technique would provide a framework for organizing information and communication among opposing parties and structuring technical decisions.[27]

By 1982, only two of FEMA's six subcommittees were still active. One on communication had lapsed when funds were unavailable for two or more personnel slots recommended by the Corps in each agency to carry out formal "communicator roles." Instead FEMA should be used as a "forum." A subcommittee on research disbanded after publishing a report already becoming obsolete on projects completed and underway. Another on training issued a catalogue of agency programs open to outsiders, but bogged down on the issue of liability. It feared that if it even issued videotapes for training operators and inspectors of a dam that later failed, it might be caught in a chain of liability. Because of this issue, some agencies also resisted installing instruments, fearing that they might be misread.

One active subcommittee on emergency preparations pointed out proudly that TVA was enlarging spillways on several 50 year-old dams to meet modern standards. That agency was also in the forefront in preparing emergency evacuation plans. A simple formula had been discovered for estimating downstream "inundation areas" with a hand-held calculator as an alternative to more complex, debatable, and time-consuming methods. TVA reasoned that it was better to get on with planning and evacuate a few people unnecessarily than have no plans at all.

The most active subcommittee had become one on technical guidelines, after it had decided to work only on items on which its members could agree. It had, for instance, tabled work on standards for spillway design after the Corps refused to accept turbines and pinstocks to handle excessive flow, as other agencies did, claiming that these sucked in debris and soon became damaged and ineffective. After hearing arguments among peers, it was trying to build national standards "by consensus." "Maybe that's not the right way," Bivens said, "but it's the best we can do."

The technical group was seeking a single method for interpreting watershed data to define a standard "project flood" for design, since heretofore the Corps, TVA, the Bureau, and the SCS had used different methods to arrive at different designs for any given site. It had also finally convinced the Bureau to use National Weather Service data for calculating the "maximum probable flood" rather than its own data, as it had done for years. To Bivens this was a sign that the Bureau was "opening up" again after it had "closed up" at the end of the OSTP program.

Bivens had his own diagnosis for Teton's failure: in an agency where people talk only to each other, they become convinced that they are the only ones who know anything about a subject. The lesson he drew was "Don't become drinking buddies with your crew." His message were ambiguous. A small elite group could set national standards by talking among themselves; an agency should be open to criticism from outside; but within it, engineers should not fraternize with lowly construction workers. Horizontal boundaries and top-down control must be maintained.

ICODS was beginning to relax some technical standards in the realization that achieving these would cost as much as the Gross National Product. Meanwhile, Bivens was learning to do without money as he tried to keep the Federal Dam Safety Program alive.

Evaluation. Was the program a success? Tschantz, reflecting on it later, from the distance of Tennessee and time, thought so. Top officials in the federal agencies were still talking with each other and working together on issues. He was less certain that the momentum would continue after the most active individual members of ICODS retired.

But, for the moment, he was optimistic in part because the States were slowly moving forward on their own programs, prodded by private consulting engineers interested in designing improvements for older dams. He also saw a new attitude, a kind of revolution in the thinking of many members of the engineering community, who were now talking about dam safety, where a decade before only a handful would admit that dams do fail.

Outside observers were less sanguine. The Bureau has not significantly changed its ways. All agencies seemed to be preoccupied only with more paperwork, seldom read or analyzed. The old overconfidence remained in offices and at projects far below the agencies' dam safety offices.[28] Whether the new attitude and a sense of responsibility would filter down, as Tschantz and the first panel hoped, remained to be seen. The guidelines seem to have only added more bureaucratic requirements to an already overburdened technical staff.

One thing is certain. Should a federal dam fail now, the cause would be easy to identify. One person alone could be blamed. As one OSTP staffer sarcastically remarked, the new guidelines were "Draconian," like a Hammurabi Code under which, should a federal dam fail, the dam safety officer and a dozen others in the agency should publicly be put to death.[29] Even a lesser punishment would provide a symbolic sacrifice to appease the public but contribute little to learning and leave intact the institutional arrangements that seem at the heart of the problem.

The National Dam Inspection Program

Before concluding this story, one should consider the other 65,000 large dams in this country, outside of the federal government but of major concern in OSTP. A series of events in the early 1970's focussed public attention on the hazard of these. In February, 1971, an earthquake damaged the lower Van Norman Dam in California and put at risk some 80,000 people in the valley below. In February of the next year, a poorly engineered mining dam on Buffalo Creek, West Virginia, collapsed, killing 125 people and causing $50 million in damages. Then a freakish, highly localized rainstorm caused a Rapid City, South Dakota, dam to rupture. In June of 1972, Hurricane Agnes brought floods that threatened a number of dams in the Northeast.

Congress responded promptly to the latter events and introduced a bill; two weeks later without public hearings, it passed the National Dam Inspection Act. This directed the Corps of Engineers to make an inventory of all sizable dams under the jurisdiction of the States and to inspect the most hazardous ones. It excluded those built by TVA, the Bureau, the IBWC, under FERC, and most of those of the Corps.

President Nixon reluctantly signed the bill into law on August 9, 1972 but wrote; "I think the particulars of this bill are most unfortunate, for they depart from the sound principle that the safety of non-federal dams should primarily rest with the States,[30] Under the Constitution, state governments are responsible for protecting the health and safety of their citizens, and most had passed legislation regulating dams built and owned by all non-federal entities. But the legislation was often inadequate, or the States lacked funds or technical staff to enforce these laws.

OMB subsequently denied the Army's request for funds for inspection, apparently fearing this would open a Pandora's box of State requests for repairs. The Corps then asked the States and some federal agencies to list and submit basic facts on larger dams within their jurisdictions, compiling the data into a National Dam Inventory. The initial tally of less than 50,000 dams nationwide, of which approximately 2,000 were federally owned, over 3,000 more were on federal lands, and over 1,500 subject to federal licenses, has continued to increase, not only as dams have been completed, but also as the inventory became more accurate, since both States and some agencies' records had been incomplete.[31]

Although the Corps had initiated the legislation -- some say in the hope that the program would compensate for its decreasing opportunities to build new dams[31] -- through 1976 it did little more in a practical sense than prepare the four volume inventory and some recommendations to Congress. It was also said that the Corps backed off out of fear of liability.[32]

Meanwhile, many professional dam engineers thought that the cluster of incidents in 1971 and 1972 had been unusual. The U.S. Committee on

Large Dams (USCOLD) marshalled data to show that through 1972, of 4,918 dams in this country over 45 feet in height, only 74 failures had been recorded. Of another 274 near accidents, only 104 would have affected public safety; remedial action was taken before the structures collapsed.[33] Moreover, the average annual failure rate was declining: from 1900 to 1939, the rate averaged .0027 failures per dam per year; from 1940 to 1972, it had dropped to one fourth or .0007. [FCCSET, 12] At this rate of decline, dam failures would soon become a thing of the past.

In 1973, in anticipation of the inspection program, a small group of engineers began to meet annually under the auspices of the American Society of Civil Engineering (ASCE) and its Engineering Foundation, to discuss aspects of dam safety. Bruce Tschantz emerged as a national expert on State programs.[34] In 1976 the Corps finally proposed a general set of inspection guidelines that generated considerable discussion and raised concerns about the liability of private consultants. P.L. 92-367 stated that no action or failure to act under it should be construed as creating any liability for recovery of damages from the federal government or its employees, but court decisions had made the law somewhat ambiguous particularly on the liability of consulting engineers. Many felt that Congress should adopt special legislation to protect them.[35]

One man, who had had an unfortunate experience in this matter, described how excessive liability could put a professional in a difficult position.[36] For instance, if he used his best judgment on an old dam, which had successfully weathered time and seismic tremors and served a valuable function, such as for municipal water supply, he might be loathe to condemn it, especially if it might remain sound with some remedial work. If he ran scared and condemned the structure, his decision could be disruptive and costly for the local community. Ultra-conservative inspections across the country could be disastrous.

On the other hand, the public, unaware of how much judgment was required, expected a dam to be perfectly safe, especially after it had passed inspection. If it later failed and they suffered damages, their lawyers would seek maximum compensation for them in the courts. Under common law, the owner is usually liable for a failure but seldom carries enough insurance to pay more than minimum damages. After that, operators, constructor and original designer are seen as links in a "chain of responsibility" going back over time. But many may be long gone or unable to pay for the damages. The lawyers may view the inspector as the most recent link in a new "chain of liability" and turn on a responsible professional, who had tried to perform a public service but made a minor error in judgment or overlooked a detail, and accuse him of being negligent, fraudulent, or worse. Not only would his professional reputation suffer, but his firm's or personal assets would be in jeopardy.

Under the law an engineered structure need only meet the standards in the state-of-the-art at the time that it was built. But the courts might interpret inspection guidelines very differently from what the framers intended and expect an inspected dam to meet current technical standards, or view standards in guidelines not as maximum but as minimum ones, expecting the professional to have used even higher professional standards.

For such reasons, many competent engineers were reluctant to participate in dam safety programs and were tempted to leave inspections to the incompetent, who lacked experience and judgment, or to the foolhardy or the happy-go-lucky, who did not even care about the quality of their work.

Another concern was how a single set of guidelines could be uniformly and fairly applied to tens of thousands of dams built at different times under varying states-of-the-art, and with different histories of maintenance and stresses, and existing under different climatic, hydrological, geologic, seismic, and other conditions.

Leaving inspections to the most competent professionals presented other difficulties. The number of trained professionals was limited and determining who among them was competent seemed an "insurmountable" problem. Contracting with private consulting engineers would also be time-consuming and costly.

An alternative might be specific guidelines for each high hazard dam, such as the Bureau was preparing, for use by less advanced professionals. But these could be a "dangerous tool" if treated as "cookbooks" or applied "by rote," leading to overlooking significant clues and complacency. Moreover, if individual guidelines had to be prepared for thousands of dams, the program could not be carried out in a timely way.

A third alternative, barely discussed, was a program in California with no guidelines at all. Forestry department field workers kept close watch over dams in their regions and developed an "intimate knowledge" of each. They and their supervisors would "talk with each other" frequently and discuss any changes that might warrant closer, more professional attention.

After Teton's failure, the pressure for national inspection increased; Tschantz had helped the cause in Congress and OMB. When a private dam collapsed on the Toccoa River on November 6, 1977, killing 35 Bible students and their families in Carter's home State of Georgia, the President demanded immediate action. In less than a month, by December 5, one dam in each of the 50 states had been inspected. During the next four years, the Corps provided technical assistance and funds to States to inspect almost 9000 "high hazard" dams, defined as those whose failure would result in "more than a few" lives lost and "excessive" economic losses.[37]

The guidelines finally devised by the Corps were an ingenuous compromise among the alternatives. They offered a checklist of general items, which were then to be refined into specific lists for particular dams. Inspection was also to be done in stages, first by a thorough review of existing engineering data and a visual inspection, and then if conditions warranted, a second phase involving more sophisticated technical analyses. Two types of personnel were also involved: less professional people in the first stage but under a registered engineer who would be in charge of phase II, if required. Most of the subsequent 9,000 inspections involved only Phase I: a review of the records that could be found on design, "as-built" construction, and the operational history of the dam, and then a visual inspection.

The general checklist for inspecting embankment or earthen dams is not esoteric, especially when translated into simple English, but understandable even by a layman. For instance, one should look in the streambed below the structure and on its lower face for "sinkholes" or depressions; these may indicate settlement. Also look for "boils" or springs, which indicate excessive seepage. Look up over the slopes and along the crest for surface cracks or irregularities in alignment, which may indicate that the structure is sliding on its foundation and potentially instable. Check the upstream slope for gullies and wave-formed "benches" or notches, signs that waves in the reservoir have worn away the outer protective layer; excessive water could be seeping in at these points. Around the banks of the reservoir, masses of wet, highly saturated soil indicate incipient landslides, which could reduce the capacity of the reservoir or even cause overtopping.

Intimate Knowledge. What is remarkable about this list is how readily ordinary people might be able to notice these phenomena. Anyone could observe a spring or avoid a soggy mass of earth about to slip into a reservoir, but might take these for granted as normal. On the other hand, people who have regularly walked around an earthen structure or fished in the reservoir may one day sense something new and different, a change. They may notice with mild surprise a crack, a surface depression, or a notch on the reservoir side, which they never saw before. They can check that this is new by reflection, comparing what they see with images from the past.

Like the intimate knowledge of dams by California foresters, those who frequently visit a dam repeatedly observe, however idly, many details, and develop a kind of knowledge of that particular artifact through deep familiarity over time. They know in a sense analogous to the way we say we know a spouse or a special friend. They may wonder about a change but be unaware of its significance. Yet it could be an early warning of impending weakness in the structure.

Even if they knew their observations to be significant, ordinary people are unlikely to report them to the dam's owner or local officials. They are more likely to feel that this is not their responsibility but that of others more knowledgable than they. Even if they did report, they would probably be rebuffed as ignorant laymen.[39]

Those who have this kind of intimate knowledge are outsiders to the formal institutions that claim to bear exclusive responsibility for dam safety. But people who live below a dam have more at stake than engineers, who might lose money and reputations in the event of a failure; local residents could lose their property or even their lives.

People willing to speak up about their intimate knowledge take the risk of being humiliated, but there is also much to be gained, if officials are willing to listen, investigate, and confirm their findings. If a layman's concerns prove groundless, he should at least be commended for a sense of responsibility and informed why his observations merit no concern. In this way responsible citizens could learn to refine their judgments. But if something is found to require remedial work, our observer should be treated as a local hero of sorts. He could become an example for others to emulate at that site and across the country. One can imagine other local people forming small quasi-official cadres to monitor particular dams, learning among themselves what signs to look for and to report. Undoubtedly they would come to advocate good maintenance and form a constituency for timely repairs.

In this way, dam safety inspections could be transformed from a costly, one-shot, top down government program, the result of a time-consuming formal process of devising and managing elaborate procedures, into a continuous operation, at little cost to the taxpayer. Local volunteers, working in small groups with owners, officials, and engineers could provide a new model for the management of this technological risk.

The Corps completed its inspection program by October, 1981. It reported that over 2,900 or 1/3 of the high hazard dams inspected did not meet federal standards; repairs had been completed on less than 5% of these.[40] The fifty States are now responsible for monitoring impoundments within their jurisdiction, but with the end of federal assistance, most had cut back on their dam safety programs.[41] While the NAS committee was seeking to define a more positive federal role on non-federal dams and the small FEMA office simply trying to survive, the resources of intimate knowledge were going unrecognized and untapped.

Other Kinds of Knowledge

Awareness of intimate knowledge at existing dams leads one to wonder about other kinds of knowledge available at dams under construction. That such existed at Teton was apparent in testimony before the House Subcommittee. Further evidence was buried in Appendices of reports on the technical investigation. Before considering this, one needs to understand the physical and sound context in which construction took place.

The foundation at the Teton site was vast, extending over more than 60 acres. Some was flat riverbed, but most of this surface was steeply sloped, at 30 degree angles up each side of the cutoff trench across the riverbed, at approximately 45 degrees up the two banks, where the bases of the key trenches were terraced to facilitate work, and about 60 degrees up the sides of these trenches. These surfaces were primarily of irregularly fractured rock.

The Bureau supervisor's officer was high on one bank, with a panoramic view of more than 3000 feet across the canyon and over 400 feet down to the deepest part of the foundation. It was connected to specific work centers by telephones, with lights to signal incoming calls. [IRG G-19] The site must have been extremely noisy, during excavation from initial blasting, later from drilling deep grout holes, and throughout from heavy machinery, and echoing off the steep canyon walls, must have made ordinary conversation impossible.

The construction season was only about six months, from May to November, when the necessary water was not frozen. Three shifts worked around the clock, except that core material was not laid during the graveyard shift, allowing time to maintain the heavy equipment. Continuously during the other two shifts, trucks brought in and dumped construction material or fill, which was spread, watered, and compacted by "twelve passes of standard tamping rollers" into 6 inch layers. Quality control personnel then tested these for density and moisture. Thus, half a foot at a time, the structure gradually rose from late in 1972 to early 1976 to a crest more than 450 feet from the bottom of the key trench.

Grouting at Teton. As soon as the site was excavated, the grouting subcontractors began their work. To create the curtain, they drilled holes as deep as 310 feet and generally twenty feet apart, in three parallel rows, in conformance with the designer's detailed specifications on depths and angles. When a set of holes was drilled, the grouting crew pumped into them a basic mixture of about eight parts water to one of concrete, filling these holes incrementally in 20 foot stages. Pressure was required to force the grout past loose rubble that might block subsurface openings and to spread it through fractures over a radius of at least 10 feet so that it would intersect with grout from adjacent holes and form an impervious curtain.

The grouting program was carried out through a five-tiered hierarchy under the Project Engineer. Reporting to him was a special civil engineer from the Bureau, who supervised those third in command, three field inspectors, one for each shift. These in turn monitored the subcontractor, a firm owned by the three brothers, who served as foremen on each of the shifts. They oversaw the fifth and bottom level, a crew of 18 to 27 men. [IRG G-13]

Both the bureau and the subcontractor maintained extensive and duplicate records, describing the complex history of each hole in minute detail on individual sheets. Logbooks recorded the grout takes, water test information, surface leaks, etc., at each 20 foot stage. Drill sheets noted time of drilling, rock hardness, color of water returns (from water tests), and even the serial number of the drill bit used.

Pump operators kept running records of the batches and mixes of grout. [G-15]

Each morning the field inspector presented these documents and his own summary of the work on the three previous shifts to the Bureau supervisor. He in turn would plot the data on a plan and composite profile drawings for each row of holes, then overlay these and compare the data from hole to hole and row to row, in an effort to check that no gaps had been left. He could specify, and often did, that the men should drill and grout additional holes. [G-17]

The specifications required adding calcium chloride or salt to the grout to force it to set quickly before travelling too far and also to keep it from freezing at the beginning and end of each season. This procedure had been devised after an extensive pilot grouting program. During testing at Teton, workers encountered rock so highly fractured that water and grout, used to test the extent of sub-surface voids, would sink into holes without pressure, as into a bottomless pit. On numerous occasions, under pressure, grout would resurface as far as 300 feet from a hole. [IRG 62, 69]

To prevent excessive travel, the Bureau experimented by adding up to 10% $CaCl_2$ by weight of concrete. The use of up to 3% salt was standard practice. Shortly after construction began, the grouting subcontractors, apparently with some authority in this matter, voiced concerns and the maximum was limited to 8%.[42]

The Bureau tried to devise precise standards for the proportions of salt under various conditions. The workers analysed the effects of such factors as the temperature of water, sand, cement, and air and the distance from the mixing plant to the hole and the rate of "take" or flow of material into the hole. But they could find "no precise criteria to predetermine accurately" how rapidly the grout would set with a given amount of salt under the variable conditions. They finally decided to correlate the amount of salt initially to the temperature of the mix on a thermometer at the pump as the most "feasible" criteria. This was certainly the most readily observable and quantifiable criteria. [G 125-127]

But still grout would sometimes harden before it reached the bottom of a hole and have to be drilled open. At other times it would set in the mixing bins before it reached the pump and have to be softened with water. The Bureau had specified that grout should travel no more than 100 feet from a hole (how compliance could be confirmed is unclear) and had printed on the forms for each grout hole that "reasons for 'Waste' must be explained in detail." [C-18] The grouters tried to strike a balance between "slugging the hole" and wasting grout. [G-126]

Procedures were specified for filling each hole. First the crew would pump in water for five minutes, measuring the volume. If the hole took water at the maximum pressure of the pump, 250 cubic feet per hour, the basic mix would be thickened with either sand or salt. Sand would be added if grouters suspected an open cavity lay below, but salt would be added if they sensed that water was flowing into narrow fissures. Grouting would then begin. When the pump began to operate under pressure, the grouters would decrease the amount of sand or salt.

After filling 20 feet of a hole, the Bureau required the crew to wait for a specified time and then pump in water again as a check that all voids had been closed. Investigators later feared that this procedure may have washed out some of the grout. [C-6] Then raising the pipe through which the grout was inserted, the crew continued on other stages until that hole was filled.

The Bureau supervisor checked the work, but it was said that the field inspectors made the critical decisions. The behavior of each hole and even of different stages of a single hole, could vary widely. The grouters determined what mix to use and when to change it based, it was said, on the rate of take, drilling characteristics, pumping pressure, but most important on "intuition" or the so-called "feel of the hole". [G-18]

<u>The Grouter's Feel</u>. What is this "feel of the hole"? Obviously no one can directly observe the conditions of rocks 300 feet beneath the surface. Neither the grouter nor anyone else can know with certainty where the grout is travelling and settling or where voids remain. Nor can anyone evaluate the grouter's work except rarely, when, as in the case of Teton, a dam fails and part of the work is uncovered or exposed

in sample cores. Thus everyone must have faith in the grouters' mysterious "feel." The integrity of a dam may depend upon it.

Obviously the grouter is a skilled craftsman. But a craftsman's work can usually be seen and judged by others. The grouter's "feel" entitles him to be called an expert of sorts or an independent professional in that he alone can claim to know how to diagnose and treat a particular situation and is solely responsible for the results. Not even his peers can challenge his specific knowledge, unless they participate in his direct experience. What sort of knowledge does the grouter possess and how does he acquire it?

This "feel" seems difficult for even a grouter to articulate in words. What he knows better than anyone else is not amenable to verbal abstraction or open to inspection or testing. It certainly cannot be measured or represented by rules and formula. He does not learn this feel in a classroom; it certainly is not taught there. He learns it only by direct "hands-on" experience in specific situations in the field, probably under the guidance of a master craftsman. Procedures may be applied around its application but apparently do little to validate the quality of the work; they may even jeopardize it, as water testing possibly did.

The art of grouting seems to require continuous attention to a myriad of subtle qualities, such as the "hardness" of the rock or the "color" of the water flowing back. But the meaning of this data, of these clues, cannot be abstracted from their object nor do they seem independent of one another or of the larger context of the particular site and situation. Just as objective criteria could not represent the time that grout will set with added salt, the grouter's knowledge cannot be based on analysis. His feel combines visual perceptions with kinesthetic sensations and data from other senses. These senses may be sharpened with time much as blinded people learn to hear sounds which sighted people miss.

We posit that the grouter builds up a repertoire of strategies for different kinds of holes in different situations. In that sense he acquires a kind of general knowledge. But since every site, each hole, and even different stages of a single hole, are unique, he cannot depend upon formal models, general rules or recipes. If he settles into a mindless routine, the quality of his work may suffer. He must constantly be alert to the "back-talk" of the specific and immediate situation.[43]

Analogies to the grouter's kind of knowledge are found in many forms of art. The sculptor in stone, for instance, combines data from many senses in his skill in working with his hands and through his tools. In trying to shape a particular piece of rock into the form he has "in mind," he continuously studies its specific texture, grain, and potential lines of fracture, from various angles, near and far. Keeping in mind what he has learned, he must also imagine what lies ahead, unseen, within the material. That particular object speaks to him, tells him what he can and cannot do, constrains and offers opportunities for his actions.

The sculptor builds up expectations, to be tested, of how the material will react to each tap or blow, a kind of theory or predictive knowledge, much as when scientists say they know a particular thing. In this way incremental actions and hypothetical knowledge, images and substance, are synthesized and continuously adjusted to one another. But at any moment the material may surprise him and force him to modify his formal idea or abandon it entirely. He lives with that uncertainty.[44]

The grouters, working under contract at Teton, had no options to talk back to the designers or abandon their work. If their knowledge and skill failed them, more could be lost than a creative idea for a work of art. Studies have shown that foundation failure has been the most frequent single cause of dam collapse.[45]

<u>Surface Treatment at Teton</u>. The grouters' "feel of the hole" is not the only kind of knowledge essential in securing a foundation. The surface of the foundation, especially under the core, must be adequately sealed to prevent hard-packed silt from eroding into large fissures, leaving weak spots or voids through which water can later tunnel. The surface is usually treated by removing loose material, cleaning out crevices in the rock with jets of air or water, and filling voids with

silt, sand, or grout. Incidentally, the grouting subcontractor did not participate in the surface treatment at Teton.

Surface grouting is generally done with "slurry grout," a very thin mix of cement, water, and often sand. But the work is specialized, as various terms attest, such as in "gravity," "broom," or "bucket" grouting, or with special forms of material such as "slush," "shotcrete," or "dental concrete." At Teton, the specifications called only for generalized slurry grouting as necessary.

As was said, the Bureau was subsequently criticized for not prescribing more detailed procedures. It justified itself on grounds that only field personnel can directly observe the variability of fractures and patterns of joints. They can decide what openings to treat and how to treat them better than if they simply follow "an arbitrary set of rules devised in the Denver Office." [IRG G-100] But the expert investigators argued that the field crew could not make valid decisions without understanding the theoretical principles on which such treatment must be based. [C-6]

As we know, the surface at Teton was extensive and extremely variable. The crews worked night and day on all three shifts first on the flat base of the central trench and then on the terraced levels of the key trenches. They generally worked about five feet above the level at which the fill was being laid and compacted. The Office was said to set policy; for instance, after clean-up, an inspector would look over an area and spray red paint around the crevices to be treated. The crew would take it from there, developing their own procedures with plenty of room for individual judgment. [C-15]

At times, a crew treated up to ten voids per shift but at other times worked on a single void for more than eight hours, as the level of fresh fill rose around them. On a few occasions they poured grout through pipes into voids remaining under overhanging rock after the fill was compacted. Obviously they were always under pressure not to hold up the work on the core, a factor that must have influenced their judgment on how much and what kind of treatment to perform.

The Bureau was criticized for not setting criteria at least on the minimum width of crevices to be treated, such as 1/4 or 1/2 inch. But the men used more than spatial measurements. For instance, if a large void appeared to be tightly packed with natural silt, it needed little treatment. A sense of touch was also critical as when, frequently, "one could hold one's hand over a hole and feel cold air." [C-15]

Disaggregated Knowledge. The Bureau's justification seems appropriate: the workers had to use more knowledge than could be put into specifications. The designers, with data abstracted from sample cores had only indirect, partial, general knowledge of what that crew would encounter and grossly underestimated the difficulties of site conditions. The Project Engineer from his high vantage point depended largely on the observations of others. The designers who had come periodically for a day or two to inspect the construction site could know even less than the Project Engineer. Only those who actually did the work could know the kinds of treatment required for specific conditions.

As these workmen interacted with unique and seemingly trivial details, they directly observed and sensed each specific hole, sometimes for eight hours or more, and acquired more complete understanding of the heterogeneous conditions than anyone before or after construction. They knew foundation conditions in the only sense in which they really were known, the only way that real knowledge is possible.

Unlike that of the grouters, their kind of knowledge was spacially limited to superficial characteristics of each crack; they had less sense of what lay beneath. Moreover, since conditions would change under pressure from the fill and with seepage, their knowledge was also temporally limited and would soon be obsolete.

Moreover, no single workman knew all about all of the surface conditions. Each man on each shift -- no count was given of how many worked on tens of acres on three shifts over several seasons -- possessed only a small fraction of the total knowledge that they collectively possessed. Given the noise, the pressure of time, and the organization of separate shifts, it is doubtful that the men communicated much or shared, compared, or combined their individual impressions, except perhaps in a local bar on weekends. From a larger perspective, this kind of knowledge could be called a disaggregated "feel for the whole." But even after the failure, the investigators never tapped nor tried to aggregate these diverse impressions, which remained fragmented and dispersed.

Passive and Critical Knowledge. Still another kind of knowledge emerges from the description of events at the site. At an elevation of 5200 feet, where the water later tunneled, the crew received orders "from above" to stop surface treatment. Those interviewed later did not agree on whether the rocks at higher elevations were less or just as fractured as those below. Some members of the surface treatment crew were apparently bewildered by this order and tried to construct an explanation. One reason "floating around" was that the stresses high in the dam "would be low enough to allow quitting," indicating that these workers understood technical matters in at least a general way.[IRG-C-15]

Of course, they had no formal training in the theory and techniques of dam engineering. But they understood the physical principles well enough to appreciate that the weight and horizontal force of water in the reservoir would be less at the top than at the base of the structure. They accepted this as a sound hypothesis or reason behind their orders.

In so doing, they demonstrated a kind of passive knowledge, as when people understand a language or appreciate good music, but do not have the skill or expertise to read or write it. Such passive knowledge often carries with it critical ability. An amateur may be able to differentiate between a masterful and sloppy bit of work and support his judgment with reasonable arguments and plausible explanations almost as well as a professional. But those who lack formal academic credentials or status in special institutions are seldom credited with such knowledge, or heeded.

We do not know if these workmen had a passive understanding of other matters or made other critical judgments about the work of their superiors, but we do know that if they had continued grouting, this dam would probably not have failed in the way or as soon as it did. Moreover, since the special investigators talked only to those at the top of the hierarchy of construction workers, these various kinds of knowledge became buried in the structure and lost in individual memories.

Summary and Conclusions

This has been a story about one type of engineered technology, dams, intended to control the range of natural atmospheric events, of droughts and floods, and manage water resources for social benefits. It described the long social evolution of a large public works project, from a gleam in the eyes of new agency engineers to an idea shared by local people, who pushed it up through levels of government, until Congress transformed it into a technical project for agency engineers. For a decade, the design was pieced together with sample data from the site, rules of thumb, and examples and lessons learned from other sites. About five years later, when the idea was almost a reality, the structure collapsed.

Investigators from different institutional settings sought the cause of the failure, but all ignored the slow social evolution in the distant past and the social aspects of construction in a complex physical reality. Diagnoses of the problem and prescriptions to prevent further disasters diverged sharply, particularly between technical experts and bureaucratic managers. A remedy was chosen that fit the setting, more administrative rules to control technical decisions throughout bureaucracy. OSTP pre-empted a group of top level bureaucrats to make the rules, but it saw the development and use of the new technique of risk analysis as a remedy, especially to the momentum of Congress to construct.

The agencies feared that risk analyses would unduly alarm the public. They were reluctant to admit problems except those that could be blamed on others: inadequate data from scientists for design, laws and regulations overburdening engineers, and quality control weakened by specialized middlemen. Admittedly, inspections were better in concept than practice, funds limited for maintenance and repairs, and emergency plans were rare, but none of these were necessary when engineers knew how to build permanent dams.

Outsiders to bureaucracy criticized this arrogance and offered another view of engineering, in a different physical and social reality. Each dam is built at a unique site, acquires its own personality, and ages in a dynamic environment. Irreducible uncertainty persists about geologic and material conditions, even with instrumentation and quality control procedures. Over time engineering principles have been devised, often by learning from failures, but engineers' knowledge and control is limited, particularly in the complex context in which they must work. Therefore, like artists, they must use skill, imagination, and judgment, gained through experience, to synthesize combinations of physical factors with non-technical considerations in their designs and specifications. In essence dam engineering is a form of art.

The tension between this and the mainstream view of engineering echoed throughout this story. It was seen in GAO's opposition to the use of discretion and judgment at any level, the demand for objective criteria, and implicit expectation that engineering would become a predictive science. It appeared in the agencies' contradictory arguments against risk analysis: both disdaining them because they required judgment, fearing that they would replace professional judgment and artistry.

When engineering was linked to bureaucracy, this conflict divided the Panel, for instance, on whether to leave inspections to managerial discretion or to add more bureaucratic rules. The conflict emerged in Biven's embarrassment at setting technical standards by consensus when he expected some "right" or more scientific way. It emerged in the debate over whether to use competent engineers or consistent rules for national inspection guidelines. The guidelines sought to balance both views, in encouraging critical judgment in assessing data, interdisciplinary teams in site investigation, and informal communication between design and construction. But they also spelled out general roles and rules.

Parallels and linkages in institutional arrangements exacerbated problems beween bureaucracy and engineering. Both bureaucratic compartmentalization and engineering specialization impeded communication and led to discontinuity in design decisions. Contractual arrangements distracted attention from the unexpected in the physical reality and diverted energy into legalistic disputes. Inflexible budgets and fixed front-end financing spurred the momentum to construct, inhibiting new information from the site flowing up the bureaucracy and back into the design. Bureaucrats use rules and engineers use formulas like recipes, displacing judgment and creating false impressions of safety.

The outsiders realized that both management and technical lessons were best learned in free and open discussion, such as occurs in an academic organization. Inadequately understanding the nature of bureaucracy, they expected new attitudes to "flow down" from competent engineers at the top. Instead, their remedy only elaborated bureaucracy and compartmentalized dam safety into an identifiable component. a separate responsibility of only a few at the top.

FEMA saw no need to protect artistry as it expanded technical standards and sought consistent definitions, as if to transform a once taken for granted quality of dam safety into a new and specialized field. Meanwhile, management rules took on a life of their own, demanding more staff to interpret them and to spell out exceptions and more paperwork to demonstrate conformance, while the lead agency fought a bureaucratic battle for survival.

Finally, the threat of liability from outside of both bureaucracy and engineering was paralyzing the sense of professional responsibility. This fear justified suppression of talk of risk and failure, inhibited competent engineers from inspecting old dams, and stopped FEMA from issuing training guides. Ironically, legal experts in another institutional setting promote this threat on behalf of lowly outsiders who have accepted the engineers' claims of certain knowledge and control and who, after an accident, will throw these claims back at them, holding them accountable. Yet the guidelines had dismissed citizen concerns as "constraints" to be removed before beginning design and construction.

These men at the top treated engineering as if it were performed by heads decapitated from bodies. Their model of engineering artistry ignored the skill and artistry of others below and outside and failed to address how institutional arrangements of both bureaucracy and engineering suppress other kinds of knowledge of the local physical reality.

Ironically, potential victims and lowly workmen possess the knowledge and ability to act in ways that seem to offer opportunities to compensate for the limited number of competent professionals and cost constraints on elaborate programs, for instance, in inspecting older dams and even constructing new ones. But such opportunities would only become reality if old institutional arrangements were modified and new social ones formed to foster continuous attention to local details and aspects affecting safety at all levels.

Finally, we offer our diagnosis of Teton's failure. With Perrow, we see dams as made up of tightly coupled component parts. But they also appear to have interactive complexity, internally and with their environments; only in the analytical minds of designers are interactions simple and linear. Indeed earthen dams appear to be less like mechanical systems and more like living organisms.

Moreover, in contrast to the divergent diagnoses of experts and officials, the "cause" of Teton's failure seems traceable to a combination of both physical and organizational events, to the unexpectedly heavy snow melt and the failure to deliver one outlet part that preceded that failure. The first is traceable to inadequate knowledge in the domain of science of normal variations in snow precipitation. The second factor lies in the domain of bureaucracy and could be considered a small but normal "administrative glitch."

If the outlet had been open, it would have released the flow downstream; if the runoff had been as expected, the lack of the outlet would not have mattered. But the coupling of these two conditions made the reservoir rise, forcing the engineer to request a change in specifications to allow it. This was a mere bureaucratic formality; had it been denied, the engineer would have had no choice but to break the rules. Nor did the workers, who felt surface treatment should continue, have much choice under the organizational arrangements; they had to leave the upper portion of the embankment vulnerable to excessive seepage and piping.

Perrow simplified the nature of earthen dams to complete a formal matrix. Much as Congress used its theory of momentum to selectively attend to evidence and ignore fine bureaucratic distinctions, so Perrow accepted a simplified engineering model of dams to complete his theoretical model. For such reasons, both erred in prescribing a remedy of more rules and safety regulations.

The question now is whether these findings about particular dams have more general applications and broader implication, when we look at a program to protect against, prepare for, and predict a natural hazard.

NOTES

To simplify references, frequently cited sources are abbreviated here or in the text, where page numbers of obvious sources will be inserted in [], as follows:

"CGO": U.S. Congress, Committee on Government Operations. Teton Dam Disaster. House Report No. 94-1667 on Hearings of Subcommittee, August 5, 6, and 31, 1976.

"FCCSET": Federal Coordinating Council for Science, Engineering and Technology. Improving Dam Safety. Washington, D.C. November 15, 1977.

"FEMA": Federal Emergency Management Agency, Office of Mitigation and Research, Early Progress to Implement the Federal Guidelines for Dam Safety and Recommendations to Improve Federal Dam Safety Programs. Washington, D.C. July 30, 1980.

"GAO": Controller General's Report, Actions Needed to Increase the Safety of Dams of the Bureau of Reclamation and the Corps of Engineers. U.S. General Accounting Office, Washington, D.C., June 3, 1977.

"ICODS" Interagency Committee on Dam Safety (ICODS). Federal Guidelines for Dam Safety. Federal Coordinating Council for Science, Engineering and Technology, Washington, D.C. June 25, 1979.

"IRG": U.S. Department of the Interior, Teton Dam Failure Review Group, Failure of Teton Dam: A Report of Findings. Washington, D.C., April, 1977.

"OSTP": Office of Science and Technology Policy, Federal Dam Safety; Report of the Independent Review Panel. Executive Office of the President, Washington, D.C., December 6, 1978.

"Panel:" Independent Panel to Review the Causes of the Teton Dam Failure, Report to the U.S. Department of the Interior and the State of idaho on the failure of Teton Dam. Washington, D.C., December 1976.

1. CGO, 8

2. The history of Teton is derived from CGO, GAO, IRG, and the Panel, passim.

3. Encyclopedia Britannica, 1966 ed., s.v. "Idaho."

4. GAO, 5-22, compares the elaborate sequences of formal procedures and practices of the Bureau and the Corps of Army Engineers.

5. Robert B. Jansen, Dams and Public Safety, A Water Resources Technical Publication: U.S. Department of the Interior: Water and Power Resources Service, Denver. 1980, 143.

6. U.S. Corps of Army Engineers, Chief of Engineers. National Program of Inspection of Dams. Five Volumes. (Washington, D.C., Department of the Army, 1976). Vol. I, Definitions; Vol. III, Inventory.

7. Jansen, 1-79, gives a concise history of dam building.

8. Elting E. Morison. From Knowhow to Nowhere: The Story of American Technology. (New York: Basic Books, 1974.) "The Works of John B. Jervis," 37-67.

9. Bruce A. Tschantz, U.S. Congress, Committee on Government Operations, Federal Dam Safety, 68-86. Hearings of Subcommittee, March 15, 17, and June 30, 1977.

10. The World Almanac: the average annual deaths from dam failure in the United States was 36, accounting for only .002 percent of deaths from all causes.

11. Encyclopedia Britannica, 1966 ed. s.v. "Dams."

12. FCCSET provides a list and "An Overview of Current Agencies Responsibilities and Practices," 14-27.

13. Corps of Army Engineers, Brigadier General Drake Wilson, Deputy Chief of Public Works. CGO 15.

14. Jansen, 211-213.

15. Eric Van Marke interview at MIT, May 28, 1982.

16. Tschantz in U.S. Congress.

17 Philip Smith in an interview in the White House, December 1980.

18. Bruce A. Tschantz during several meetings in 1980 and in March 1982 provided insights into this program. Attributions will not always be made because his remarks were often given in confidence but confirmed by others.

19. "Summary of Third Meeting of IDOCS, July 28, 1977" in FEMA files, report on meeting with Eliot Cutler, OMB, July 27.

20. Frank E. Perkins, Chairman of the OSTP Panel, described the issues around risk analysis in a meeting at MIT, December 16, 1982.

21. For instance, when overtopping threatened one of its dams, the Corps tended to raise its standards for spillway design, subjecting these to tests. See Robert Buehler, "Unit Curves for Judging Spillway Adequacy," in Engineering Foundation Conference Proceedings 1976, The Evaluation of Dam Safety, Pacific Grove, California, November 28 - December 3, 1976 (New York: American Society of Civil Engineers),

219-239. "There is no technical support for the Corps of Engineers' spillway criteria beyond mere intuition."

22. Alan DeMarr interview at MIT, May 25, 1981.

23. IRG C-6. The chief project engineer, William G. Harber, wrote nine menorandum between 1966 and 1970 that were ignored but called for surface treatment and other procedures.

24. Bruce Tschantz.

25. Except as noted, this description of the FEMA program is based on a conversation with William Bivens, March 1982.

26. National Research Council and Federal Emergency Management Agency. Safety of Nonfederal Dams: A Review of the Federal Role. (Washington, D.C.: National Academy Press. 1982).

27. Ibid. 27.

28. Alan De Marr interview.

29. Letter Richard Curl, OSTP staff engineer, in a fictional letter from "A. Hammurabi, Babylon-on-the-Potomac, D.C." dated January 12, 1979.

30. Public Law 92-367. National Program of Inspection of Dams. House Report 15951, August 8, 1972.

31. Bruce Tschantz, "Recent Progress to Improve Dam Safety," paper presented to 14th ICOLD Conference, Rio de Janiero, Brazil, May 3-7, 1972, 2.

32. A Kenneth Dunn, "A State's View of the Recommended Inspection Guidelines," in Engineering Foundation Conference Proceedings, 1976, 156-162.

33. Committee on Failures and Accidents to Large Dams. Lessons from Dam Incidents, U.S.A. (New York: American Society of Civil Engineers (ASCE) and U.S. Committee on Large Dams, 1975).

34. Bruce Tschantz "Review and Responsibility of State Dam Safety Programs" in Engineering Foundation, 1975, Responsibility and Liability of Public and Private Interests on Dams, 51-62. Proceedings of Conference, September 28 - October 3, 1975. (New York: American Society of Civil Engineers), 51-62.

35. OSTP, 41, Appendix B. "Position Statement: Dam Inspection Program and Engineers' Liability." ASCE, Association of Soil and Foundation Engineers, U.S. Committee on Large Dams, American Consulting Engineers Council." Also see John R. Little, Jr. in "Liability of

the United States under Federal Tort Claims Act," in Engineering Foundation, 1975, 130.

36. William A. Wahler, "Dam Safety Guidelines -- Responsibility and Liability," in Engineering Foundation Conference Proceedings. 1976. 479-499.

37. Homer Willis, provided information on the Corps' inspection program in March 1982.

38. Homer Willis supplied me with a copy of Chief of engineers' "Recommended Guidelines for Safety Inspection of Dams," Department of the Army, Appendix D. [no date]

39. Frank Perkins pointed out how laymen would be treated.

40. National Research Council and FEMA, 7, 9.

41. Bivens.

42. IRG, G-8, letter from Edwin L. McCabe, President of McCabe Brothers [grouting subcontractor] to Duane E. Buckert. August 25, 1976.

43. Ralph Hummel, "Bottom-Up Knowledge in Organizations."

44. This analogy is drawn from personal experience.

45. Jansen, 100.

46. IRG, Appendix C, 1-16, describes interviews with Bureau grouting personnel; the supervisor hotly denied that grouting was ever done through pipes through the fill (C-15), but lower level people claimed it was (C-14).

CHAPTER IV

EARTHQUAKE HAZARD REDUCTION

This story is more complex than the last for it takes a critical look at seismic engineering and seismology as background for the OSTP program.[1] It then describes later events in California and earlier in China. The reader should remember that earthquakes are no more a natural hazard than flowing water; the harm arises from structural failures, just as it does from dams.

This chapter continues the discussion of engineering knowledge in the previous one, describing briefly the nature of seismic engineering knowledge, how it was acquired, and its use. It then addresses these characteristics of knowledge in the nearest field of science, seismology, which began to pre-empt the claims of seismic engineers. The central part of this chapter is a three part story about how these fields joined to gain more federal funds for a whole earthquake hazard reduction program, the seismic engineers lost out in the process of planning in OSTP, and the program became focussed on research needs and reducing hazards to national defense. Here the term seismic engineering is used in a broad sense to cover an array of programs, such as land use planning and building code enforcement, which seismic engineers have long supported.

This chapter goes on to assess earthquake preparedness and prediction programs at a state and local level in California and concludes with a story about successfully combining both prediction and preparedness programs under very different concepts of science and institutional conditions in Revolutionary China. As the theme of the previous chapter was kinds of knowledge, the theme of this becomes different kinds of planning.

Seismic Engineering

In contrast to federal dam engineering, seismic engineers carry out many small projects in the private sector. This field began after the 1906 San Francisco quake, with the question of why some buildings stood while adjacent ones collapsed. Research was done on structural models on shaking tables; students later formulated huge sets of mathematical equations to represent differential stresses in tall buildings.[2] At first these engineers encouraged development of seismology through the installation of seismographs in California, but soon realized that the data on acceleration from these was of little value in their work. More important in structure failure was the duration of vibrating motion. Then they made the vital discovery, that not one factor but a combination of factors in an overlapping pattern caused most of the damage. Each structural member naturally vibrates with a characteristic range of frequencies; if these are amplified by a corresponding range in ground motion, that member may fail.[3] Engineers could estimate the normal vibrations of members but they still did not know the range of vibrations so expect at a particular site from a future earthquake.

Seismic engineers wanted detailed information along a chain with many links, from some unknown source, over a particular path to the soil under the foundation, to each structural member and the configuration of members as a whole. Seismologists studied earthquakes after the fact in particular locations in order to understand general causes not effects; they offered little help. Finally research engineers turned to tables of random numbers for approximate quantities to use in some formulas for design.[4]

Aware than earthquakes could affect much of the nation, these engineers, mainly Californians, promoted the threat.[5] They also sought to embed their knowledge in national or regional model building codes and seismic standards, to be applied by engineers and building officials. Local communities often adopted these codes by reference. Thus seismic engineering knowledge became institutionalized and flowed down and out in uniform rules to govern construction practices throughout the nation.

But local building officials with small staffs and limited power, especially in rapidly growing areas of California, were often forced to loosely interpret or laxly enforce the codes. Violations slipped through, morale would drop, and staff would burn out. Neither stronger regulations nor more formal training made sense to these harried officials. The best work was often done by experienced officials who liked their work and could sense violations before they saw them.[6]

As moderate earthquakes continued in California, other protective measures were explored. An earthquake in 1971 gave impetus to formation of a state level Seismic Safety Council.[7] New legislation also required controls on development in seismic zones throughout the state and a "seismic safety element" in every local land use plan.

But inadequate knowledge of active faults limited the number of seismic zones and data for planning. Most of the seismic safety elements were prepared by consultants and seldom read or understood by local officials. Only a few communities went through a time-consuming, contentious, "messy process" of widespread citizen participation in preparing plans. These were well understood and accepted as legitimate by local developers and officials.[8]

The greatest hazards were older unreinforced masonry buildings. In municipal centers, these structures were sometimes rehabilitated for commercial use under local historic building codes. Such actions might be economically feasible but often displaced low income families and did not make these buildings safe.[9] In the political context, local building officials had little choice but to go along, recognizing that they could be liable should these buildings fail.[10]

One respected senior engineer, Henry Degenkolb, expressed critical insight into what he called the "pretense of knowledge," embedded in local codes and engineering formulas.[11] Like some dam engineers, many building officials and civil engineers treated these without thought as recipes, going by the book or simply not caring or even trying to beat the codes. On the other hand, conscientious engineers, who followed the codes but also used judgment to consider factors outside the codes and safety features, were often penalized for imposing higher costs. Moreover, engineers who depended on tidy formulas derived from analytical research seemed to forget that if a theory was wrong or neglected one factor, as it did for the Tacoma Narrows Bridge, a disastrous failure could result. Many engineers lacked a sense of history and like the public believed that structures build under earlier codes were safe. They forgot that new understanding often made past knowledge obsolete. New knowledge after a surprising failure might do the same to present knowledge. One could not know what it is that one does not know.

On the other hand, the leading engineers in OSTP would be concerned primarily that codes and standards did not fit the extent of hazards in many parts of the country or were not up-to-date, due to a lengthy process for revising them. Developers particularly in eastern cities protested against overconservative codes based on national maps indicating the strength of tremors in the past but ignoring the infrequency of earthquakes outside of California.[12] The engineers needed better information from seismologists, not only for designing individual buildings but for codes to govern construction. But seismologists faced difficulties of their own.

Seismology

This field began to emerge as a modern science in the mid-nineteenth century out of two traditions, an older more speculative one of natural philosophy that led to geophysics, and the more empirical natural history resulting in geology; both are now encompassed by the earth sciences.[13]

Empirical work began in Italy when a Britisher, Robert Mallet, scaled and mapped structural damage and "felt reports" of a strong earthquake, using isometric lines to reveal the focus or epicenter of the tremors. In 1889 such reports from around the world were mapped in an atlas showing seismic regions or belts much as they are known today.[14] The observed effects of earthquakes are now ranked on a modified Mercalli scale of intensity.

Geophysicists disdained felt reports and sought to create a more objective quantitative discipline. They developed accoustical instruments to be their eyes and ears, record and measure tremors, which they treated as waves of energy. They analyzed four types of seismographic signals and calculated the magnitude of energy released from a distant source on what is called the Richter scale.[15]

In practice, however, they can only estimate the amount of energy released in a quake. Much is lost initially in overcoming friction; the rest radiates in all directions. With a few miles of a source (damage seldom occurs beyond ten or twenty miles) different types of waves, released in a minute or less, are indistinguishable on a seismograph. Afar they may fade out or be cluttered with "noise," as from distant tides or nearby traffic. In between, various materials deep in the earth or at the surface may deflect or refract waves or cancel them out or amplify them. Distinguishing specific types of waves requires training and experience. Interpreting the wiggly lines of seismographic instruments has been likened to trying to understand the construction of a violin from the sounds of it heard over a telephone.[16]

Moreover, each seismograph at a particular location receives a unique set of signals. To select a definitive number to represent the magnitude of a distant quake requires scientists to work together to combine data from several instruments, synthesized with specific knowledge of that instrument and materials at the site and an understanding of particular paths from the source, based on many earlier geologic studies.[17]

Scientific understanding of the nature of the earth that produces these signals has changed. Originally the two traditions disagreed: geologists, following Darwin, saw the planet as an aging organism; geophysicists chose the metaphor of a heat engine, subject to entropy. Both wanted to find uniform principles or laws that determine specific phenomena and rejected the idea of sudden processes or unique events, such as proposed by catastrophy theory, as much too random. They also rejected the concept of continental drift proposed by Alfred Wegener early in this century.[18]

A theoretical breakthrough came after the great earthquake in San Francisco in 1906. H.F. Reid compared 50 years of field surveys nearby and suggested that earthquakes occur when stress increases along a fault until it overcomes friction; rocks snap into new alignments like elastic bands. Thereafter American faults were closely studied and classified and characterized by geometric models for laboratory research.

But angular blocks oversimplified the irregular edges of real fissures, which merge at depths or horizontally or end in discontinuities of rock material invisible beneath the surface. New questions ensued over what causes the build up of stress, what limits the length of a fault slip, and what finally triggers a quake. Some scientists attribute "the straw to break the camel's back," to something deep in the earth, others to factors in the surface or to the pull of the moon and the stars above. What a scientist considered a plausible answer often depended upon the scientific specialty in which he sat.[19]

In 1958, during disarmament talks in Geneva, seismologists sat with international experts and urged that seismographs be placed around the world to detect violations of limits on underground nuclear testing. U.S. opponents of disarmament argued that the plans were based on monitoring only one underground test; a second test revealed the limits of these instruments. The SALT talks floundered but the scientists, hoping to prove that science could help solve political problems, obtained liberal federal funds for improving seismology.[20]

At this time, anomalous findings deep in the ocean inspired a scientist to write an "Essay in Geopoetry" which revitalized Wegener's ideas. Seismologists, self-consciously reflecting on Kuhn's theory of scientific revolutions, adopted a new theory of plate tectonics, which seemed to explain the cause of faulting. They also adopted a new metaphor: the earth was like an atemporal cybernetic system, recycling matter and energy in feedback loops in an effort to achieve equilibrium. Other new ideas followed: earthquakes recur in regular cycles with a gap after each; if no quake had occurred for some time, a new one would soon occur. By the mid 1960's geophysical concepts dominated the field.[21]

But tectonic theory could not account for many earthquakes, such as those in the middle of plates. Various hypotheses now view in explanation.[22] Recurrence times may vary from tens to thousands of years; seismologists seldom know particular cycles, or what part of a cycle a fault is in. Nor could elasticity account for faults slipping gradually without tremors. "Aseismic creep" was finally explained after seismologists discovered that fluids lubricate fine rocks or "gauge" in fissures and "ripen" faults for premature quakes. They learned about floods after several earthquakes occured in several new dams and after citizens linked tremors in Denver to the Army's injection of fluids into deep wells nearby.[23] A proposal to insert water into the San Andreas Fault, forcing it to release stress gradually, was abandoned lest it trigger a major quake.[24] But the hope of control lingers on.

In 1965 a Presidentially appointed Panel chaired by Frank Press, encouraged by the progress in theory and research, made the surprising announcement that with enough money, within a decade, seismologists would be able to predict earthquakes and thus save lives.[25] This promise brought new saliency, especially in Congress, to seismological research. But it also deeply divided the community of scientists, many of whom felt that the promise was premature. The promise annoyed seismic engineers especially; they had long claimed saving lives as their exclusive raison d'etre in their unsuccessful competition with the scientists for research funds.

In the early 1970s laboratory scientists discovered a "dilatency effect" in acorn-sized pieces of rocks under pressure. These expanded with many small cracks before they split. Linking this phenomenon with field observations measured before quakes in Russia and Japan, seismologists devised a theory about a uniform set and sequence of precursors.[26] Even though seismologists did not fully understand the causal mechanisms, they were euphoric about short term forecasting of quakes with the accuracy of weather predictions on the basis of readily observable field phenomena. They sought funds for arrays of new field instruments.[27] They also became even more involved in political action, as the following story attests.

The Story of the EHR Act

What follows is a three-part story of the passage and aftermath of the National Earthquake Hazard Reduction Act of 1977.[28] The first chapter began after the San Fernando quake, when Senator Alan Cranston introduced the first of a series of bills, with little hope of more than public education. His legislation urged funds primarily for earthquake prediction research. Staff in the National Science Foundation (NSF) and USGS, asked to comment, pointed out the need for more practical measures; to their surprise, their suggestions were accepted.

Thus began what staff called a "conspiracy."[29] Administrative and legislative people began to work together for a "whole" national earthquake hazard reduction program encompassing seismology, seismic engineering, and more diffuse programs for seismic safety. In the face of competition over limited funds among agencies and outsiders, NSF and USGS staff also made a "gentlemen's agreement" to share funds and cooperate on programs.

Over the next five years, these self-proclaimed conspirators at mid-levels of separate institutions learned some valuable lessons. Most important was to treat neither victories nor defeats as final, but to closely guard the turf that had been gained and to take advantage of every opportunity to advance the cause. They slowly assembled a diverse constituency in support of legislation.

Seismologists might not have joined had not social scientists shocked them by suggesting that earthquake prediction could have negative effects. Local officials might ignore a prediction or question its validity; local residents might flee the area.[30] Experts later concluded that dire economic, social, and political consequences were possible but without experience to draw on the precise response was extremely uncertain. A combination of a negative public response and a devastating quake could be worse than no predication at all.[31] The seismologists' promise of social benefits had now become a threat.

The seismologists' solution was to make a simple distinction; they would issue only objective, politically neutral scientific statements. Public officials must take responsibility for interpreting these, issuing "warnings," and managing public responses.[32] The federal government now was expected not only to supply research funds but to control the public reaction. Some saw little benefit in making a prediction until government was ready to implement plans.[33]

Seismologists also sought to institutionalize control over the quality of research. Both California and the USGS set up Earthquake Prediction Evaluation Counsels, CEPEC, and a national NEPEC. A top group of scientists would validate methods used to arrive at any predictive statement. Research results must meet criteria on the expected time, place, magnitude, and probability of occurrence.[34]

Even then, seismologists felt vulnerable. Predictions were recognized as different from weather forecasts. Outsiders could have no evidence to confirm that an earthquake might occur or if it did not, that it would be more likely in the future; they would have to take the scientists' statements on faith.[35] If no quake occurred as expected, seismologists worried that their competency would be questioned, research funds cut off, or even that they would be held liable for economic damages such as the decline of local property values.[36] More than a negative social response to a prediction followed by a disastrous quake, scientists seemed to fear a negative response followed by no earthquake at all, even though the public would be spared.

Other problems arose from the nature of their work and their research institutions. Short term precursory phenomena would allow no time for publication and peer review as in other areas of science, nor perhaps for top scientists to examine evidence in the field. They would have to trust the judgement of distant field workers, subject to error or lapses in objectivity. Long term predictions, which could depress a region's economy, could not be confirmed for years. Probabilities assigned to such predictions would be little more than guesses until a sufficient number had been successfully made.[37]

On the other hand, research institutions or individuals competing for funds and the prestige of being first might either make premature predictions or withhold proprietary information until they were more confident. In a democracy the freedom of speech of scientists could not be suppressed, but in a free market system, scientists with inside knowledge might also withhold it for private advantage.[38]

The press and public tended to misunderstand or misinterpret the statements of scientists or else they accepted the word of seers as scientific; if these were discredited by scientists, the public might disbelieve scientific statements as well. With such logic, NEPEC and CEPEC must "filter" all statements about seismic events and certify predictions.[39] A few scientists who later made public statements were humiliated; thereafter other were afraid to try.[40]

As a general policy, seismologists retreated from research in densely populated areas to focus instead on rural areas, where they said that earthquakes were the most likely to occur.[41] They also came to support legislation appropriating funds not only for basic research but for federal planning.

In 1976, natural and human events seemed to conspire to foster passage of the National Earthquake Hazard Reduction Act. Although quakes in this country were less than normal, the death toll worldwide was the highest since the great earthquake in Lisbon, Spain in 1556.[42] Evidence of an uplift of land in Southern California later acknowledged to have been the result of surveying methods[43] was reported by Frank Press to Nelson Rockefeller, gaining the Vice President's support for legislation. Then President Ford commissioned a study of funding requirements, to attract California voters.[44] By 1976 all important constituencies, Congress, the President's Office, major agencies and scientists and engineers were united in favor of a large appropriation for research and hazard mitigation measures.

Chapter Two of the story began long before the Earthquake Hazard Reduction Act, P.L. 94-282, was passed in October 1977. Frank Press persuaded Congress to let OSTP plan for use of the funds. In the late Spring of 1977, with OMB's approval, Philip Smith organized a staff and hired Karl Steinbrugge, a seismic engineer to prepare the plan. Two major impediments soon arose. There was no budget data on the cost of earthquake related federal programs; agencies had not recognized these as separate items.[45] Second, OMB was determined to include this new program in its proposed reorganization of emergency services and asked the planners to defer discussion of organizational matters.[46]

Instead, the staff borrowed from various agencies focussed on accumulating a comprehensive set of more than fifty "issue statements," in part by consulting with leaders of national interest or "umbrella" groups in Washington. Since everyone interested in reducing seismic hazards already knew what the issues were, the staff carried out the process of "going public," largely to show that plans were not made in an ivory tower.

Assembling these separate statements into a cohesive document proved difficult; a comprehensive plan was never completed. The draft called largely for additions to existing federal programs, such as HUD, for land use planning and for structural rehabilitation of older buildings. The USGS was to set criteria for State and local mapping of seismic hazards for local planners, who should be trained to use the data.[47]

New uncertainities emerged, such as about the effects of earthquakes on "critical facilities" -- nuclear power plants, liquified natural gas tanks, toxic waste facilities. A major concern was that public protests impeded timely decisions and drove up construction costs.[48] A secondary issue was that risk analysis was inadequate to calculate the chain of events from the failure of these to unprecedented tertiary disasters.[49] On the other hand, risk assessment techniques were lauded for new national seismic maps and as a way to avoid over conservative design in particular structures such as hospitals, and for the new national seismic maps, which gave local officials a choice in the acceptable level of seismic risk.[50]

"Shared Responsibilities" was emphasized in the draft in an introductory paragraph: "... virtually every level of society -- the individual, family, firm and community" make decisions affecting seismic safety. "The achievement of a safe environment is basically a shared responsibility of all levels of government and the private sector."[51]

This statement was retained in the final version, but given less emphasis.

The draft also contained contradictions. On the one hand, it described the huge aggregate national consequences of future earthquakes, requiring strong federal leadership and planning. On the other hand, it mentioned that only a few small areas would experience a major earthquake by the year 2000. Individuals correctly perceived that the probability of personal harm is exceedingly small.[52]

These widely divergent representations of the hazard -- in the aggregate and to individuals -- was reconciled as follows: The failure of past programs for disaster mitigation had taught disaster planners that persuasion and shared responsibility would not work, because of "human nature." People live in the present, assume that everything is all right until events prove otherwise, avoid even thinking about future disasters, and are not motivated to act even in their own self-interest.[53] Moreover, they expect the government to protect them. Federal planners must therefore package earthquake hazard reduction with other emergencies, including nuclear war, and prepare general plans for damage control. Such plans were especially necessary after a quake when looting and other forms of social disorder would be rife. However, this expectation of social disorder contradicted evidence that local people organize themselves and help one another after disasters.

One general principle in planning was to avoid arousing the public about hazards until there is something for them to do.[54] Protective control should be build into the myriad of social institutions that give order to society and could assure compliance. Incidentally, this concept of command and control allied the disaster planners with the scientists, who expected knowledge from basic research to flow down like water to inform the actions of key professionals and public decision-makers.[55]

Time began running out for OSTP to prepare a plan to present to Congress. Smith took control and, using the appearance of the power of the President's office, exacted commitments of general programatic support from a dozen agencies. He quickly obtained their "sign-offs" on further discussion.[56] He also insisted over staff objections that a lead agency for EHR be placed in the proposed Federal Emergancy Management Agency. The staff argued that FEMA's leadership, drawn from the military, with little experience in preventing disasters but only in cleaning up afterwards, would not understand the slow pace of scientific research and incremental planning required for any successful program.[57] The staff lost that battle.

The "whole" national program suffered a major defeat a few weeks later. Central to OSTPs plan was a program granting funds to states for planning. In a routine "decision memo", the President was asked to approve these funds.[58] Much to OSTP's surprise, he vetoed it instead. The reasons were obvious: Carter was reluctant to benefit a political rival, California Governor Jerry Brown, who would get the largest grant; OMB also feared setting a precedent for federal funds to states for planning protection from other natural hazards. When the OMB threatened to cut off funds for scientific research unless OSTP acquiesced, it did.[59] Congressional intent was thwarted; the funds went primarily to research.

After the plan went to Congress,[60] Charles Thiel was put in charge of the new program, still in OSTP. With a skeleton staff, he used "mirrors" to enlist support from state officials, to create committees to clarify model codes and standards, and to try to persuade federal agencies to retrofit their own unsafe structures.[61] Meanwhile, seismologists struggled to extricate NEPEC from provisions of the Freedom of Information Act requiring several weeks notice of meetings and precluding timely decisions for a short-term prediction. They also tried to protect themselves legally from liability for damage from a prediction or a quake.[62]

Finally established in FEMA, Thiel puzzled over how to use the policy making process not only to advance theoretical knowledge but to save lives. He kept "striking out." Then an Asian friend pointed out that Thiel had been using the approach of Western science, analyzing the problem into parts in order to find solutions one at a time, and hoping that these would add up to a total solution. He advised Thiel to work with others equally concerned, gain agreement on the essence of the problem, and then build a constituency for a solution.

Meanwhile, USGS seismologists were planning the next phase of research. Reflecting on the status of their knowledge, they admitted that the pieces of their theories did not fit well together nor account for empirical evidence, especially of precursory phenomena. They pinned their hopes on improving theory through vast experiments around the globe using expensive space age technology. But they also emphasized that highly organized research should not neglect support for innovative individual research.[63]

This respect for individual research no doubt arose in response to the work of Terry Sieh. As a graduate student in 1978, using only a pick and shovel and an understanding of local geology and geologic history, he had uncovered evidence of a series of major earthquakes on the San Andreas fault. Dating the recurrence intervals with a Geiger counter, he suggested that another rupture would occur late in this century.[64] The fruitfulness of this simple research amazed seismologists.

<u>The third chapter</u> of the federal story began unexpectedly with the volcanic eruption of Mount St. Helens. Frank Press and others, planning to fly with the President to view the devastation, rehearsed their comments: the damage was minor compared to the impact of a major earthquake, especially on military installations in California. After the trip, Carter convened a committee of the National Security Council to plan protection for national defenses.[65]

Meanwhile, a second conspiracy had begun, this time also involving administrative and legislative officials in California. People from FEMA, Congress, the California legislature, and its Seismic Safety Council agreed that the problem was to fund a small prototype program. Early in 1980, seismological data indicated that a prediction might be forthcoming for Southern California,[66] threatening chaos in Los Angeles. This threat was seen as an opportunity since the city had the first plan in the nation for preparing for a prediction.[67] That metropolitan area became the focus of the prototype program.

A Machiavellian scheme evolved: the California legislature, convinced that Carter would give no money to that state, would be persuaded to appropriate funds for the prototype program, contingent on federal matching funds. While support for legislation increased, the planners accumulated commitments of unexpended funds from FEMA and future money from Congress for a joint three-year planning program in the Los Angeles area. Governor Brown, who had previously disdained seismic safety, then claimed credit for the new program, while Carter reluctantly approved FEMA's participation at the end of his administration.[68]

In the second year of the Reagan administration, Thiel resigned, leaving a small staff without a director. But FEMA was preparing elaborate plans for the Army to control an area immediately after a quake and for coordinated federal relief efforts.[69] Thus the final chapter about this federal effort ended. We now turn to planning and research at the local level.

The Aftermath

In California both seismologists and seismic engineers have recently had successes, but again on different tracks. We deal with each in turn. Those concerned with hazard mitigation achieved success more in spite of and not because of the federal program. Early in 1981, the Los Angeles program, known by the acronym of SCEPP, began unpropitiously with a conflict over its name and purpose, whether it was the Southern California Earthquake Prediction -- or Earthquake Preparation -- Project. Its objectives and a dictatorial director delighted FEMA but alienated State and local officials. After only a few months, the Seismic Safety Council fired the director and turned this fiasco to advantage by limiting FEMA's control of the grogram and assembling a dedicated professional staff.[70]

To spite FEMA, Governor Brown initiated his own program in 1981. Its charismatic director scorned FEMA's attitude of "Big Government versus the Great Earthquake," which encouraged people to feel more helpless. Instead, he dreamed of organizing and training teams of people in business and industry to demonstrate their resourcefulnesss as paramedics and firefighters immediately after a disaster, when citizens would be "protected from big government."[71] He failed to organize permanent committees, but did inspire leading citizens to prepare their corporations and communities, thereby strengthening the program in Los Angeles and throughout the State.

At SCEPP, the professional staff soon learned to live with uncertainty and to operate like a multidisciplinary team. They made flexible agreements with the many jurisdictions in the metropolitan area. They collected and nurtured support and offered technical assistance to specialized groups, such as shop keepers and gasoline station managers. They shared ideas for educational and practical actions. For instance, school children observed doll houses on shaking tables to overcome their fears. Residents were encouraged to bolt structures to foundations, secure ceiling fixtures and heavy furniture to the walls, and to learn where to shut off gas intakes to prevent fires in the event of a major quake.

SCEPP was said to be getting into the "doingness" of earthquake protection, helping others to prepare for a major disaster. Its aim was not to create paper plans, such as FEMA's, nor establish a centralized bureaucracy but to work from the bottom up to enable individuals and groups to be self-sufficient during and after a quake.[72]

In 1985, California generated national interest in its first Earthquake Hazard Preparation Week. The Governor entered a shaking van with Yogi the Bear, a mountain rescue team removed people by helicopter from tall buildings, and public officials handled simulated emergencies or observed the collapse of structures on Hollywood sets. Ham radio operators practiced communicating messages and first aid teams dealt with mock disasters. Private firms distributed booklets on household self-protection and signs for elderly to put in their windows asking for help.[73]

FEMA showed off its new plane equipped as a portable press room, apparently to keep newsmen from interfering with the Army's management of a disaster. With its fire-fighting mentality -- wait til the crisis occurs and then send in masses of men and equipment -- it was bewildered by SCEPP's success and frightened by what had become a kind of popular movement, that seemed out of control.[74]

<u>Prediction research</u> was also progressing on a federal model with a costly program near Parkfield, an almost unpopulated area in the center of the State. The USGS blanketed the area with sophisticated equipment to monitor signals from a remarkably well-behaved fault. Since 1856 it had erupted every 22 years, with one premature exception, enabling scientists to predict another quake in 1988, plus or minus four years. Finally, after two decades, seismologists had made good on their promise and issued a prediction.[75]

But that program has limited applicability to less well-behaved sites; it serves mainly to improve general understanding and test equipment. Some scientists suspect that the first socially useful prediction will come from a consensus among a handful of field geologists monitoring data in the hills above Los Angeles, adjusting data from modest equipment for atmospheric changes and other "noise," and seeking convergent patterns.[76] But would anyone take their consensual judgment seriously?

Once before when seismologists had warned of a local quake a few days in advance, State officials ignored them leaving residents confused and inactive; fortunately no one was harmed. The State Geologist was concerned about the four-way fragmentation of the prediction system, dividing scientists and public officials at both federal and state levels, and questioned whether FEMA, especially, would respond in a timely and appropriate manner.[77]

Others criticized the prediction system as too passive, simply waiting to validate research. Instead, NEPEC should be aggregating data from every possible source and searching for patterns that might warn of coming quakes. The prediction system was the product of old-time seismologists, uncomfortable working under public scrutiny and, like prophets without honor, burned out from bearing bad news. These scientists also did not seem to appreciate the qualitative difference between the effects of earthquakes in the past and what could occur in the future.[78]

On the other hand, seismologists had led the public to believe that they could put a stethoscope to the earth and diagnose a coming quake. The best they could do was reach a consensus to bracket a time and place, but would know only after the fact if their collective judgment had been sound. The public must learn that there are no guarantees.[79] However, even a failed prediction would be valuable to provide data on public reaction to replace costly speculation in federally funded social studies.[80] Meanwhile, there was a shortage of funds and staff for state programs to explore faults suspected to be active near urban areas and to replicate Sieh's site specific studies, to enable better local planning and warnings of potential local quakes.[81]

The field of seismology has also been criticized as overly dependent upon geophysical theory, which is unable to account for the variety of natural phenomena observed in the field[82] or explain the different processes that seem to generate earthquakes around the world. Indeed, as in other fields of science, research is seldom convergent; for each problem solved a dozen more questions arise, proliferating like rabbits. Seismologists' latest problems arise from new instruments and more data than can be processed, and have been characterized as problems of too many rabbits and too much noise out there. Some observers agree that prediction seems less likely than a decade ago and was badly oversold for political reasons.[83]

Another Perspective

An American account of a successful earthquake prediction sparing thousands of lives in the mid-1970s in China, one in an uneven sequence of successes and failures, gives a fresh perspective on Western science.[84] Admittedly, the Chinese have records dating back 3000 years, from which they have organized, mapped, and statistically tested data on earthquakes, searching for patterns. They have also operated under a very different cultural, social and institutional system, especially during the Cultural Revolution, favoring massive empiricism. They disdained building theories, preferring to let theory "grow from the roots up, like a tree," or "be honed on the fine edge of practice."

Under Mao-Tse-Tung, China adopted an "open door policy" toward science, welcoming in the masses. It sought to combine knowledge old and new, indegenous and foreign, from folk lore and from science. The revolutionary government put highest priority on a precautionary program against earthquakes, common throughout the nation. When a six-year pattern of quakes pointed to one in Haicheng, a few scientists set up instruments there to monitor changes, and recruited or accepted services volunteered by groups of untrained citizens who began to gather data with simple and often homemade equipment. The scientists incrementally checked teams reporting significant findings, instructing those whose methods were poor, sending the best to enlist and instruct others, but encouraging all, until a network of 5000 observation points blanketed the area.

Meanwhile, local people received literature explaining what was known about earthquakes and how they could take precautionary actions, such as building temporary shelters and first aid stations, and leaving their homes if a warning were issued. When the prediction came, 600,000 people had moved outdoors and were spared from harm.

A U.S. team visiting China later acknowledged that the Chinese generally practiced good seismology, using many fine instruments, some unknown in the West. But they disdained the lack of parsimony as inefficient; they also suspected that they were shown only the best of the data. They considered reports on strange atmospheric phenomena and animal behavior, with no basis in Western theory, as the product of a kind of group madness. They dismissed reports of earlier successful predictions, especially by village groups, suspecting that local people might have been punished by Communist leaders for unauthorized actions. The Chinese could not even cite statistics on their success to failure rates. The Americans concluded that this prediction was not the result of science, since it did not use hypothetico-deductive methods, but was the result of good luck in response to a crescendo of small tremors.

The Americans were especially bewildered by how the Chinese assessed data, arrived at a decision to predict , transformed the prediction into a warning, and obtained citizen compliance. Why would people abandon normal activities and suffer winter discomforts outside? In response to these questions, the Chinese considered the decision process and mass response unproblematic and did not distinguish between a scientific prediction and an official warning.

Decisions were obviously made incrementally by groups combining a variety of perspectives at different levels, as data was gathered and flowed up for repeated evaluation in exhaustive discussions. After using "intuitive judgments," these groups as a whole, not one leader, issued increasingly specific statements up to the warning a few hours before the quake. The visitors thought that such a process in the U.S. would impede effective decision making and be viewed as a delaying tactic, exacerbating conflict. But they envied one attribute of the Chinese program, that citizen participation relieved the scientists of full responsibility if a prediction failed.

An American sociologist, John Turner, explained how these groups bridged the dual institution of science and civilian authority like rungs on a ladder. At the lowest level, volunteers served as staff to scientists but gained status as scientists in their villages, while scientists served as advisors to government officials at various levels. Compliance was explained by the cultural emphasis on social over individual welfare, reinforced by viewing participation as indicating commitment to the revolutionary ideology. In this way, the Chinese had solved the tripart problem of "incentives, control and communication," plaguing voluntary programs in the United States, where individuals receive little credit in their neighborhood or vocational life for voluntary work. These lessons were barely apparent in the OSTP program but were partially applied by SCEPP.

Summary and Conclusions

This chapter has shown how seismic engineers have been socially motivated to acquire and institutionalize their knowledge for widespread use and have encouraged others to participate in improving public safety. Still their knowledge appears to be little different from that of dam engineering, not based on science and in practice more like an art. Moreover, in a social reality as messy as the physical one of dam construction, these professionals long had difficulty in obtaining adequate use of their knowledge. The seismologists, with even more difficulties making predictions about an equally complex physical reality, have focussed on building better theory, while protecting themselves against potential threats in a poorly understood social environment.

On the other hand, planners, first at mid-levels of government and then from the bottom up in California, were able to bring together broad-based support for national legislation and then for a successful prototype program that has become something of public movement for earthquake preparedness in California. A similar kind of movement took place in Haicheng, where the Chinese disdained building theory but set out to save lives. Although never referred to as "planning," their precautionary program could be seen as planned actions that involved a messy incremental bottom-up process and elaborate social arrangements for doing science, resulting in saving lives. Both the California and Chinese cases seem to validate the prescription of some of the planners, that decisions about seismic safety must be made by all kinds of individuals and groups at all levels of society and that responsibility for a safe environment must be shared. That is the moral of this story.

We now move to planning a policy for protecting the public against the hazards of nuclear waste. Our focus will be on the extent to which these planners understood the lesson from the dam safety program about the nature of knowledge for engineering and the prescription of the planners in the story just told.

NOTES

1. This chapter is a summary of two more extensive studies, one on seismic engineering and general hazard reduction efforts and one on seismology. I am grateful to Anton Dainty, a visiting scientist at MIT, who read the long version of the section on seismology.

2. John A. Blume describes the early struggles of seismic engineering in "Earthquakes and Stamford" in The Future of Earthquake Engineering: Proceedings of the Inaugural Symposium of the John S. Blume Earthquake Engineering Center. (Stamford, Calif.: Stamford University, 1976).

3. Encyclopedia Britannica, 1966 ed., s.v. "Earthquake."

4. David M. Boore, "The Motion of the Ground in Earthquakes," Scientific American, May 1976, 10.

5. For instance, FEMA expected that a catastrophic earthquake would be the greatest national disaster since the Civil War, in Federal Emergency Management Agency. An Assessment of the Consequences and Preparations for a Catastrophic California Earthquake: Findings and Actions Taken. Washington, D.C. November 1980, p. 2. OSTP saw the hazard as one that may affect "sometimes hundreds of thousands of square miles," cause structural damage in "tens of billions of dollars," and "life loss and injury to tens of thousands." in Office of Science and Technology Policy: Working Group on Earthquake Hazard Reduction. Earthquake Hazard Reduction: Issues for an Implementation Plan. Executive Office of the President. Washington, D.C. 1978. 7. Hereinafter referred to as Issues.

6. International Conference of Building Officials. Issues Which Affect the Role of Building Departments in Earthquake Hazard Mitigation. The State of California Seismic Safety Commission. April, 1980. 31 and passim. Hereinafter referred to as ICBO.

7. Robert E. Olson, Director of the Seismic Safety Council for a decade until 1982, recounted its history in an interview in Sacramento on November 24, 1982. J. Henry Lambright also gives some history in "Policy Innovation in Earthquake Preparedness: A Longitudinal Study of Three States." Paper prepared for the Annual Meeting of the American Political Science Association, Denver, Colo, September 2-5, 1982.

8. ICBO, 20-27.

9. Issues, 114-16.

10. ICBO, 19-20.

11. Henry J. Degenkolb, "Earthquake Engineerng and the Practicing Engineer," in The Future of Earthquake Engineering: Proceedings of the Inaugural Symposium of the John S. Blume Earthquake Engineering Center, (Stamford, Calif: Stamford University, 1976), 117-129. Degenkolb was then Director of the Earthquake Engineering Research Institute.

12. Issues, 39-41.

13. Steve Pyne describes conceptual models in "From the Grand Canyon to the Marianna Trench: The Earth Sciences after Darwin" in Nathan Reingold, editor. The Sciences in the American Context: New Perspectives, (Washington, D.C. Smithsonian Institution Press, 1979), 165-92.

14. Bruce A. Bolt presents basic concepts in seismology in Earthquake Primer, (San Francisco, Calif: W.A. Freeman and Co, 1978), 99.

15. Carl Kisslinger "Birth and Growth of Seismology" in National Academy of Sciences, Committee on Seismology. Trends and Opportunities in Seismology, Washington, D.C. 1977. 93-99.

16. Boore, 11.

17. Ibid, passim.

18. Pyne.

19. See Robert L. Wesson and John R. Filson, U.S. Geological Survey. "Development and Strategy of the Earthquake Prediction Program in the United States." October 30, 1980, draft of Paper presented at the Ewing Symposium on Earthquake Prediction.

20. James R. Killian, Jr. Sputnik, Scientists, and Eisenhower, (Cambridge, Mass: MIT Press, 1977), 150-66.

21. Pyne, passim.

22. Walter Sullivan. "A Year of Earthquakes: Is There a Worldwide Link? New York Times, November 8, 1983. C-1. Frank Press provides three hypotheses to explain a particular mid-plate earthquake.

23. Bolt, 119.

24. Charles Perrow, Normal Accidents, (New York: Basic Books, 1984), 243-44.

25. Office of Science and Technology: Ad Hoc Panel on Earthquake Prediction. Earthquake Prediction -- A Proposal for a Ten-Year Program of Research. Executive Office of the President. Washington, DC. 1965.

26. Bolt, 142-43 gives details of precursory theory.

27. National Academy of Sciences, Committee on Seismology. Trends and Opportunities in Seismology, (Washington, D.C., 1977), 27.

28. This story has been likened to Rashomon, in which different people tell different versions. Because so many people worked on this program, each knew a part but no one knew the whole story. What follows is compiled from interviews with members of the OSTP Steering Group, Charles C. Thiel, Jr., Director of the Earthquake Hazard Reduction Program, 1979-1981, and Robert M. Hamilton and Robert L. Wesson of USGS, mainly in March 1981, and with others.

29. Although some informants did not want the "conspiracies" to become public knowledge, a general understanding of these events, far in the past, is important to my argument. To protect confidentiality, attributions will not be made for some specific statements.

30. Eugene Haas, "Forecasting the Consequences of Earthquake Forecasting," in Social Science Perspectives on the Coming San Francisco Earthquake. Natural Hazard Working Paper No. 25. Boulder, Colo.: Institute of Behavioral Sciences, 1974.

31. National Academy of Sciences, Panel on the Public Policy Implications of Earthquake Prediction. Earthquake Prediction and Public Policy. Washington, DC 1975, describes the issues around prediction that divided groups of scientists and engineers. Hereafter referred to as Turner Report.

32. This distinction was initially made by the Director of the USGS, who proposed a national evaluation council similar to one being established in California. Issues, 29-31.

33. National Academy of Sciences. Committee on Seismology. Predicting Earthquakes: A Scientific and Technical Evaluation with Implications for Society, Washington, D.C.. 1976, 27. Hereafter referred to as the Allen Report.

34. Issues, 106.

35. Turner Report, 29.

36. Ibid, 82-84.

37. Issues, 33.

38. Allen Report, 28.

39. Issues, 107.

40. J. Henry Lambright, "Earthquake Prediction and the Governmental Process," paper prepared for conference of the Policy Research Center, Redlands, Calif. June 25, 1982.

41. Allen Report, 15.

42. Walter W. Hays. Program and Plans of the U.S. Geological Survey for Producing Information Needed in National Seismic Hazards and Risk Assessment, Fiscal Years 1980-84. Geological Survey Circular 816. Washington, D.C. 1980. 5.

43. "Palmdale Bulge Doubts Now Taken Seriously." Science, 214 (18 December 1981): 1331.

44. National Science Foundation and Department of Interior. Earthquake Prediction and Hazard Mitigation Options for USGS and NSF Programs. (The "Newmark Report"). Washington, D.C. September 15, 1976.

45. The Earthquake Hazards Reduction Working Group (WG), Staff Notes #8, Meeting of October 5, 1977, and Staff Notes #9, October 12, 1977 describes OSTP's aborted efforts to obtain budget data expected from OMB.

46. Nye Stevens describes the chaotic state of emergency preparedness and assistance programs in "President's Reorganization Project; Issue Summary; Disaster Preparedness draft undated paper of mid-July 1977, attached to memorandum for Cliff Berg from Philip M. Smith, July 26, 1977.

47. Office of Science and Technology Policy, Earthquake Hazard Reduction: Issues for an Implementation Plan: Draft, prepared by the Working Group on Earthquake Hazard Reduction. Executive Office of the President. Washington, DC. December 1977. Hereafter referred to as Draft Issues.

48. Draft Issues, II-49-50.

49. Issues, 143. Discussion of the limits of risk analysis on critical facilities was added to the final version.

50. Draft Issues, II:45-48.

51. Ibid, 8.

52. Ibid, 33.

53. Ibid, 9.

54. Ibid, 14. This wording was later deleted.

55. Thiel pointed out the parallel between the idea that knowledge of science flows down and that bureaucratic orders flow down.

56. Karl V. Steinbrugge in an interview on November 25, 1982, characterized this procedure as Smith's "executive style of management" in contrast to his own "teamwork approach."

57. Charles Hamilton in a letter to Phil Smith, October 3, 1977, and memorandum to the Steering Group, March 16, 1978.

58. Decision Memorandum for the President, May 5, 1978.

59. Frank Press in a letter to Jim McIntyre, May 15, McIntyre in a letter to Press, May 25, and Press in a letter to McIntyre, May 30, 1978.

60. The President transmitted the proposed program to Congress on June 22, 1978.

61. Office of Science and Technology Policy, Report on the National Earthquake Hazards Reduction Program. Executive Office of the President. Washington, D.C. September 1978.

62. Frank Press letter to Secretary of the Interior Andrus, May 12, 1978, and draft document from the USGS to Representative O'Neill, no date, 1980.

63. Wesson, op. cit.

64. Kerry Sieh, "Prehistoric Large Earthquakes Produced by Slip on the San Andreas Fault at Pallett Creek, California, Journal of Geophysical Research. 83:3907-3933. August 10, 1978.

65. National Security Council ad hoc Committee on Consequences and Preparation for a Major California Earthquake: Minutes of Meeting, June 12, 1980.

66. The data was reported by C.B. Raleigh, USGS, in "Review of Geophysical Data Pertinent to Earthquake Prediction in California Summary." December 14, 1979, unpublished, and discussed in a memorandum of March 3, 1980, to Frank Press, OSTP, from Rob Wesson, ?? data, like that on the Palmdale uplift, was later found unreliable.

67. Rachel Gulliver, Consensus Report of the Task Force on Earthquake Prediction, (City of Los Angeles, Calif.: 1978).

68. Joseph Lang, a staff member in the California legislature, described details of this story in an interview in Sacramento November 25, 1982. Part of it is told in J. Henry Lambright, paper of September 2-5, 1982, op cit. Also see Federal Emergency Management Agency. An Assessment of the Consequences and Preparations for a Catastrophic California Earthquake: Findings and Actions Taken. Washington, DC. November 1980, Annex 1, pp. 37-42. for the correspondence between Brown and Carter.

69. Federal Emergency Management Agency, Annex 2, "Federal Earthquake Response Planning," 46-51. The most complete plan was that of the Sixth U.S. Army.

70. Interviews in California in November 1982 were used for this description of SCEPP. Also see J. Henry Lambright. "Applying Earthquake Prediction to Southern California: Preliminary Notes on an Intergovernmental Project." Proceedings, The Third International Conference on Microzonation for Safer Construction. Seattle, Wash: June 28-July 1, 1982. 1513-1525.

71. William W. Whitson, former Director of the Governor's Emergency Task Force on Earthquake Preparedness, telephone interview in San Francisco, November 21, 1982.

72. Paul J. Flores, Executive Director of SCEPP, interview in Los Angeles, November 18, 1982.

73. Sandra Blakeslee. "California at the Ready, It Hopes, for Big Quake." New York Times. April 14, 1985, 24.

74. Robert E. Olson made the analogy with the citizen participation movement of the 1970s, as in the Model Cities program. He recalled how technical transportation planners were likewise confused and frustrated by planners working with citizen groups, who did not appreciate technical requirements.

75. Richard A. Kerr, "Quake Prediction Under Way in Earnest," Science 233 (1 August 1986): 520.

76. Robert A. Wallace, U.S.G.S. member of the Policy Board of SCEPP, at its monthly meeting in Los Angeles on November 17, 1982.

77. James F. Davis, State Geologist, interview in Sacramento, on November 26, 1982.

78. Richard A. Andrews, Executive Director of the Seismic Safety Council, interview in Sacramento, November 24, 1982. Also Richard Andrews, "Earthquake Prediction and Preparedness in Southern California: Science and Public Policy." Paper presented to the Annual Meeting of the Seismological Society of America, Anaheim Calif., April 20, 1982.

79. James F. Davis, "Trends in Programs and Research Directed Toward Earthquake Predictions and Responses," in United States Geological Survey: Office of Earthquake Studies. Proceedings of Conference XII: Earthquake Prediction Information, 116-32. Menlo Park, Cal. January 28-30, 1980.

80. Gilbert F. White, "What is Enough Information about Earthquake Prediction?" United States Geological Survey. Office of Earthquake Studies. Proceedings of Conference XII: Earthquake Prediction Information. Menlo Park, Calif. January 28-30, 1980.

81. See the California Division of Mines and Geology. Proposed Earthquake Safety Programs and Activities of the Department of Conservation; Fiscal Years 1982 through 1986. Special Publication 57. Sacramento, Calif. 1980, and other publications of the Division.

82. John McPhee, In Suspect Terrain, (New York: Farrar, Straus & Giroux, 1984).

83. Such informal assessments were made by several seismologists during my visit to California.

84. American Seismology Delegation to the People's Republic of China. "Earthquake Research in China." EOS Transactions of the American Geophysical Union, 56 (1975): 838-881.

CHAPTER V

NUCLEAR WASTE MANAGEMENT

The story of planning a policy for the management of nuclear waste will be told only briefly, with emphasis on institutional issues and problems of knowledge. The reader is urged to consult more complete and detailed accounts.[1] In contrast to the previous stories, this topic became high on the federal agenca due to broad and continuing public concerns about past failures of government to deal with a long-term threat to health and life. To isolate man-made radionuclides from the biosphere for millenia could require engineered systems with permanence beyond the wildest dreams of dam engineers and predictive knowledge from the earth science beyond the highest hopes of seismologists. Moreover knowledge must be combined about phenomena at the atomic scale with geologic knowledge up to the broad scale of regional water systems.

This story begins with background on the history, institutional context, and technical terms. It then describes policy planning during the Carter administration. A third section describes how the planners tried to overcome gaps in knowledge. This is followed by a synopsis of recent problems in dealing with various kinds of waste and concludes with a local perspective on policy implementation.

The Institutional Context

Historically Congress empowered the Atomic Energy Commission (AEC) in 1954 to promote and regulate nuclear materials developed for the atomic bomb for peaceful use. For two decades the AEC was part of a "cozy subgovernment" linking the Commission and the Joint Atomic Energy in Congress.[2] The AEC was eventually attacked for mismanagement and suppressing safety problems and for a conflict of interest between promotion and regulation, a conflict not so apparent when the peaceful use of the atom was considered overwhelmingly beneficial. Its functions were split between the Nuclear Regulatory Commission (NRC), to control commercial activity, and the Energy Research and Development Agency (ERDA), which would become the Department of Energy (DOE) promoting civilian use of nuclear energy and managing both nuclear waste from commercial and military activity. Physicists seemed to have assumed that radioactive material could be handled safely under federal regulations and by competent engineers, placing faith in institutions outside their realms of knowledge and control.

In spite of these accusations, a chronology of events suggests that the AEC at least planned to take timely actions to manage wastes. For instance, it promptly requested advice from the National Academy of Science; in 1957 the NAS announced that a solution was to bury the waste in salt.[3] In 1963, when the AEC awarded the first contract for building a commercial reactor, it issued a permit for a plant to reprocess spent fuel that was to cool in storage at reactors for five years. It also began a timely experiment for the disposal of the residual waste in salt in Kansas. By 1971, the AEC spent some $100 million studying bedded salt.[4]

A combination of unexpected events and limited understanding of local geology upset its plans; local knowledge also played a role. For instance, waste from a plutonium production plant in Rocky Flats, Colorado, was injected into deep drilled holes and caused seismic tremors until local citizens recognized this cause and stopped it.[5] Then in 1969, a fire badly damaged that plant and set in motion a series of disastrous events. The AEC hastily relocated waste to its reservation at Idaho Falls. A trout farmer realized that radionuclides could seep into groundwater through the highly fractured rock and migrate to the surface; he notified the Governor. Idaho's Senator Frank Church then demanded that all this waste be removed by 1980.[6]

The AEC logically turned to its Kansas experiment for a solution, only to be confronted by state geologists. Inadequate knowledge of that particular site ended this idea in 1972.[7] The AEC then turned to the U.S. Geological Survey (USGS) and found a site in New Mexico, a state long supporting nuclear activities, where a Waste Isolation Pilot Project (WIPP) was begun and is still underway.

Meanwhile, the first plant designed to actually reprocess spent fuel, in West Valley, New York, was plagued with unexpected problems. One was a lack of suitable fuel that forced it to operate below capacity and to treat material from experimental reactors, which damaged the chemical process.[8] Again the AEC's timely plans had gone awry. The plant closed in 1972 for repairs; it never reopened but left a legacy of legal problems and intractable waste, while spent fuel overcrowded cooling pools at commercial reactors.[9]

The AEC then proposed temporary storage of waste in special mausolea above ground until it cooled. Environmentalists feared that these would become permanent and rejected the proposal. When ERDA proposed placing waste in deep holes drilled in various types of rocks in any of 36 states, it aroused wide-scale resistance, leading many states to ban waste-related activities within their boundaries. Public protests grew against all types of nuclear activity, as did a demand to halt licensing new nuclear power plants until the waste disposal issue was settled.

New institutional and technical difficulties arose. Reprocessing came under Presidential scrutiny and was halted indefinitely by President Jimmy Carter early in 1977. USGS scientists began raising questions about actual salt deposits, such as unexpected heterogeneities and brine inclusions, which could migrate to and rapidly corrode waste containers. In this context, the President asked DOE to assess the problem as a first step in devising a sound administrative policy and program to deal with all types of nuclear waste.

The Institutional Problem

Nuclear waste is frequently said to be no technical problem but an institutional one. Many interest groups within diverse perspectives focus on particular facets of the aggregate problem of nuclear waste, engaging in what seem like irresolvable conflicts -- deemed the "institutional problem." This way of looking at the institutional problem seems too fragmented and narrow, ignoring how closely technical and institutional matters interact.[10] It also ignores the larger institutional framework in which nuclear wastes are generated. Finally, it overlooks the routine ways in which people in fine-scale social institutions in all sectors and at all levels of society, make decisions that foster waste production. The following paragraphs describe the most important aspects of and distinctions made within this complex institutional framework.

For instance, the technical community justified delays by claiming that wastes were at the "back end" of the fuel cycle and lacked the urgency of producing weapons or power plants. With characteristic optimism, it expected a "technological fix" eventually. This phrase, back end, is misleading; radioactive residues are produced throughout a lengthy process, from dusty mill tailings at uranium mines, as gases and liquids released in refining or processing fuels, as heterogeneous solid "low level waste" (LLW) from research, medical practice, and reactor operation, as "high level waste" (HLW) directly from the fuel in reactors, or as transuranic elements (TRU) from reprocessing that fuel, and finally as massive refuse from unusable reactors, most aptly at the tail end of a technical process.[11]

This list of types of wastes must be further subdivided between commercial wastes, generated by the private sector, and military waste created largely in the production of weapons for national defense, in which the Armed Services Committees in Congress and the Department of Defense have major interests. High level defense wastes have been temporarily and inadequately stored mainly at three federal reservations.[12] No formal procedures exist for public review of military waste management in order to avoid a "threat to national security," or more properly to avoid public interference and delays in defense activity.[13] Thus public understanding and discussion of this part of the larger institutional framework has been limited.

The most hazardous commercial waste is the spent fuel from plants generating electric power, a function long supported by federal energy policy to protect the nation against the uncertainties of imported oil. Here disaggregated consumer decisions about the use of electricity directly and indirectly foster continuing production of waste. The well organized nuclear industry, made up of utilities in more than 30 states and their suppliers, had long treated the residual spent fuel as a resource to be reprocessed to extract valuable uranium and plutonium for re-use, leaving small amounts of TRU for disposal. Carter's ban on reprocessing created new technical and institutional problems. Technically, whole fuel assemblies are greater in volume, initially hotter, and more hazardous for workers to handle, than TRU waste. The industry was also loathe to accept the ban and argued that fuel should be stored retrievably for later re-use. The planners would mute such debates on technical grounds by classifying spent fuel as "high level waste."

When loosely organized coalitions of environmentalists and antinuclear activists began to take political and legal actions in the 1970s, they posed a dilemma for a government that supported the nuclear industry. To extricate itself, the waste policy planners split the issue of waste from the future of nuclear power and declared themselves neutral on the future of nuclear power, placing this topic, and the antinuclear activists, outside the bounds of discussion.[14]

But behind this neutrality is a questionable premise, that once a technical solution to the waste problem was found, it could be used for any amount of waste.[15] Indeed, nuclear electric power was then expected to increase rapidly and to produce more waste than military activity, then expected to decrease in a climate of lessening international tension.[16] But the validity of this premise depended upon many factors that were unforeseeable in the larger institutional framework. In any case, such distinctions compartmentalized the system of generation through disposal and enabled the planners to limit discussion of major parts.

Federal government programs for managing wastes involve complex arrangements. Eight Congressional committees deal with facets of waste; on the administrative side at least fourteen departments and agencies have interests.[17] Three key ones are expected to interact in a kind of logical sequence are: the Environmental Protection Agency (EPA) to assemble regulatory standards and guidelines to protect public health and safety, the NRC to follow with rules and procedures for public review and licensing of commercial activities and to govern DOE, the operating agency and in principle the last in line. But EPA's work has lagged behind schedule because of debates about how to set regulatory standards, in part because of uncertainties in such areas of knowledge, as radiobiology and ecology. Meanwhile DOE has tended to forge ahead with programmatic momentum, reversing the expected sequence, and creating the possibility that it would not be able to afford to make costly changes to meet more stringent standards.[18]

The U.S. Geologic Survey (USGS) became a fourth key agency at the start of the Carter administration when it questioned long held assumptions about the suitability of salt deposits for containing radionuclides. Other agencies with central roles were the Department of Transportation, on shipping wastes, and the Department of State, determined to accept spent fuel from abroad to prevent nuclear proliferation.[19]

Organizationally, within DOE, waste management programs for military and commercial waste not only had separate budgets but were further fragmented by separate contracts with decentralized national laboratories. Their records on stored wastes were often incomplete and their data in different forms, difficult to compare. These labs were managed by old-timers from the AEC, determined to "prove" the "sacred theory of salt" entrusted to them by scientists. They were defensive at criticism by the USGS and accused it of wanting to enlarge its turf in order to build scientific knowledge and of not appreciating that engineers need only approximate numbers.[20]

In this context, Carter's policy planners focussed on high level wastes and TRU, considered to be technically the most difficult and politically essential to protect the nuclear industry and old promises. They left to Congress the responsibility for unstabilized mill gailings, highly hazardous now and far in the future.[21] At the request of State Governors, low level waste would be hendled by the states under new legislation.[22] "Decommissioning" old facilities and reactors, not yet an urgent matter, would be treated rather superficially.[23]

The Policy Making Process

When ERDA was reorganized late in 1977, DOE was deemed the "lead agency" on programs for nuclear waste, to overcome the impression that responsibility was split with the NRC.[24] Its first task was to assess the issue, under the leadership of John Deutch[25]. Early in 1978, the "Deutch Report" called for the federal government to take responsibility for all types of waste, to license all long-term disposal facilities, and do what was necessary to assure public confidence that all types of wastes would be disposed of safely.[26]

This report created a positive climate for policy making. Environmentalists praised it as a good first step.[27] At least three western States indicated support for hosting the first repository.[28] Congressmen took new interest in the topic.[29] Such conditions offered reasons for optimism and opportunities for building a constituency, based on compromises, for a new approach to this old problem.

However, the Deutch Report revealed a basic conflict that would divide the planners to the end, labelled "timing versus certainty."[30] DOE urged rapid timing in completing WIPP for TRU waste; it was also willing to add a small licensed experiment at WIPP for retrievable storage of spent fuel, to show the public that action was being taken. It proposed opening several small licensed research and demonstration projects prior to the first full scale repository. These were called intermediate scale facilities because successful isolation could not be demonstrated for thousands of years.

The USGS was leery about any repository in salt and wanted WIPP halted. OSTP particularly wanted DOE to project a new image by proceeding slowly step by step with intensive research, investigating many sites in other media, to provide sound scientific knowledge for site selection and engineering design. Such programmatic redundancy should preclude repeating a time-consuming series of fiascos and produce at least a few politically acceptable sites.

We posit that this conflict was based on two very different implicit models of how engineering knowledge is acquired. DOE's approach seems more like that of practical engineers in the past who obtained initial understanding of unfamiliar matters by conducting small experiments in the real world. OSTP's approach was more like that of modern engineers who claim that their knowledge involves the application of general laws and principles derived by scientific methods. Moreover, this modern approach was essential, given the limits in assessing the results of full-scale experiments.

Early in 1978, President Carter established an Interagency Review Group (IRG) made up of representatives of fourteen departments and agencies, chaired by Deutch and directed by a small closeknit steering group that included OSTP.[31] The IRG allocated analytical tasks to six groups, each to work on a part of the policy. OSTP was asked to assess the status of geologic knowledge in addition to a most important task, assessing alternative technical strategies.[32]

Six technical strategies had long been proposed: to put nuclear waste under the sea, eject it into space, insert it in deep drilled holes or in mines to melt with the rocks, bury it in massive graves, or chemically partition it and bury the residue. OSTP chose massive burial because time was vital to eliminate the threat to new nuclear power plants and burial would be "available soonest."[33]

For both its tasks, OSTP followed an impersonal procedure used in situations of great uncertainty in science and that may be described as a modified Delphi process.[34] It widely circulated drafts of reports for IRG comments and then revised them until comments abated, and a consensus could be assumed.

In early Fall, the task groups completed their work and the IRG issued a draft report that indicated new understanding and significant consensus on many points. Environmentalists praised it as a welcome change from tired rhetoric.[35] For instance, DOE agreed to study various media and look closely at specific sites. It would also now consult with the States and seek concurrence before it opened any repository, although this was not required by legislation. Licensing would be extended to some noncommercial waste facilities, including WIPP, a decision reached without consulting the Armed Services Committees, much to its dismay.[36]

Considerable emphasis was placed on adequate public participation, a point stressed by the Council on Environmental Quality (CEQ), which made a plan for "public input" into the IRG. The plan started with "going public" in meetings with representatives of special interest groups in Washington, as had been done for Earthquake Hazard Reduction planning. Three public hearings were then held across the country during the summer. Unfortunately only short notice and incomplete position papers were available for these and the IRG was roundly criticized.[37] The final phase of the plan was to distribute the draft report, eventually 15,000 copies. The comment period was extended into 1979 to accommodate demand. Staff then analysed and sorted 3,300 responses into categories, attempting to tabulate pros and cons on more than 40 separate sections of the IRG report, and then summarizing these comments and drafting IRG responses.

The comments raised many new issues. They also often reflected readers' confusion with language in the report. For instance, CEQ, a stickler for compliance with the National Environmental Policy Act (NEPA), insisted that IRG decisions not prejudge but preserve policy options. Therefore these options had to be couched in terms of procedures for decision making to avoid their being subject to environmental impact reviews. The wording of the four "interim strategic planning bases" mainly on the timing of decisions was so abstract that even IRG members were initially confused about their differences.[38]

Many commentators wanted to extend licensing procedure for commercial waste to military wastes consistent with their physical and chemical properties. Their questions about the actual threat to national security raised basic issues about civilian control of the military that have plagued the nation since its early days.[39] The IRG largely ignored suggestions for alternatives to licensing or for decisions made on a case-by-case basis. The IRG stuck with licensing new facilities for high level waste, which would only be used in the unlikely event that these would be needed, and for TRU, unnecessary with WIPP and the ban on reprocessing.

The IRG modified its stance on several issues, notably on opening small licensed research facilities, in part because some feared these would grow into permanent repositories. Moreover, "Every HLW repository will in its early phase be an ISF" and, based on an optimistic schedule for opening the first, would provide as much information. "Let's junk the whole concept once and for all."[40] This was done. DOE now conducts experiments without public review.

Especially controversial was a concept of "consultation and concurrence," what it meant, how it differed from a State veto over site selection, disallowed under existing laws, and how it would work. This led the IRG to discussions of federal-state relations and of detailed procedures that took it far beyond more practical matters at hand.[41] One remedy for federal-state relations seemed to be a new institution, a State Planning Council, establishing an "equal partnership" of federal administrators and selected governors, plus one representative each from a county, city, and Indian tribe. The council's main task would be to identify those "decision points" in which all had an equal say.

The most comments and greatest confusion arose in a statement on the status of geologic knowledge for mined repositories, to be described.

Even more important was that the array of comments opened up fresh debates on broad political philosophy and on procedural details thought to have been settled, apparently testing the patience of many. As one critic said, the basic error of the President's immediate advisors lay in their "dogged determination ... to raise and re-raise their points of view in terms more specific than was needed."[42] As patience waned, the spirit of compromise dissipated, and valuable time elapsed.

The IRG report to the president, weaving comments and IRG responses into the draft, was finally issued in March, 1979. But the new conflicts soon split apart the small group that had steered and energized the IRG. Deutch had become a favorite in Congress for his lucid testimony and was moving up through the DOE bureaucracy. OSTP began looking to the President as leader and teacher and the final arbitrator of disputes.[43]

Then apparently with little warning, DOE withdrew its offer to license an experiment with spent fuel at WIPP. Supported by the Armed Services committees, it would proceed rapidly to open an unlicensed TRU repository in New Mexico. Another surprise was the accident at Three Mile Island in March 1979, which shifted public attention from issues of waste to issues of power plant safety. At that time a Presidential statement only on nuclear waste seemed ill-advised.

The justifiable delay allowed a small group centered around OSTP to squabble over the wording of material on issues to be sent to the

President[44] One residual issue, the extension of licensing, was sent to a study committee. Finally, only two issues remained: on continuing WIPP and the initial one implicitly of "timing versus certainty," or of how many sites would be closely studied before a first was chosen. Material was given to the President's staff early in June but did not reach his desk until late August because of vacations and meetings abroad.[45]

The decision memo gave the President a choice between characterizing two to three or four to five sites before the first was chosen. He solved that simply with a mathematical compromise, suggesting three to four. Time elapsed in clarifying the underlying philosophical differences between a rapid program of practical action and a more conservative redundant program of slow and costly research. Then Carter's new Secretary of Energy backed an unlicensed WIPP; the President faced a dilemma. If he fought against WIPP as OSTP advised, he could alienate Senator Frank Church, whose support he needed for a pending disarmament treaty. Carter took the chance and lost both the ratification of the treaty and his ban on WIPP.[46]

On February 12, 1980, OSTP's work was finally completed when the President stated the administration's nuclear waste management policy. Some said that at best the policy echoed the consensus in September 1978, which no longer existed, that it was weak and untimely, or that it was simply a plan for planning, delaying tangible action.[47] The policy statement received little notice in the midst of the Iran crisis. Carter did appoint a State Planning Council, but his staff, pre-occupied in an election year, shelved an OSTP implementation plan and ignored its advice that an official be appointed in the Executive Office, at least temporarily, to oversee the waste management program.[48]

Problems of Knowledge

From the start, Deutch and others recognized the unprecedented challenge of the technical task of waste isolation but seemed confident that adequate research could fill the gaps in knowledge. Their primary concerns were about geologic knowledge for mined repositories. As a field of science, geology offered a perspective of time commensurate with the life of many rationuclides, but it was a retrospective science, focussing on the past and, like all earth sciences, lacked the theory necessary for prediction. As one geologist said, "There is no philosophical or logical basis for predicting the frequency of geological events or the intensity of geologic processes. Moreover, geologic processes often destroyed empirical evidence about past events.[49]

Building theory was considered especially difficult when the object of study was a continuous earth that limits controlled experiments. In contrast, nuclear physics, for instance, has ample particles for controlled experiments to advance its theory. Moreover, OSTP never questioned the adequacy of the general laws of physics; implicitly problems lay only in obtaining specific data on how the laws would operate in particular situations.[50]

On the other hand, biological scientists were less than sanguine about their understanding of the causal chains with many links, from exposure to phenomena that cannot be directly observed to lethal and mutagenic affects. Biologists disagreed wisely as new research indicated that previous exposure standards had been too lax, and their disagreements fed public anxieties. Moreover, even with reliable statistics on aggregate affects of exposure to given doses of radiation, biologists cannot predict individual cases.[51]

The IRG barely mentioned hazards to workers transporting and handling wastes, leaving these and other problems prior to closing a repository to the engineers. For instance, pumps, fans, and spacing of waste cannisters could be used to handle such contingencies as mine flooding, excessive heat, gases in the medium, or waste becoming "critical," e.g. exploding.[52] Long term biological impacts were not discussed in detail, apparently because a repository was expected to isolate material for several thousand years; by then biologists would have a technological fix.

The small group in OSTP that assessed the status of geologic knowledge for mined repositories, soon simplified its task by dismissing scenarios of release that seemed least likely, such as that wastes would be uncovered by meteorites, or that involved "the human problem." This latter was the most difficult to characterize, for instance, knowing whether men would later exhume wastes in the search for minerals. Instead, the group focussed on the most credible scenario of release, in the movement of water to the surface.[54] They structured this scenario with a kind of transport model. Put simply, the rate that the water would move and the length of its path would determine the travel time, hopefully long enough to allow radioisotopes to decay to harmless levels.

To characterize this process, knowledge was needed from three fields, roughly matching three scales of time and space into which the problem was divided. Geochemistry would address near-term interactions between the waste form and nearby rock in the repository, rock mechanics would describe the effect of heat on the site in the thermal period, and geohydrology would deal with the gradual long-term movement of water up to the surface. One difficulty in this subdivision was that that different fields operate at different scales and use different methods to measure the same properties, such as permeability or stress in rocks. The results are often inconsistent. Another problem lies in correlating laboratory results with field measurements.[55]

Each field also faced particular limitations in predicting even generally what would happen. Geophysics, the most mature theoretically, recognized that elements in nature possess a wide variety of changeable properties; even simple crystals of salt are too complex to be characterized by a few uniform attributes. The challenge was to model innumerable combinations of physical and chemical interaction among naturally occurring elements and in combination with an array of man-made radioactive particles. The dangers here were in oversimplifying and making misleading models, on the one hand, or, on the other hand, of errors propogating as the models were made more complex.[56]

The most hazardous stage of a repository might be when the heat expanded and fractured the surrounding rock along planes of weakness, possibly uplifting the surface. When the rock cooled, openings could remain through which water could enter and radionuclides escape. To learn in advance about weaknesses in a particular rock mass required drilling boreholes, but that created a dilemma: each new hole made the data more reliable, but each also progressively weakened the integrity of the site and its usefulness for containment. Since every rock mass is unique, neither a study of analogous rocks nor data in generalized models derived from study of many similar rocks would do. Analogies and extrapolations from models could miss features of vital interest at a particular site.

Many radionuclides were expected to be captured by, or chemically bonded to, elements in the surrounding rocks, but others would dissolve in water existing within or near all sites and decay as the water flowed to the surface. But geohydrologists did not understand well the three-dimensional flow of groundwater at even the best known aquifers.[57] They could study selective short term retardation of some elements at sites where waste had been leaching into the ground for up to forty years, as at Hanford. But extrapolating from these to another region and over thousands of years could not be done with confidence.

Given such problems, the best the planners could do was to make conceptual models of the most important factors and their possible interactions and to use mathematical techniques such as sensitivity analysis to evaluate the significance of major variables. [58] There would also be a need for a risk analysis if only to satisfy EPA regulations, which require a summary number on the level of risk prior to review and licensing of proposals.

Meanwhile geologists could study the general attributes of rocks in various regions, a task multiplied by the need to study six types of media and also many specific sites.[59] Ultimately the media, site, and design of the repository, waste form, and packaging were expected to function together as a system of multiple geologic and man-made barriers or redundant defenses, which are common in conservative engineering. Because of unique characteristics of sites and types of rocks, many design decisions could obviously not be made until a first site finally was selected.[60]

OSTP decided that the site selection process could be simplified by devising lists of technical criteria by which to characterize media and general locations and to evaluate specific sites. Such criteria would provide a basis, if not for an absolute judgement, at least for rational decision making, comparing and ranking sites, and choosing the best among them. OSTP concluded that current knowledge and modelling capacity was not yet sufficient to permit confidence in the safety of any particular repository design nor the suitability of any particular site.[61] This somewhat bleak conclusion was softened by the expectation that time, investment, and study would yield the necessary knowledge.[61]

OSTP summarized its conclusions on the status of knowledge in two paragraphs in the IRG draft report. Responses varied widely and were divided generally into pessimists and optimists. Some readers voiced distrust of any statements by scientists, especially after their exaggerated claims for salt. Others thought that the optimistic tone of the conclusions was not warranted by the full report nor by other studies. Specific doubts were expressed about the value of models and about risk analysis, as on whether uncertainties could be "bounded." A few felt that knowledge was insufficient even to begin a search for specific sites. Others argued that knowledge was or would soon be sufficient; the program should simply move ahead.[62]

For the report for the President, OSTP completely rewrote its conclusions, dropping references to risk analysis. It emphasized that present knowledge was adequate to identify sites for further study and to assess their suitability against predetermined criteria. Information would become more complete as a site was excavated and operations began. Two new provisos were added: work could stop at any time until a repository was "decommissioned' or closed, but even then some uncertainties would remain. Second, the choice of a site would require not only technical judgment but also a "societal judgment" on the level of risk and uncertainty.[63] That this judgment would be made through licensing procedures was clear; less clear was what "society" would know to make this judgment and how it would express itself.

The Aftermath

Events in the next two years revealed again that neither the defeats nor the victories were final. Late in 1982, Congress passed the Nuclear Waste Policy Act, incorporating many IRG concepts. It outlined elaborate procedures: DOE was to study at least five sites before recommending three choices for both a first and second high level waste repository, but under a tight schedule that negated OSTP's intent.[64] President Ronald Reagan dropped the ban on reprocessing (a technology now stymied by financial and other problems) and allowed the State Planning Council to lapse.

Programs to deal with other types of waste did not fare well. The uranium mining industry, which once supplied over 90% of the free world's market, was in financial straits and tried to shift responsibility back to the federal government for masses of tailings abandoned near inactive mines in seven western states.[65] With increased defense activity, military waste has accumulated more rapidly than the spent fuel from a battered nuclear industry, but consolidation of leaking military waste faced financial battles.[66] The states, which had chosen to manage low level wastes, have had difficulties working together in regional compacts and agreeing on new sites. One exception was a site offered in Colorado by unemployed uranium miners accustomed to the hazard.[67]

The choice among three strategies for decommissioning aging or unusable power reactors, such as that at Three Mile Island, has vanished, largely because of problems of knowledge. A graduate student discovered that an unforeseen and long-lived radionuclide could leak from reactors entombed in concrete, the cheapest option and preferred by utilities. "Mothballing" a facility to cool before dismantling was scratched when formal records were found to lack vital information. Only engineers long at a plant could have such knowledge but would be long gone when the plant was cool enough for disassembly. The remaining option, immediate dismantling, is the most hazardous to workers and most expensive, and obviously puts pressures on utilities to operate plants beyond the thirty years for which they were designed.[68]

DOE sped up its work on WIPP on a double track of research and excavation, running roughshod over local anxieties, exacerbated by IRG vacillation.[69] The host State, New Mexico, now lives with a dilemma: it cannot demand NRC review and licensing without becoming eligible to be the nation's first or only repository for spent fuel.[70]

While the IRG was still deliberating, DOE had identified seven sites for a first high level waste repository and began reviewing geologic data for a second. But technical and political problems proliferated as more was known of the geology and hydrogeology. Sites in domed salt have subsequently been dropped because of difficulties in characterizing irregularities in the subsurface material. Utah blocked exploration by simply denying permits to DOE to move its equipment over State roads. A site at Hanford, Washington was preferred because it was on federal land and already contaminated, even though the poorest geologically. On that site, experts widely disagreed in interpreting geologic data.[71] Although many local businessmen welcomed funds for exploration, by 1985 the three remaining candidate states had filed lawsuits to halt DOE site studies.[72]

On January 16, 1986, DOE announced that it would seek a second site from 18 areas in seven eastern states. But on May 28, the President halted this phase, claiming that the volume of spent fuel accumulating did not warrant a second repository. Environmentalists, especially, accused Reagan of bowing to political pressures from Congressmen and Governors during an election year.[73] This diagnosis oversimplifies what actually happened and is ironic, coming from a group that acquired power through broad public participation but now relies on institutionalized laws and procedures. A once apolitical housewife told a story of a small New England town that is probably not atypical of other sites.[74] The story follows.

Hillsboro, New Hampshire, first learned of the government plan from televised news. Elected officials then told them to prepare to testify at hearings on technical matters of interest to DOE and to refrain from emotional expressions. A DOE official explained that emotions and social concerns are difficult to quantify and record in technical documents and a geologist admitted that the DOE selection process somehow neglected the well-being of local people. On the other hand, the community was urged to make personal sacrifices for the public good.

Local scepticism mounted when people remembered being told a decade before that waste from the Seabrook power plant proposed nearby would not be a technical problem. But now DOE could not answer many of their technical questions. Public confidence further waned after the Challenger disaster and the news that U.S. officials had suppressed reports of a holocaust at a waste site years before in Russia. Chernobyl brought reality to the magnitude and subtlety of radioactive hazards.

Instead of being torn apart by local conflicts and controversies, as state and federal officials had expected, this community quickly united, informed itself on technical and political issues and behaved in a cohesive and socially responsible manner. Apolitical rural residents joined a spectrum of social and political interest groups to raise technical questions and express social concerns at DOE hearings; they also contributed local knowledge.

For instance, one old-timer shared his intimate knowledge of occasional springs and hidden wells, which might transport radionuclides into surface water supplies. A trucker warned of icy patches in winter and potholes in spring that could cause accidents in transporting wastes. A housewife speculated that granite containing hot waste might behave like her pressure cooker, left too long on the back of the stove, and explode. A six year old boy linked what IRG neutrality had attempted to decouple and expressed dismay that the stuff would still be made at Seabrook.

The author herself, no technician, thought that storing the waste above ground to cool for 50 to 100 years was the best alternative, a strategy that international experts preferred.[75] The IRG said that they rejected storage on the grounds that the generation that benefits should bear responsibility and costs. The real reasons, of course, were political.

The combination of questions, concerns, and new understanding from this and other local areas flowed up to State governors, Congressmen, and into the President's office.[76] Congress subsequently slashed funds for seeking a first site, at a time when DOE was being accused of mishandling even technical matters, e.g. improperly classifying cores at candidate sites.[77] Congress then scrapped the legislated procedures and chose to explore only a single site in Nevada. Less than a decade after work began on the policy, it now seemed undone.

Techniques for evaluating sites and for risk analysis have not fared well. DOE devised a multi-attribute utility technique for evaluating options for the first site, and asked the National Research Council to do an independent assessment. The NRC approved the method but expressed concern that the technique "demands scrupulous methodical implementation" and could be used subjectively to mask the real uncertainty of a repository's ability to contain radionuclides.[78] If this technique is no better than risk assessments being done on the now familiar technology of nuclear power plants, there is reason for concern.

Experts have found sophisticated models of risk so complex and arcane as to confound critical review. Many are based on obscure but untenably optimistic assumptions, oversimplifying specialized knowledge or using it inaccurately, or omitting realistic possibilities of accidents so unprecedented that they cannot be described.[79] Yet these techniques are expected to be the basis for a societal judgment on the acceptability of risk.

Conclusions

This federal effort floundered, we posit, largely because OSTP had poorly understood both the physical and social reality. Just as the AEC's plans unraveled after the unexpected accident in Colorado, followed by inappropriate organizational responses combined with limited understanding of local geology, so the IRG's plans began to fall apart when local people responded to a dreaded hazard and acted together in a poorly understood social reality.

The IRG also ignored the larger institutional context in which nuclear wastes are generated. It bounded out of discussion those anti-nuclear types most concerned with wastes, and also its potential allies in Congress and potential supporters in states that might have willing hosted a repository. Thus it lost opportunities for building a broad constituency for a robust program.

Opportunities did exist for a multiple but messy approach, for instance, combining both models for acquiring engineering knowledge, through small experiments and with scientific research. Could not the planners have compromised so that DOE's willingness to experiment with spent fuel at WIPP was combined with a slower process of licensing reactors, and a policy of energy conservation and long-term surface storage to slow the generation of wastes and buy time for scientific investigation? Why not involve a spectrum of local groups, pursuing local site investigations and monitoring small-scale experiments and storage? But an approach based on messy compromises and multiple local incremental programs with broad public involvement would have violated the expectations of a simple clear cut solution based on predictive knowledge and firm control by the federal government. Yet that is what mainstream scientists, engineers, and the public expect. We now turn to an overview of these three projects.

NOTES

1. See Ted Greenwood, "Nuclear Waste Management in the United States," in William E. Colglazier, Jr. ed., The Politics of Nuclear Waste, (New York: Pergamon Press, 1982), and also the series of articles by Luther J. Carter in Science listed in References.

2. Edward J. Woodhouse, "The Politics of Nuclear Waste Management," in Walker, Charles A., Gould, Leroy C., and Woodhouse, Edward J., eds. Too Hot to Handle? (New Haven, Conn.: Yale University Press, 1983), 157-83.

3. National Academy of Sciences, - National Research Council. The Disposal of Nuclear Waste on Land, A Report of the Committee on Waste Disposal. April 1957. Salt deposits were recommended as plentiful, dry, durable, and plastic, thereby sealing in waste, and simple to mine by familiar technology.

4. Ronnie D. Lipschutz, Radioactive Waste: Politics, Technology and Risks. A Report of the Union of Concerned Scientists, (Cambridge, Mass.: Ballinger, 1980), especially Chapter 4. "The History of Radioactive Waste Management,"113-138.

5. Mentioned in the previous chapter.

6. H.P. Metzger tells the story of the trout farmer in The Atomic Establishment, (New York: Simon and Schuster, 1972) 150-154.

7. Susan Fallows, "The Nuclear Waste Disposal Controversy," in Dorothy Nelkin, ed. Controversy: Politics of Technical Decisions, (Beverley Hills, Calif.: Sage Publications, 1979), 97-111.

8. Lipschutz, 122.

9. Fallows, passim

10. Thomas H. Moss, "What Happened to the IRG? Congressional and Executive Branch Factions in Nuclear Waste Management Policy," in Colglazier, William E., Jr. ed. The Politics of Nuclear Waste, (New York: Pergamon Press, 1982), 98-109.

11. A somewhat arbitrary standard of curies or heat of radiation per volume distinguishes high from low level wastes, which may include hot spots. Transuranic wastes contain man-made elements larger than uranium, which decay slowly and initially are not as hot and easier to handle than HLW. Lipschutz, 29-54, provides a clear description of the nuclear fuel cycle.

12. Hanford, Washington, Idaho Falls, Idaho, and Savannah River, South Carolina. Several forms of storage: as liquids, sludge, salt cake, and dustlike calcine, each posing advantages and disadvantages for disposal.

13. U.S. Department of Energy, Report to the President by the Interagency Review Group on Nuclear Waste Management, Washington, D.C., 1979. 29-30. Hereafter IRG Report. It stated that licensing military waste are that it could lead to disclosure of national security information and delay our ability to respond to changing world situations.

14. IRG Report, 5-8.

15. Ted Greenwood provides this insight.

16. IRG Report, 11-12 and Appendix D 1-32 describes projections of nuclear wastes.

17. Moss, 102.

18. Greenwood, 18.

19. The policy of nonproliferation aims to prevent plutonium from being extracted from spent fuel to make nuclear bombs.

20. David B. Stewart, USGS, letter to Les Dole, Project Manager WISAP, Oak Ridge, Tenn. September 30, 1977.

21. The Low Level Waste Radioactive Waste Policy Act of 1980. P.L. 96-573.

22. The Uranium Mill Tailings Radiation Control Act, November 1978.

23. IRG Report, 84-86.

24. Phil Smith memorandum to Ted Greenwood, August 25, 1979.

25. John Deutch came from MIT to direct DOE's new office of Energy Research.

26. U.S. Department of Energy. Report of Task Force for Review of Nuclear Waste Management, Draft. Washington, D.C. February 1978. Hereafter called the Deutch Report.

27. William D. Metz, "New Review of Nuclear Waste Disposal Calls for Early Test in New Mexico," Science, 199 (31 March 1978); 1422-1433.

28. Luther M. Carter, "Nuclear Wastes: Popular Antipathy Narrows Search for Disposal Sites," Science, 197 (23 September 1977) 265-1266. pointed out an apparent political acceptance of nuclear waste disposal in three states, where business and political establishments had grown accustomed to nuclear activities more threatening than buried waste: Washington, where plutonium production began in 1944, then governed by Dixie Ray Lee, a former AEC Chairman; New Mexico, where the first A-bomb was detonated, and Nevada, where weapons have been tested for three decades. All contained antinuclear groups, but there was not much outcry against waste storage or disposal.

29. For instance Luther J. Carter, "Congressional Delegation Ponders Where to Give States Veto." in Science. 9 June 78, mentions that Congressmen Gary Hart of Colorado, Morris Udall of Arizona, and Peter Domenici of New Mexico all supported the Deutch report's position on extending licensing, in opposition to the Armed Services Committees.

30. Deutch Report, 11-12.

31. IRG Report, Appendix A, contains the President's letter and list of representatives to the IRG.

32. Interagency Review Group on Nuclear Waste Management. Subgroup Report on Alternative Technical Strategies for the Isolation of Nuclear Waste, Draft. "Isolation of Radioactive Waste in Geologic Repositories." October 1979. Appendix A: "The Status of Geologic Knowledge for Mined Repositories." This Appendix is hereafter called "Appendix A" and in the text "the status of knowledge."

33. IRG Report, 35.

34. In this procedure, a group of experts are individually asked for their best estimates on some uncertain matter, then shown the distribution of responses and given an opportunity to revise their estimates. This process is repeated until, hopefully, the estimates converge. Here for instance, an early draft on geologic knowledge was deemed too pessimistic, and urged to cite research proposed and underway. The final draft listed 89 projects.

35. Luther J. Carter, "'Cooperative Federalism' Proposed for Siting Waste Repositories." Science, 202 (3 November 1978) 501-502.

36. Moss, 106.

37. IRG Report, Appendix C.

38. Gregory Baecher, in letter to Ted Greenwood, September 14, 1978, wrote, "I think the problem of major importance is that the strategy options are ambiguous. I thought I understood what they were, but I found out that everyone at the 9/12 meeting, while also thinking they knew the options, had very different ideas about what they were. Reading the paper itself, without trying to read in thoughts, I find myself confused."

39. Moss, 106.

40. Isaac Winograd comment on memorandum from Greenwood to the IRG, "Draft of Decision Issues for the President's Decision Paper." February 9, 1979.

41. IRG Report, 94-95.

42. Moss, 104.

43. Philip Smith in a letter to John Deutch, December 7, 1978, referred to the President as "leader and teacher." Smith and Greenwood in a letter to Frank Press, January 12, 1979, noted that Deutch's testimony before Congress was inconsistent with IRG positions and that his assistance in holding DOE together was no longer needed now that they were moving toward the President. Deutch would become Assistant Secretary of DOE.

44. Luther Carter, "Radioactive Washington Policy in Disarray." Science. 206 (19 October 1979): 312-314.

45. Ted Greenwood in a memorandum to Frank Press, August 20, 1979, "Talking Points for use with Stu Eisenstat and Eliot Cutler on the Status of the Nuclear Washington Decision Paper."

46. Luther Carter, "Carter Says No to WIPP, but DOE May Appeal." Science. 206 (14 December 1979): 1287.

47. See, for instance, Edward I. Woodhouse, "The Politics of Nuclear Waste Management," in Charles A. Walker, Leroy C. Gould, and Edward J. Woodhouse, eds. Too Hot to Handle? (New Haven: Yale University Press, 1983), 169.

48. Greenwood, 26.

49. William C. Luth, "Proposal for Earth Sciences Research." prepared for John Deutch, January 30, 1979. Deutch praised Luth's paper as "outstanding." See Luther Carter in "Nuclear Waste: The Science of Geologic Disposal Seen as Weak," in Science 200 (9 June 1978): 1135-1137. Luth also called for major changes in management or management philosophy in Washington and in the labs for new blood in research, and for more than "paper studies" evaluating alternatives for decision making.

50. Greenwood made these points in a conversation in November, 1987.

51. See for instance, Charles E. Land, "Estimating Cancer Risks from Low Doses of Ionizing Radiation." Science. 209 (12 September 1980): 1197-1203. "Reasonable men have disagreed by as much as a factor of 100 or more" over the risks from exposure to a single rad. Difficulties arise from the need for impractically large samples up to a 10 million and in extrapolating the shape of the curve from high- to low-dose data.

52. Appendix A, 12.

53. The paragraphs in the next few pages are drawn from a dozen drafts of Appendix A from May 9 onward. For a recent overview of problems of geologic knowledge, see A.G. Milnes, Geology and Radwaste, Orlando Fla.: Academic Press. 1985. particularly Part 2, on the complex problems of describing coupled thermomechanical, hydrogeological, and hydromechanical processes.

54. Appendix A, 15.

55. Luth, 5.

56. Ibid, 11.

57. Gregory Baecher comment on June 9 draft of Appendix A.

58. Appendix A, 18.

59. Ibid, 60-81.

60. Ibid, 27.

61. IRG Report, 38.

62. Ibid, 37-43.

63. Ibid, 42. See Luther J. Carter, "Interagency Group Cautious on Nuclewar Waste Disposal." Science 203 (30 March 1979): 1320.

64. P.L. 97-424. Also see Luther J. Carter, "Waste Bill Approved." Science 219 (7 January 1983): 35.

65. Mark Crawford, "Mill Tailings: A $4-Billion Problem." Science 229 (9 August 1985): 537-38.

66. Matthew J. Wald, "Report Assails Safety of Nuclear Waste Storage at Carolina Plant, New York Times, July 24, 1986.

67. Norman Colin, "High-Level Politics over Low-Level Waste." Science 223 (20 January 1984): 258-260.

68. Norman Norman, "A Long-Term Problem for the Nuclear Industry," Science: 215:376-379. 22 January 1982. Based on study of two reactors operated for about 4 years, experts expected only negligible amounts of long-lived isotopes. Outsiders discovered that reactors operated for 30 to 50 years generate nickel-59 with a half-life of 80,000 years and niobium-94 with a half-life of 20,300 years, both at levels much above acceptable EPA standards. This discovery "went against the whole mind set" of "thousands of engineers all moving in one direction." Dismantling requires proper handling and shielding, may cost over $10 million, would reate 18,000 cubic meters of waste, equal to one fourth the LLW generated each year, raising questions where to put it.

69. Marjorie Sun mentions "flip-flops" during Carter administration in "Radwaste Dump WIPPs Up a Controversy." Science 215 (19 March 1982): 1483-1484.

70. Luther J. Carter, "WIPP Goes Ahead Amid Controversies." Science: 222 (19 March 1982): 1104-1106.

71. Luther J. Carter, "The Radwaste Paradox," Science 219 (7 January 1983): 33-36.

72. Mark Crawford, "DOE, States Reheat Nuclear Waste Debate." Science 230: (10 November 1985): 150-151.

73. Robert D. Hershey, Jr. "U.S. Suspends Plan for Nuclear Dump in East or Midwest." New York Times, May 19, 1986.

74. Joyce Maynard, "The Story of a Town." New York Times Magazine. May 11, 1986. 19 ff.

75. J.M. Harrison summarizes recommendations of scientists appointed by the International Council of Scientific Unions after a 1977 request from the National Academy of Engineering and the International Atomic Energy Agency. "Disposal of Radioactive Wastes." Science 226 (5 October 1984): 11-14.

76. For instance, see Matthew L. Wald, "Maine Says No to U.S. Nuclear Waste Plan. New York Times, March 29, 1986. p. 1 ff. He quotes one housewife: "If we should manage to elude DOE's flying fickle finger of fate, I don't think we should congratulate ourselves on shifting the reponsibility to someone else's community, someone else's children."

77. Mark Crawford, "Data Problems Halt Work at Two Nuclear Waste Repository Sites." Science 233 (4 July 1986): 22. DOE ordered contractors to "stop work" in concern about a mix up and the quality of documents on 50,000 linear feet of core samples drilled since 1983 in Nevada and to establish a sound paper trail on samples taken in Washington since 1976 to assure an adequate record of geohydrology.

78. Mark Crawford, "Outside Review Urged for Waste Site Study," Science 230 (22 November 1985): 924.

79. Eliot Marshall, "Nuclear Meltdown: A Calculated (and Recalculated) Risk," Science 232 (11 April 1987): 153.

CHAPTER VI

INTERPRETATIONS AND CONCLUSIONS

These stories have been about federal attempts to prevent failures, control and monitor hazards, and predict disasters in order to manage technological risks. But the planners at the top, scientists, engineers, and bureaucrats, encountered surprising difficulties and dilemmas, and responded in ways that have not made the hazards more manageable. Mainstream explanations for the lack of effective federal action, although partly appropriate, were deemed insufficient. Instead, our thesis is that the dominant models of knowledge and institutional arrangements do not fit well with the reality in which these hazards arise.

This chapter draws on examples from the case studies and is organized in two parts. The first part looks at how science, engineering, and bureaucracy, respectively failed to meet mainstream expectations for handling risks. It then examines a kind of retreat to probabilistic knowledge and a further retreat to a call for a societal judgment of the acceptability of risks. After arguing why this retreat is impractical, the argument pivots to a second part: an exploration of alternate kinds of knowledge and social arrangements for science and engineering and alternate forms of planning for managing technological risks. The chapter concludes with a critical review of the literature.

Science

A common theme in our stories has been reliance on knowledge from science, especially the earth sciences, for predicting hazards, constructing failsafe structures, and managing risks. Traditionally, science has been expected to provide a kind of deterministic knowledge of invariant causal relationships (turning more recently to probabilistic knowledge). This kind of knowledge is represented by generalized theories and abstract principles; it is acquired by prescribed methods and with instruments that are expected to be precise and reliable. Using such knowledge, scientists are expected to make predictions, so that people can avoid or prepare for disasters, and engineers are expected to design reliable artifacts to bring nature under control.

Evidence for such expectations about science lay in the claims of engineers about knowledge sufficient to build failsafe dams, in the seismologists' promise of predicting earthquakes, and in the hopes of policy makers of permanently isolating nuclear waste.

This section describes how these expectations were thwarted, first when dams and seismic engineers turned to science for help, and then when they tried to acquire general knowledge of their own. Next it discusses the knowledge expected of two types of earth scientists, seismologists and geologists, and how this knowledge also failed to meet expectations. It then considers problems with instruments used by both scientists and engineers. Finally, some responses of scientists to these problems are considered. We begin with dam engineering.

In FDS, for example, even engineers in the Corps admitted that scientists supplied inadequate data for the design of dams. Considerable judgment was required to extrapolate what lay beyond core samples and to estimate the range and variation of conditions in foundation and construction materials. Knowledge of past hydrology or seismicity was often insufficient to forecast what might occur. Environmental conditions might change over time in unpredictable ways. More than scientific principles were needed to deal with such uncertainties, as the FDS panel pointed out; engineers had to use skill and judgment in designing, especially in synthesizing an array of non-technical factors with technical ones.

Seismic engineers faced similar difficulties but got little help from seismology. The scientists' interest in general causes, their research on past earthquakes, and their basic data, on acceleration, did not help engineers anticipate the effects of future tremors at particular sites nor meet their varied data needs. Competent seismic engineers anticipated earthquakes from faults that had never been known to rupture. Like competent dam engineers, they had to use judgment in practical designs that balanced technical and safety factors with cost, constructability, and the structures' intended use.

At least three issues thwarted both types of engineers: the data from science was inevitably incomplete for use in design, knowledge of the past was insufficient for anticipating the future, which was essential unknowable in important ways, and scientific principles and methods were of little use in synthesizing many types of factors to be considered in design.[1]

When science failed to provide the reliable knowledge expected, engineers tried to create their own general knowledge. Both dam and seismic engineers learned much from practical experiences in the past, treating earlier projects somewhat as scientists might treat lab experiments. When one failed, these engineers tried to learn what had happened. Indeed, engineers are expected to describe the physical causes of failures and to explain what went wrong in order to improve their knowledge for future projects.

But the conditions necessary for learning are often inadequate: the cause of a seismic collapse may be burned in the rubble, the problem may be hidden in the foundation, as it was at Fontenelle dam, or the evidence may be destroyed, washed away by the accident, as in the case of Teton. Technical investigations into that failure revealed limits in the generalized knowledge of dam engineering.

Although experts used scientific methods to analyze the unique conditions of the site, they apparently lacked a general theory with explanatory or predictive power. They suggested several hypotheses, such as one on hydraulic fracturing, but they did not fully understand this process nor had sufficiently tested it. They could not precisely describe the sequence of events leading up to the failure, but settled on a very general explanation, "piping." This process cannot be theoretically described and is but one of several general causes on varying lists compiled by experts. In fact, these "causes" are not independent but often occur in combinations unique to particular failures.[2] Finally, the experts at Teton concluded that the cause lay in some specific combination of events that they could not precisely describe and moreover considered irrelevant. Unable to improve on a theory, they suggested that designers better visualize generally what might go wrong and add more defenses (which would have made the dam prohibitively expensive). Here scientific methods and general knowledge were inadequate even to describe the past. The remedy, essentially, was human imagination and judgment.

Seismic engineers had carried out research in the lab as well as in the field in order to build theories and devise formulas for design. They had incorporated their generalized knowledge in building codes and standards. But as a leading seismic engineer pointed out, the knowledge in codes and engineering formulas was little more than "a pretense of knowledge," often inappropriate for the design or inspection of specific projects, but leading engineers and the public to believe that the structures would be safe.

Turning now to some scientists, the seismologists too were frustrated in their mainstream expectations. For a time, they had theories at three different scales, which did not fit well together nor function adequately, leaving gaps, as the USGS admitted, to be filled by scientific research. For instance, the global theory of plate tectonics offerred a general cause for earthquakes at plate boundaries but did not adequately account for those elsewhere. The concept of faulting left the final cause of the elastic snap open to competing hypotheses from various specialized fields. The theory of precursors has been largely abandoned in the face of contradictory field evidence.

Science advanced its knowledge through controlled laboratory experiments, but these usually oversimplify field conditions. In seismology, for example, smooth blocks in the lab did not represent the jagged irregular nature of faults nor did experiments with them explain aseismic creep. As an example of how scientists deal with anomalies, seismologists now suggest that creep is part of a more complex process; to put it simply, fluids lubricate fine-scale material and allow rocks under pressure to slide slowly until a far-off binding rock gives way. The relatively simple process of friction binding two smooth plates together until it is overcome by stress now requires an elaborate explanation involving fine-scale local material.[3]

Further field studies have revealed new complexities. Additional plates and local fragments of distant origin complicate tectonic theory, while qualitatively different processes appear to generate earthquakes around the world. As is often the case in science, for every question answered, a dozen more arise. New answers often require elaborating a theory to account for local phenomena, in violation of the criteria for good theories, of simplicity and comprehensive explanatory power. New answers may also require labor-intensive field investigations that may uncover still more complexities.

Greater difficulties plagued those who hoped to predict the long-term fate of buried radionuclides. Modest USGS geologists, even with a perspective of millions of years, denied theirs was a predictive science, and eschewed uniformitarian assumptions. They could not explain, for instance, how voids had occurred in ancient salt deposits and warned that the stability of rocks in the past was no guarantee of future stability.

These earth scientists perceived some basic difficulties in the practices of science. First of all, they recognized that because every site is unique, knowledge about one site could not be used by analogy to describe another. One also cannot expect a general description comprised of common characteristics abstracted from many similar sites to be used in a model from which to deduce an adequate description of still another site; the new one may have peculiar properties.

Critics have argued that geology is an immature science, without adequate theory and pre-occupied with the past. Compared to fields like chemistry and physics, it is handicapped by an inability to perform controlled experiments on phenomena that are all interrelated parts of one large planet. On the other hand, nuclear physics has split elements apart and experiments repeatedly on masses of individual atoms to refine its theory.

But geologists faced difficulties with chemical and physical experiments in the lab. Selecting and separating elements from naturally occurring rocks might leave out liquid or gaseous phases that occur infrequently in the field. Under controlled conditions, these elements might not behave as they do under varied humidity or temperature, for instance. Properties identified by analyses could not simply be added up to represent the qualities of natural rocks. The heterogeneity, variability, and complex interrelationships among "parts" of fine-scale phenomena appeared to defy complete description.

This suggests several types of problems of knowledge. One lies in classification, in seeing one thing as "like " another. A second arises in leaping from a general description to a description of a specific thing. A third problem lies in trying to describe a whole from the knowledge of a few parts, as from samples taken for site investigation. A fourth type of problem arises when one assumes that a thing remains the same under other conditions, in a different particular context, and a fifth is in characterizing a whole as a simple aggregate or specific set Of discrete qualities. Several of these types of problems rest on the false assumption, common in physical science, that the whole is nothing more than the sum of its parts.

In summary, the knowledge in generalized models, whether conceptual ones or physical ones in the lab, seems too simple or gross to capture particular details and describe heterogeneous properties of natural phenomena or to characterize complex processes past or future. On the other hand, analytical methods produce fragmented knowledge that does not represent essential qualities of a whole in a manner required for design and prediction.

Turning now to some instruments that scientists and engineers use as their "eyes and ears" in order to identify aspects of particular phenomena, we find that they too are often inadequate. For instance, seismographs are too simple and insensitive to discriminate "noise," such as vibrations from nearby traffic, from data deemed meaningful, such as faint earth tremors. On the other hand, they may be oversensitive to their environments, as were the uselessly frozen piezometers at Teton, or surveying equipment that responded to atmospheric changes near Palmdale with data interpreted as earthquake precursors.

The choice of a type of instrument and where it is put are important. For instance, seismographs too close to a quake will not distinguish types of waves and may even be damaged. At new locations, data may be meaningless until men recognize localized patterns, such as in the groundwater flow around a new dam or in local seismic activity, or temporal patterns such as the grand cycles of quakes. The data about a single phenomena will also vary at similar instruments in different locations, as do signals from a distant quake. Conversely, different types of instruments in one area, measuring a single property such as permeability or stress in rocks, will yield different measurements at a scale of a millimeter, a meter, or a mile.

In such cases, these instruments fall short of the precision and reliability expected. Their value depends upon human judgments in choosing, for example, specific combinations of instruments and locations, in sorting out what is meaningful to humans, and in identifying patterns.

To review, instead of general theories, universally applicable and enabling complete descriptions, explanations, and predictions, and instead of reliable instruments providing clear and consistent data, there were obvious gaps. For instance, there were gaps between what was expected from theories or from models in the lab and what was observed in the field and between general properties characterizing many field locations and unique ones elsewhere. Moreover, as theories have changed with new understanding, as in seismology and seismic engineering, gaps or inconsistencies appear in the knowledge embedded in models at different periods of time.

In essence, the knowledge of mainstream science seems too general, gross, and fragmented to produce descriptions, explanations, and predictions suitable for handling risks of the type discussed here. By oversimplifying complexity and overlooking that which is rare, this kind of knowledge fails to capture the irreducible uncertainty inherent in combinations of details of particular phenomena that may change over time in unexpected ways. This raises basic questions about science. What is needed to fill these gaps? Can limitations such as these be overcome through further research or are the gaps, in spite of some scientists' claims, inherent in the very nature of science. These are questions we will return to. A glimpse at some responses by scientists to these problems brings us to the social context of science.

Some scientists, this time represented by seismologists and nuclear physicists, have not been notably humbled by such limitations. For instance, seismologists did not seem publicly embarrassed when a new underground test discredited their claims at Geneva or when further observations forced them to abandon precursory theory. After geologists discredited their general views on salt, physicists persisted in claiming that their general laws were adequate; what was needed was more time and money for research to obtain more adequate data. Seismologists also called for more money and time to build a big science like physics and gain the knowledge to fill the gaps.

Money has seemed as critical in building science and overcoming its limitations as it was for engineers who would build safe dams anywhere if clients would pay the price. Seismic engineers outside of government also long sought research money in competition with the seismologists. But thirty years after seismology began to get massive federal support, data from distant seismographs on underground nuclear tests is still contentious. Even after liberal funding from the EHR Act, earthquake predictions seem more difficult to make than in 1957. In sum, more than money is needed to fill the gaps.

The difficulties for scientists and engineers have often been exacerbated by traps set by the public and by outsiders to specialized fields, for instance, by people expecting seismologists to hold a stethoscope to the earth and guarantee a prediction. If scientists' statements prove invalid or artifacts fail, the public may mistrust all such statements or may hold individuals liable for damages.

The seismologists faced a surprising dilemma when they learned that the public might react negatively even before an accurate prediction. Their response was, predictably, to request more funds for basic, not socially useful, research. They set up a new institution, NEPEC, to control what their members said publicly about their research. They placed new demands on government, that it plan for responding to a prediction and for controlling public reactions, and also asked for protection from liability. They defended themselves from the social world.

In contrast, although nuclear waste planners had already lost public credibility, they barely mentioned liability since they did not anticipate a repository failure for thousands of years. They seemed confident of adequate funds for research. But they too requested more time and turned to government for procedures to manage the public response. For both sets of scientists, the federal bureaucracy was to fill the gaps.

In conclusion, these scientists in their particular worlds remained optimistic that with government's help, they would fulfill their own and public expectations. We now consider how engineers have fared without generalized knowledge or the data they needed from science.

Engineering Rules and Procedures

Engineers expect to solve technical problems and create failsafe structures and systems to control natural and man-made hazards and increase the benefits of technology. Dams bring water to irrigate new land as well as to control floods and droughts. Cities can be raised with confidence in seismic areas. and waste from radioactive materials, which is harnassed to provide almost unlimited energy, will be disposed of safely, thanks to the reliable knowledge and procedures of engineers in charge. Most non-engineers share such beliefs.

Even though they lacked adequate generalized knowledge, engineers have depended on implicit rules and standard procedures to create the expected failsafe structures. Both the rules and procedures rest on tacit assumptions about the kind of knowledge they have and the conditions under which they use it. These assumptions are taken for granted, raising certain expectations of engineering performance. For various reasons illustrated by the case studies, these engineers, like the scientists, were often unable to deliver the knowledge expected of them, leaving gaps between expectation and delivery that can exacerbate risks.

This section first describes a set of implicit engineering rules and some of their implications. It then considers common procedures of engineering, particularly in the organizational context in which most engineering takes place. Next it describes how some engineers responded to uncertainties and complexities in their knowledge, based on particular conceptual models. Last, it seeks to explain the inadequacy of engineering rules and procedures.

Engineering knowledge tends to be grounded in examples and advanced on a case-by-case basis according to one implicit rule: do not question what worked well in the past and elsewhere but use these as examples.[4] Unquestioning acceptance of earlier artifacts is based on the assumption that they will not fail, but overlooks how contextual factors may change, upstream in the case of dams, as the panel recognized, and also that structures and their foundations deteriorate with time. Oddly, seismic engineers never mentioned the factor of aging.

One difficulty with this rule is that over time, as engineering fields themselves have aged, the work of engineers has become increasingly specialized. Specialists tend to copy examples piecemeal, so that, like a recipe, a design may be put together of separate components. For example, Teton designers specified fill and drainage materials of the sort that performed well independently at other sites, ignoring how these would function in combination and at that particular site. As in analytical science, the underlying assumption seems to be that the parts of a design would add up to an adequate representation of the whole. This piecemeal approach may be valid for free-standing mechanical systems with loosely coupled parts. But it seems inappropriate for more organic earthen dams, for dynamic buildings at seismic sites, or for high temperature material buried in rocks. The context for each of these cases is vital: a changing or unstable earth.

If something does fail or does not function as intended or perform as first expected, a set of unwritten rules comes into play. One is the remedy prescribed for Teton and also for a nuclear waste repository, to elaborate a structure by adding on more defenses. Variations of this remedy are to make something bigger or to somehow extend the control of a structure or system over its environment, as a new dam was intended to do in Idaho.

But to add more defenses or greater internal redundancy is to ignore the possibility of new problems in some unforeseen combination of events. For instance, no one imagined that trenches added in the banks at Teton would allow water to seep through and cause hydraulic fracturing as a new source of structural weakness. Enlarging a system may also add new risks, as of dams failing in a series like dominoes. Indeed, extensive water management systems have transformed the high probability of small seasonal floods, once locally expected and avoided, into a low but rising probability of unexpected catastrophes.

The related rule that bigger is better also assumes that a larger structure will provide more control. This rule is reinforced by considerations outside the technical domain. One example lay in bypassing the most solid site for Teton and choosing one higher up that permitted a larger reservoir to irrigate more land. This rule seems implicit in the decision to aggregate nuclear waste in a few huge repositories, not only to offset high overhead costs and limit the areas to be contaminated but also, no doubt, to avoid political hassles over many sites. This rule also assumes that there are no natural limits, as did claims for failsafe dams at almost any site. Both overlook the larger consequences if failures do occur.

Another implicit rule of engineering, which the public seems to accept, is that engineering can solve all types of problems. In fact, as exemplified by the Corps, engineers tend to respond to almost all demands, accept problems pretty much as given, and seldom refer them to others or seek solutions outside their own domain. Even after Teton's failure, experts did not question whether a dam should have been build but only how it should have been designed.

Thus floods and droughts in Idaho were interpreted not as normal climate variations but as an engineering problem. No one seems to have thought of alternative remedies in the domain of organizations or economics. For instance, flood damage can be reduced by limiting farming on flood plains. To compensate farmers for losses in droughts, a system of local insurance could have been adopted requiring earnings in good years, in excess of some past average, to be set aside and distributed in poor years, a remedy costing little to national taxpayers.

This solution would have violated an implicit assumption, that technological benefits increase without limits so that no stopping rule is needed. Why limit agricultural production when another dam would solve the problem? The IRG's neutrality on the future of nuclear power implied such reasoning. Why forego its benefits when once an engineering solution was found to nuclear waste, that solution could be used for any amount? Again little thought was given to solutions in other domains, such as stimulating energy conservation or developing renewable sources.

The use of these rules is justified by a standard procedure, benefit-cost analysis, which also fuels expectations of increasing benefits from technology. But as public challenges to Teton implied, the knowledge used in these techniques is none too reliable. For instance, analysts may estimate multiple benefits, such as from fishing and recreation, that may be difficult to quantify precisely. For more tangible benefits, such as increased value in agricultural production, they must rely on data from the past or from other projects, but will project these over a project's theoretical life of up to 100 years. Yet no one could anticipate unprecedented events, such as the depressed agricultural prices in the 1980s. Assuming permanent dams, costs of repairs and final dismantling could also be ignored. Like physical instruments in science, the tool of benefit-cost analysis is expected to be precise and give reliable results; instead the results seem like artists' creative sketches.

Engineers also rely on their organizations to provide reliable knowledge. Yet the structure and routine procedures of large organizations, considered essential for creating huge projects, may accentuate limits in knowledge. For instance, dam making is commonly divided into a temporal sequence of steps, first investigating the "is" of existing conditions and then, given this data as input, deciding on the "ought," or how a structure should be designed. General plans are then fleshed out with detailed specifications, both sent down to control construction under contractual arrangements.

Gaps in knowledge occur between steps in this process. As noted, initial knowledge of site conditions may be incomplete. A chasm can also exist between the knowledge of designers and people at the construction site, who may not understand the assumptions or intent of designers. The latter gap is analogous to deriving knowledge from models or theories in science.

But plans and specifications designed to flow forward in time and control the actions of those below, constrain the acquisition of more complete knowledge later, after a site is excavated. New knowledge cannot flow up the organization and back, in a sense in time, to modify what the designers knew in the past, so that they can fit the design to site conditions. Top down organizational controls combined with sequential engineering procedures keep the best knowledge available from the design engineers.

On the other hand, when a large organization builds and operates dams, its engineers are expected to be in charge and have adequate knowledge throughout the life of a project. But even during design, this is true only in a general sense. For instance, site investigation may precede designation of a chief designer and then, as we noted, design work is parcelled out to specialists, as on outlet or material specifications, each expected to specify details of that particular project. The chief engineer, formally in charge, can critically assess their work only in a general way, somewhat theoretically, leaving a gap between the completeness expected of his knowledge and what others individually know. Once design is complete, he moves on to another project and less competent engineers take over.

Nor could design engineers know the details of how a structure is actually build, given the heterogeneity of a site, even if they stayed on and periodically inspected construction. Engineers working on seismic aspects of buildings under contract to architects may not even know about the final design, much less how the structure is build. Thereafter, large organizations owning and operating projects are prone to frequent changes of design personnel, so no one engineer supervises a completed project for long. (Local operators may remain longer, but their knowledge was ignored). When a structure remains longer than any one man's life span, no one can ever know all about it. This fragmentation of individual knowledge of large projects is no doubt a major reason for treating the knowledge embedded in design documents as sacred.

The drawings and specifications prepared by design engineers are expected, like scientific theories, to be complete so that workmen will know how to carry out all operations and deal with all contingencies. Instead, as in the Bureau's justification for specifying only general surface treatment, distant engineers cannot know local conditions as well as those at the site, who were implicitly expected to use discretion and judgment. Consistent with the rule that bigger is better, the Teton crew simply added more grout to big cracks and caves.

Just as large organizations often subdivide complex tasks into simpler ones, so engineering specifications generally break construction into routine operations. These are tightly scheduled to keep costs close to the expectations raised in earlier benefit-cost analyses. What does not fit into scheduled routines may be neglected or ignored, as when workers preparing the surface at Teton fell behind those laying the core and were forced to fill holes through pipes or to leave voids.

Another convention is that design engineers must approve and record changes in design documents before work is permitted to deviate from specifications. When designers work in offices far from a site, this procedure can take time; significant redesign causes even more costly delays. These factors inhibit requests for changes and create a dilemma for workers: to stick by the rules and ignore anomalies or break the rules and use discretion. In any case, this procedural requirement tends to suppress particular knowledge that does not fit design assumptions, which are themselves required because earlier knowledge was incomplete.

The conventional procedure of contracting with specialists to attend to construction details also limits the knowledge available at the site itself. Legal contracts discourage contractors from attending to unusual site conditions; they will tend to go by the book or to cover up deviations in order to protect themselves against breach of contract and liability, as they did at Teton. Contractual arrangements also can generate conflicts, as was illustrated by arguments over interpretations of the Corp's quality control procedures, further distracting attention from unique conditions. Adversarial relations then replace cooperation in a system already fragmented by engineering specialization and organizational divisions.

The FDS panel was aware of problems in the organizational setting, which contributed to Teton's failure. It responded in a way that seems at first to violate mainstream expectations. It emphasized that dam building is an art. But that art is implicitly practiced primarily by design engineers. They require an environment that facilitates the interchange of ideas and information and guarantees continuity of thought.[5] To achieve this, the panel endorsed new management principles to govern dam safety, but added that the most competent professionals in the agencies should oversee them. These men would set an example of technical competence, expected to flow down to correct improper attitudes and behavior throughout the agencies.

Underlying the talk about art is a conceptual model of engineering consistent with mainstream expectations: engineers are responsible for making safe dams and do so with a special kind of knowledge inaccessible to laymen. This model splits the business of making dams in two, with engineers possessing the attributes of artistry at the top. Below are lesser beings who serve engineers in carrying out their creative functions: managers, workmen, operators, even geologists investigating a site. We shall later see two variations on this split model.

Another model, this time of engineering organization, is implicit in the panel's expectations that the example of top dam safety officers would filter down and influence those below, a model of organization unlike that of the federal bureaucracy. It resembles the flat, two-tiered structure of academia, where teachers and discussion leaders provide examples and guide the work of those below in close and rather informal interactions. That model was also reflected in OSTP's organization for planning these projects, to be discussed. To the best of our knowledge neither the model of artistry nor of academic organization has affected mainstream engineering practice.

Turning now to look at procedures for detecting hazards in older structures, more problems of knowledge appear. Not only do design documents not reflect actual site conditions and how a structure was built, but over time they may overlook matters later realized to be relevant. Nuclear engineers realized this when they considered mothballing old power plants; only engineers who had worked many years at a plant had the detailed knowledge required. In this case, earlier engineers had insufficient knowledge or imagination to anticipate what they would need to know in the future. Similarly, new understanding often reveals that old knowledge embedded in structures, dams or seismic-proof buildings, once thought to be adequate, no longer is. Past knowledge itself may be a poor guide to future knowledge. But with little sense of history, both engineers and the public expect that whatever is built by engineering rules will be safe.

Identifying the hazards of older structures is even more intractable than designing new ones. Weaknesses may be hidden within or under a structure, giving few or only subtle clues of their existence. Moreover detecting these by using sophisticated sensing techniques by installing monitoring instruments, as in older dams, can be prohibitively expensive.

Seismic engineers were never able to formulate codes for inspecting the millions of hazardous older buildings in seismic areas. When dam engineers contemplated the use of general guidelines for inspecting the nations' largest dams, they encountered familiar problems. Like theories or designs or general models of media for nuclear waste, general inspection guidelines would be too gross to apply to all high hazard dams with their particular histories and unique qualities.

Some dam engineers saw a dilemma: ideally, only the most competent professionals should inspect high hazard dams, but too few such engineers existed to do a timely job. Writing specific guidelines tailored to each dam would also take time; less qualified inspectors might follow the rules by rote and miss significant factors. Even if all civil engineers were offered the jobs, the most competent would refuse out of a fear of liability and leave the work to fool-hardy ones. The idea that local people might monitor local dams was ignored.

In sum, the public generally shares the assumptions that the knowledge embedded in rules and procedures is adequate and that all structures designed or inspected by engineers are safe, until events prove otherwise. When problems do appear, instead of using precise and reliable knowledge, engineers tend to use rules of thumb, for example, that bigger and more elaborate things will bring nature under control. Such control is what the public has come to expect. Such responses ignore the possibility of limits in nature and tend to amplify the risks of large disasters. Techniques to justify these rules of thumb and the benefits of technology, fail to anticipate future changes and neglect important costs, including the cost of failure.

When engineers draw on their general knowledge for the design of individual projects, they tend to depend on assumptions derived from incomplete site investigations or on examples taken from other sites. Specifications that they expected to be comprehensive and precise enough to anticipate and effectively treat all contingencies are, like predictions, too general to fit heterogeneous site conditions. Moreover, organizational conditions subdivide knowledge for site investigation, design, construction, and operation, so that no one knows it all. Even the most complete knowledge available during construction is subdivided, or deflected in attention to rules, or suppressed.

Here, as we noted in science, are gaps between what is expected and what engineers can deliver. Instead of completeness there is partial knowledge. Instead of precision are gross rules of thumb or specifications too gross for the fine scale variations in particular situations. Instead of internal coherence and consistency is fragmentation. Instead of comprehensive solutions are narrow engineering ones that ignore more effective remedies; instead of reliability solutions are ones that may amplify risks.

How can we account for these flaws and gaps? Do they stem from engineering hubris? Do they arise from limits in the way engineers see the world? Or are they inherent in the very nature of engineering itself? The answer may involve combinations of all three. Hubris may account for claims of failsafe structures, for the desire to make things bigger and more extensive and bring nature under control, and even for the belief in solutions for all problems. Conversely, in some instances, as in adding more defenses, engineers may simply want to respond to public demands and assure greater safety. Engineers are also part of a society that expects protection from even trivial hazards and fails to recognize a stopping rule.

Some of these flaws and gaps seem grounded in engineers' hopes to achieve the reliability claimed by predictive science. Engineering perceptions are often similar to those of science. Both assume, for instance, that complete descriptions are possible through deductions from theory and by analysis, and that theoretical knowledge is somehow superior to particular local knowledge. They share parallel beliefs that all questions have answers and all problems solutions. Both tend to assume that the past is an adequate guide to the future and the whole nothing more than the sum of the parts. And both pursue specialized objective knowledge with poor understanding or even a trained incapacity to understand matters in other domains, particularly those dealing with the social reality.

The bureaucracies in which most engineers work no doubt influence their perceptions. When functions, such as design, are parcelled out to impersonal specialized roles, the human ability to imagine combinations of factors is lost. Fragmented tasks performed according to rules lead to making decisions piecemeal. When control and communication flow down and only limited formal reports confirming compliance flow up, the more complete knowledge at the bottom, fringes, and outside of organizations is lost or suppressed.

Big organizations restrict the ability of even the most imaginative at the top to recognize the weakness of general models, formulas, and rules, or the limits of nature. In a kind of vicious circle, the hubris -- or the responsiveness to public demands -- that has led to larger projects has in turn led to a need to subdivide work in the simplest, most rational way. Since design appears to be the most vital function, responsible engineers have preserved it for themselves, leaving the dirty work to less highly trained people. But in doing so, they have separated themselves from and lost sight of the physical reality where projects arise and exist and essential knowledge is found.

For such reasons, these kinds of engineering, which require close continuous attention to local phenomena and a synthesized understanding of a multitude of technical and social factors, have lost vital qualities. Men no longer work together, making decisions at all levels, and sharing responsibility, in the manner that some EHR planners saw as essential for a safe environment. On the other hand, if construction workers, for instance, were asked to participate more fully, they would probably reject the unwanted responsibility, claim inadequate knowledge, and express a preference for the existing order of things. Other people might be bewildered by what would seem to be the antithesis of rationality. So small, increasingly specialized, and narrowly focussed elites at the top of bureaucracies are forced to accept responsibility as their exclusive property, but are increasingly trapped in an almost paralyzing fear of liability. We now move on to the problem of bureaucracy itself.

Bureaucracy

As scientists are expected to create theories to provide reliable, predictive knowledge and as engineers are expected to provide reliable rules and procedures for creating failsafe artifacts, so bureaucracy is expected to convert both science and engineering into reliable policies, plans, and practices, in this case to manage technological risk.

This section focusses on efforts of OSTP to overcome the dysfunctions of bureaucracy, which seem to have impeded the federal government's responsiveness to particular hazards. We first describe what is general expected of bureaucracy and has made it a most enduring form of organization, and then illustrate how these expectations were violated. We then describe a set of OSTP strategies for planning and management to overcome bureaucratic dysfunctions, some of the difficulties these encountered, and OSTP's responses. Our analysis reveals another conceptual model parallel to the earlier model of dam engineering. Finally, we consider some complementarities and parallels among bureaucracy, science, and engineering. Now to the ideal of bureaucracy.

From the classical literature and some later theoretical work on organizations, one would expect bureaucracy to provide an orderly and cohesive structure, with internal parts that cooperate and communicate openly with one another in order to make rational decisions and carry them out in practice. One would expect government bureaucracy, especially, to be attentive and responsive to hazards in its environment, in order to protect the public from harm, and also that there would be a leader in charge and accountable.

But as OSTP began these three projects, it faced many symptoms of bureaucratic dysfunctions of the sort that are also acknowledged in the literature and that led the federal bureaucracy to fail to meet such expectations. For instance, there was structural fragmentation.

Responsibility for federal dams was divided among eighteen agencies operating under a pattern of authority marked by gaps and overlaps. As many as 100 federal programs relevant to emergencies and disasters were scattered throughout the bureaucracy.[6] Almost a score of departments or agencies had interests in the management of nuclear waste. Fragmentation also existed within agencies, as it did in the Bureau of Reclamation or in ERDA, which treated sources and types of waste in incompatible ways.

Instead of one leader in charge, even on a single large dam project, responsibility was dispersed, as discussed above. Two agencies, NSF and USGS, were mainly in charge of dealing with the hazards of earthquakes. Leadership on the hazards of nuclear waste was split three ways in a complex relationship among NRC, ERDA, and EPA, raising questions about which was in charge.

As is typical of fragmented systems, internal communication was poor. USGS field workers in Idaho could not talk directly with and warn the Bureau's local engineer. Officials in major dam agencies seldom spoke with one another. Agencies refused to report on programs relevant to mitigating seismic hazards.[7] Those planning for military waste were unaware of programs for commercial waste and of USGS findings.[8]

Governmental fragmentation engendered inconsistent practices. Dam agencies differed in their styles of engineering, for instance using either spillways or outlets to handle floods. Various agencies set different seismic standards for construction. Military waste was exempt from the licensing procedures applied to similar commercial waste. Over time ERDA's policy shifted between burial and surface storage.

Competition rather than cooperation had long marked the relationship among dam building agencies and those vying for funds for treating seismic hazards. A desire for secrecy rather than openness was evident in the reluctance of dam officials to talk about practical problems or to evaluate their own procedures. The energy agency had long hidden problems, such as leaks at nuclear waste storage sites, while ERDA research managers defensively responded to criticism and referred to themselves as "us versus them." Open hostility marked relations between USGS and ERDA/DOE. Such competition and defensiveness, hostility and secrecy, did little to overcome the bureaucratic dysfunctions of fragmentation and inconsistency, nor did it foster communication and understanding of potential risks.

OSTP responded to this situation in a number of ways, as we will document. First it evidently tried to serve as a model of bureaucratic rationality in its arrangement for these projects and its own operating style. Consistent with its position at the top of the federal establishment, it initially displayed an open and democratic style of leadership. Consistent with its mission, it placed high priority on scientific and technical matters. A major strategy was to sever technical matters from non-technical ones, apparently to free them from the contamination of bureaucratic dysfunctions and politics.

OSTP's most interesting strategies seem aimed at overcoming or correcting bureaucratic dysfunctions. Its implicit intent was apparently to force bureaucracy to speak in one voice, as it is expected to do. The first strategy, to overcome fragmentation, defuse competition, and improve communication for planning, was to gather representatives from all agencies and ask them to work together as co-equals. To foster a cohesive approach to hazards, a second strategy was to clearly articulate in public documents the general objectives of new programs. A third strategy, for management, was to put a leader clearly in charge of these programs. Evidence for each of these strategies will be presented in turn. Then we discuss the unexpected difficulties and surprises that OSTP encountered and its responses.

OSTP's style of operating exemplified what is expected of bureaucracy. As an agency created by Congress to serve the President, it responded to problems as given, as engineers tend to do, and accepted the remedies proposed by Congress or in Presidential memos. It was also responsive to Carter's policy of "openness," bringing in outsiders to review its work on dam safety, adopting procedures for "going public" in EHR, and adding public hearings and policy review for NW. OSTP demonstrated the openness of its own decision processes by putting everything in writing, leaving a "paper trail" of memos, notes on meetings, and annotated drafts in the files. The reasoning and responsiveness of the nuclear waste planners was made even more accessible by weaving public comments and IRG responses into their final published report.

To structure its work in the orderly manner expected of bureaucracy, OSTP organized three groups for each project somewhat on the federal model of checks and balances. Advisory groups performed a function like judicial review, or like peer review in science and engineering, overseeing the work of groups of agency representatives. These functioned somewhat like legislatures and will be discussed later. OSTP performed the executive function; Philip Smith served as director of the FDS and EHR projects. However, on the nuclear waste project, OSTP initially shared leadership with other members of the steering group, working closely with Deutch or DOE, chairman of the IRG, but in the end it clearly assumed an informal leadership role.

To make work more manageable, tasks were initially divided and parcelled out, as is the custom in bureaucracy, so that ICODS's specialized subcommittees wrote parts of the FDS guidelines, each member of the EHR working group dealt with a specialized type of issue, and six task groups were organized to produce parts of the IRG's policy document.

To handle technical matters, OSTP hired consultants as specialized staff for each project but reserved for itself the more delicate task of dealing with the bureaucracy. Staff work began with the analytical approach characteristic of science. The problem of dam safety was divided into 16 items for agency surveys; the planning group attempted to collect a comprehensive list of separate issues in EHR; while OSTP analyzed alternative technical strategies, policy options, and problems of knowledge in NW. In each case, the planners seemed to expect that analysis would sufficiently characterize the "is" of the problem and give sufficient knowledge to a larger group so that it could make sound decisions in prescribing the "ought" of a solution.

Consistent with its mission, OSTP focussed on scientific matters and distinguished these from administrative, organizational, institutional, and political concerns. The split between technical and administrative matters was clearly demonstrated in FDS by the decision to create only administrative guidelines, leaving technical matters to the discretion of engineers at the top, who would supervise less competent engineers below and modify the rules as engineering knowledge advanced to prevent them from becoming obsolete under bureaucratic inertia.

In dealing with earthquakes, seismologists had long set themselves apart from seismic engineers with their diffuse interests in organizing for seismic safety. To protect themselves from the threat of a negative public response, in the mid-1970s the seismologists sharply distinguished their scientific statements about predictions from "warnings" for subsequent actions by public officials. Although OSTP brought together scientists, engineers, and building officials to plan administrative and social programs, the USGS made separate plans for technical research without public review.

NW planners made a finer set of distinctions, setting technical matters apart at the top, implicitly ranking administrative decision making second, followed by organizational and institutional matters, with politics at the bottom. This was evident in the IRG report, which only summarized the technical discussion of the status of knowledge but elaborated the options and procedures for decisions, and relegated organizational and intractable institutional issues to brief concluding chapters. Technical issues were to be clearly separated from contentious political ones. Scientific research was to precede administrative decision making and sites were to be selected on the basis of explicit technical criteria, developed by DOE and untarnished by political concerns. Some members of the IRG even feared that a criteria of regional distribution would undermine technical judgments. Consistent with the IRG's approach, later legislation spelled out elaborate decision making procedures, left details of the technical process to the agencies, and deferred political judgments to the end.

OSTP's major strategy to overcome bureaucratic dysfunctions of administrative fragmentation, poor communication, and competition was to create three different representative systems. In each case, departments and agencies were asked to designate delegates, to work together on the FDS steering group of ICODS, in EHR, and on the IRG in NW. Rather than compete for resources, agencies were expected to contribute mid-level staff, often formally on loan, to work together on the planning, as staff did on the ICODS subcommittees, the EHR working group, and the IRG task groups. To facilitate communication, members met fairly frequently and informally face-to-face for free and open discussion.

To achieve consensus in NW, in addition to frequent meetings, OSTP used a time-consuming and rather impersonal process of re-circulating drafts of documents to members of various groups, until the number and diversity of comments abated, giving the appearance of consensus. Equal voice was given to staff and top officials as it might be in a seminar in an academic model of organization. In all these groups, agency officials and staff were expected to behave not like advocates for the missions of their agencies but like interdisciplinary teams of professionals dedicated to larger objectives,[9] in the expectation that these teams would eventually find one voice.

OSTP's second strategy to overcome bureaucratic dysfunctions and assure one voice in the management of these hazards was to produce general documents expressing bureaucracy's intentions, about engineering rules, federal plans, or an administrative policy. These written statements would not deal with substantive technical details, which were better left to the discretion of engineers and scientists. Even on administrative matters, they would of necessity be broad and general in order to apply to many agencies with diverse missions and to be flexible enough to guide decisions for years to come.

A final OSTP strategy was to place a leader in charge, who would manage subsequent programs. Two offices were created in FEMA for these leaders. Half a dozen additional offices were created for FDS at the top of departments or agencies. All of the leaders would have no specific powers. Yet they were exhorted to flesh out specific rules and regulations appropriate to the work of each agency. The agencies FDS were also instructed to maintain extensive documentation and permanent files on every phase of decision making on a project, such as records of all assumptions, of judgments made on the basis of technical studies, and of discarded design alternatives.

The lead agency for EHR was placed in FEMA over staff objections no doubt to avoid favoring either USGS scientists or NSF engineers and refueling competition, but also to strengthen the new agency. The new leader's first task was to draw up detailed plans to be implemented by individual agencies. He was also expected to work with agency officials and persuade them to adopt consistent seismic standards to modify programs at all levels, such as those of HUD for rehabilitating buildings or DOT's on retrofitting bridges. Later FEMA saw its role in planning to manage a post-quake crisis as spelling out rules on what agencies should make what decisions and how.

DOE became lead agency for NW before the IRG was set up to change the impression that responsibility was split between the NRC and ERDA. However, OSTP wanted a lead person to be designated on an interim basis in the President's Office, to oversee DOE and other agencies in implementing the policy. That person would also work with the State Planning Council on the process of "consultation and concurrence," to help the whole federal system achieve one voice.

But OSTP ran into unexpected difficulties and surprises with all these plans and strategies. In its determination to separate technical matters from non-technical ones, it must have been shocked by the FDS panel's talk of artistry to deal with irreducible uncertainty and to synthesize technical and non-technical considerations. OSTP was frustrated in its analytical approach when separate EHR issues did not add up to a tidy whole but left residual governmental and social issues. NW policy analysts were no doubt upset when a set of simple criteria generated an array of policy options and forced them to select four among them somewhat arbitrarily.

More critical was the failure to achieve unity among representative groups at the top. The surprising resistance to risk-based techniques in FDS fueled debates that persisted into the subsequent program in FEMA. Members of the EHR steering group argued over where the lead agency should be placed, and a larger debate simmered between those who would have a decentralized program and those who favored top down command and control. But the real crisis came when the President, for political reasons, vetoed the keystone of the plan, abetted by OMB with bureaucratic interest in fiscal conservatism. Most horrifying to Frank Press, a leading seismologist, must have been OBM's threat to cut funds for his particular kind of scientific research.

OSTP, bent on avoiding bureaucratic politics on NW, seemed surprised when new conflicts broke out after public review of the policy document. The disintegration of consensus within the IRG was attributed to members' reluctance to express reservations initially in the interest of an early consensus.[11] More likely officials were later persuaded to fight for positions consistent with their particular agencies' interests. Deutch no doubt also yielded to political persuasion within his agency and in Congress, as he rose through the DOE bureaucracy. After he defected from the IRG, the steering group fell apart, and staff became unavailable to work with OSTP. Not even the President could block Congressional funds for WIPP. Thereafter, the Iran crisis and coming elections probably distracted supporters from backing OSTP's implementation plan. So politics, bureaucratic, Congressional, electoral, and international, thwarted OSTP's rational planning.

OSTP itself often responded to crises in a political manner. In FDS it insisted on the development and use of risk analysis. Without a comprehensive EHR plan and facing a time limit for planning, OSTP abandoned the teamwork approach for an executive style, calling together high level officials from departments and agencies, but only once, and requesting them to submit suggestions for adapting their programs for EHR. After circulating drafts of a composite plan for review, OSTP met one-on-one with them in their offices to iron out problems, using its aura of White House power to persuade them to sign off. Then faced with the loss of funds for research, it apparently engaged in backroom politics, undocumented in the files, and yielded to OMB the lead office's power of budget review, once considered so essential. Like the rest of bureaucracy, OSTP's main interest was in the survival of its mission. One can imagine what would have happened to the Director's status, had he lost support for seismological research.

OSTP responded to Deutch's defection not by turning to peers, but to the top, expecting the Chief executive, as ultimate leader and teacher, to resolve remaining issues reasonably, in its favor. To its surprise, he resolved the policy choice between selecting a first site from 2 to 3 or from 4 to 5 options mathematically, suggesting a compromise of 3 to 4. OSTP was then forced to spell out the philosophical differences between its slow scientific approach and DOE's more rapid practical one. OSTP took over, negotiating, brokering, and arbitrating until an hour before the President announced the policy.

OSTP maintained that the best way to achieve consensus among diverse interests on contentious issues was by free and open communication.[12] But ironically it used another kind of political ploy in NW to achieve consensus, one that could be called "bureaucratic language games" or "fogging."[13] The IRG documents were full of obfuscating words and phrases, borrowed or invented, and substituted for simpler straightforward language.

For example, policy options were labelled "interim strategic planning basis" to avoid their being treated as subject to environmental policy review, and phrased so abstractly that even IRG members had confused the differences between options. Spent fuel, which utilities preferred to consider a resource to be reprocessed rather than a waste, was subsumed under "High Level Waste" for technical reasons; a more likely motive was to dampen debates that arose from the mere mention of "spent fuel." WIPP was called a "conceptual facility" for experimental disposal of spent fuel to avoid legal questions about an advanced project that had not met required licensing procedures.[14] Military waste was often called "DOE Waste," a confusing phrase since DOE dealt with all types of waste. Military waste could not be licensed for "security reasons," although licensing would not directly harm national security.

This kind of language is not consistent with aims of science, which coins terms as a kind of shorthand for greater efficiency and precision; the intention seems more to mask contentious matters, to avoid or suppress time-consuming debates, and to expedite speaking, however unclearly, in one voice. The months of haggling among agencies competing for control of the final words in the policy suggests that this aim was not achieved.

OSTP's intent of producing clear and consistent statements of objectives floundered in the other two programs as well. A difficulty with this approach, recognized by Tschantz, was that general guidelines were tantamount to saying nothing, while specific ones could impose hardships on smaller agencies without the resources to comply. The result was an ambiguous document, requiring bureaucratic procedures but also encouraging consensus building and teamwork. Difficulties stemming from generalities were also apparent in OSTP's plan for EHR. It ignored the issues so painstakingly assembled and called mainly for adjustments in federal agency programs; it was so general that Thiel thought it would be laughed off the Hill.

The strategy of putting a leader in charge did not fare much better. The leaders of both FDS and EHR burned out and retired. Moreover almost a decade later all three programs show evidence of persistent bureaucratic dysfunctions; continued fragmentation, poor communication, conflicts, secrecy and hiding, and neglect of hazards. Dam safety became a specialized responsibility of a few at the top, meeting less frequently and less actively and quibbling on technical matters or holding back in fear of liability, leaving the future uncertain after they retire. The most tangible results lower down are more paperwork, of the sort once blamed for distracting engineers from essential work, and more written rules, honored more in principle than practice.

A fragmented earthquake prediction system is unlikely to take timely action even if a socially useful prediction is made. The most practical federal program for EHR is hidden in secrecy, its goal to protect defense facilities, not civilians. FEMA's plan for command and control after a crisis is not only inappropriate but may stifle local initiative. Its new airplane to give information to the press seems intended to keep the press from scrutinizing military operations control, rather than aiding the victims.

The fragmented treatment of nuclear wastes continues with little practical action. Conflicts persist between USGS and DOE, which is accused of lack of responsiveness, hiding and secrecy. Dialogues with States have broken down in controversies and lawsuits about the procedures themselves, while DOE leaders claim no technical problems and blame electoral politics.

What explains these events? What conceptual model underlay OSTP's set of strategies and operating style? Apparently, its idea of how to make bureaucracy effective in managing risk was through an open consensual democratic planning process at the top. Then management officials would rationalize bureaucratic performance by requiring technical experts, free of political influence and ultimately in control, to use technical procedures, and by establishing rules for command and control from below. Indeed, it appears that OSTP was planning not so much for the management of technological risk as for the management of the bureaucracy.

Underlying these strategies seems to be a split image of bureaucracy, like the FDS panel's split image of artistry. A double standard existed, for different levels, for those at the top and those below. At the top, fragmentation would be overcome through a representative system engaged in continuous informal dialogue until divergent views were synthesized to give the impression of one voice. That voice would then be embedded in consistent and uniform policies, plans, or guidelines, giving bureaucracy the stability and continuity expected of it. These written statements would take on a life of their own, under a leader or lead agency installed to tend to them, still working with a group of peers. Since the leaders would have little authority to carry out policies and plans or enforce guidelines, they must use powers of persuasion.

What happened below would be different. Lower level officials would be encouraged to spell out generalities with additional written rules and to fill the gaps with procedures to fit specialized missions and to modify routines at all levels. Clear documentation would make the rules accessible, so that everyone would know how decisions had been or should be made. In this way, nothing would be left to chance or politics or low level discretion. For instance, decisions on individual NW sites could never be made on an informal case-by-case basis but only after numerous technical studies and bureaucratic reviews. Thus consistency and rationality would be assured.

OSTP's operating style was the essence of technical rationality; its intent was to make things uniform and consistent, orderly and predictable. Matters would be subdivided and subjected to formal analysis, based on explicit and often measurable criteria. Decisions would be technical in the sense of not being open to political interference. The results would then be incorporated into such clear and simple procedures that even idiots could be held accountable in carrying them out.

This model of effective bureaucracy complements and parallels science and engineering in several ways. Bureaucracy complements science by stimulating the growth of theoretical knowledge through support for basic research and by fostering its use in practical applications. Nuclear science and great dams supported by the federal bureaucracy have given it prestige around the world. Prediction research is expected to have a two-fold value, as the USGS pointed out: to add to human understanding in general and in applications, to inform and supplement engineering in saving lives. The same double value was expected of risk analysis, to supply a new way of understanding hazards and for making decisions on how to abate them. Moreover, society supports these functions in the expectation that government could and should protect it from harm.

The hierarchical structure of bureaucracy also complements science, by facilitating the flow of knowledge like water downhill, as OSTP expected, and into applications. Research on risk analysis would flow into dam engineering and geologic research into engineering for NW. According to some EHR planners, influential professionals must help guide the transference of basic knowledge into applications outside of bureaucracy.

Parallels also exist among science, engineering, and bureaucracy. As in OSTP's strategy for bureaucracy, science and engineering used small groups at the top to decide upon theory, judge the validity of hypothesis, set technical standards, or for peer review. For example, a few nabobs of American geology rejected Wegoner's theory; a small group in NEPEC would validate prediction research and in FEMA create technical standards for dams the only way it could, by a consensus of individual judgments.

As bureaucracy tried to free technical matters from administrative and political interference, so scientists and engineers also tried to stifle purely personal motives, biases, institutional considerations, or unprofessional behavior. This was the intent of ethical guidelines in seismology and of professional development programs in FDS.

Scientists, bureaucrats and engineers also seemed to expect that the use of agreed upon analytical methods, specific criteria, or legitimate procedures would add up to assure reliable results. For instance, if seismologists used the methods of science properly and met quantitative criteria on the magnitude, time window, and probability, NEPEC could be confident about issuing predictions. If numerous risk analyses reached similar conclusions, the results could be expected to be valid. CEQ seemed to believe that technical accuracy would be assured if all can agree upon legitimate procedures.[16]

Furthermore, as scientists expect to predict the future by deducing testable hypotheses from theories, and as engineers expect to control uncertainty with artifacts devised from principles, so human behavior in bureaucracies is expected to follow logically from policies, plans, and procedures and become predictable. Bureaucracies have no place for behavior that does not fit the rules any more than theories have for rare and unusual phenomena. If something goes wrong and technological accidents happen, bureaucracy turns not only to science to explain the past but also turns critically on itself to learn what rules have been broken and what new ones are needed, as it did after Teton's failure.

In conclusion, as science seeks internally consistent theories about the natural world and engineers would create comprehensive designs, so bureaucrats would make coherent generalized policies and plans for organizations. All can be seen as ways of knowing, of describing, explaining, and controlling the physical and social world, or variations on one way of knowing the nature of the world. Meanwhile, reports on risk analysis as a tool for decision making do not bode well. But many experts still considered probabilistic methods, to be discussed in the next section, as a panacea for managing technological risk.

Retreat to Probabilistic Knowledge

When science could not deliver the deterministic knowledge expected, in each case bureaucracy fell back to expect more reliable results from a different kind of knowledge, probabilistic or statistical knowledge. This retreat was obvious in OSTP's demand for such knowledge from risk analysis.

This section first describes the way our engineers and some scientists used simple statistical methods and more complex techniques. It looks closely at the NW planners' approach to models of risk in their search for reliable predictive knowledge and then at their retreat from these. Next it considers what a societal judgment on acceptable risk might mean in practice. The conclusions suggest a third split model, this time of society and its knowledge as a whole.

Statistics is a way of describing things mathematically and of substituting a few quantitative relationships for qualitative descriptions. Underlying all statistics is the concept of a normal frequency distribution; if we had all possible data, it would cluster in a pattern within a limited range around some central measure or norm. On this assumption, statistics are applied to samples to produce generalizations about what can normally be expected. But statistics also make possible estimates about the frequency (if not the range) of extremes and predictions of their probability. Thus the uncertainty of knowledge can theoretically be bounded.

Simple statistics have long been used by engineers as input into their formulas for design. Dam engineers, for instance, used data on recent rainfall to estimate the maximum probable flood that a dam might have to handle once in a thousand years; they usually added a margin of error of a factor or more. However, as in deterministic science, their knowledge might be too gross; aggregate regional data neglected heavy local storms, now recognized as a cause of failure. Or samples might be too small, as was 20 years of data on the snow above Teton. Observations for a century or more did not provide knowledge of earthquakes with cycles of many hundreds of years. Again the past was an insufficient guide but it is all that anyone could reliably know.

Seismic engineers, in their uncertainty about future earthquakes, even turned to tables of random numbers, which statisticians have devised, for input into some of their formulas for design. As an ERDA offocial remarked, engineers do not need the precise numbers required in science; approximate, even random, numbers will do, especially with conservative design.

Statistics are sometimes misused inadvertently, often to support a position. For example, in the early 1970s dam officials wished to show that a recent cluster of failures and near failures was unusual; the average rate of failure of all large dams had decreased from .0027 per dam per year before 1940 to .0007 thereafter, because of improved engineering. This data implies that failures will soon end. However, the method ignored contextual factors, such as the fact that dams are aging and newer ones are built on poorer sites with greater economies, as costs rise. Thus a gap arose between the simplicity of their analysis and a more complete representation of the situation.

OSTP hoped that risk analysis in conjunction with benefit-cost analysis would curb dam construction, but seemed unaware of some simple mathematics. A layman can understand that if a low probability of, for instance, .0004 (extrapolated from the trend above), were applied to a billion dollar loss in one year, the "cost" would be only $400,000 the first year and would decrease thereafter, due to the convention of discounting future values. This cost would not appreciably offset the value assigned to benefits from a multimillion dollar project, especially if analysts massaged the data and parameters.

More sophisticated statistical techniques are used in risk assessments to calculate the probability of unprecedented failures of a particular artifact. Such analyses commonly require identification of all contributing factors as independent variables and the assignment of probabilities to significant factors. Federal dam officials, who had willingly used simple statistics, validly criticized risk assessments for dams: they could not assume the independence of factors in failures nor even make a comprehensive list of all causal factors, as the case of Teton suggested. Implicitly they had a model of dams that was more organic than mechanistic. The expected accuracy of this kind of analysis collapsed under the limits in their professional understanding of dam failure.

Seismic engineers were more enthusiastic about using these techniques, perhaps because they considered structural elements in free-standing buildings to be more independent than the layers of fill in a dam. In principle they could calculate the probability of failure of each component and what effect strengthening it would have on the total cost and the probability of failure. But the parts do not add up so straightforwardly. It is not possible to model accurately the performance of a complex configuration of parts in dynamic interrelationships under wide ranges of seismic conditions. Here again a gap arose between the theoretical possibilities of these techniques and the grossness of the models in practice.

On one point the seismic engineers were insistent: whenever risk analysis was used, the public or public officials must have the final say on the acceptable level of risk. They did not spell out how this sould work in practice. Less publicly, they simply defined acceptable risk as an acceptable number of fatalities.[16]

Risk analysis is seldom used on older structures because internal conditions are difficult to observe. Loathe to inspect them on a case-by-case basis due to the risk of liability, engineers and building inspectors have reverted to statistics in a simpler form, classifying buildings by material, age, and other factors, and aggregating data to arrive at probabilities of failures for various types of structures. The number of structures and the potential losses in some cities and states is so great that an expensive federal program seemed to be the only remedy. Meanwhile local public officials could forego programs to deal with individual buildings because the problem was too large for local remedies.

Current models assessing the probabilities that critical facilities would fail in an earthquake could not yet trace out the consequences, especially from tertiary effects, such as a fire storm from the explosion of liquified gas, leaving the magnitude of catastrophes almost unbounded. Incidentally, the "yet" or an equivalent modifier often used in discussing these techniques shows that analysts recognized gaps in the performance of these models but were optimistic that the gaps would soon be overcome.

Seismologists also had high hopes for statistical knowledge in especting that earthquake prediction would become as reliable as weather forecasting. The analogy ignores the fact that seismic phenomena are not so easy to observe and that seismic events are scattered and infrequent; at least it implies that many errors were expected! These scientists also wanted to assign probabilities to all predictions; then they realized that such numbers on long range predictions would be little more than guesses until enough successes provided statistical confidence in their methods.

To advance its cause, the seismic community combined in one statement the high probability of future earthquakes and the aggregate consequences as statistics might do, rather than treating matters case-by-case. FEMA chose the aggregate approach, ignoring the low probability that any particular individual or community would be harmed. Individuals and small groups could not be relied upon for hazard mitigation; decisions should be made only by those looking down at the big picture.

Nuclear waste planners were most enthusiastic about risk assessment. They realized that engineering experience, experiments, and prototype testing were insufficient to eliminate all uncertainties. When they could not get the predictive knowledge they needed from the earth scientists, they turned to mathematical models in the hope of analyzing the uncertainties and assessing the risks. They seemed to expect earth scientists to aggregate data about geologic areas, describe them generally, and characterize how types of sites would normally behave over thousands of years. These descriptions could then be used as model for assigning probabilities and predicting risk.

But even the first step in such analyses was difficult. To analyze modes of failure required a combination of scientific reasoning, engineering experience, and intuition. Moreover, the ability to build the necessary mathematical models was limited. However, that ability was deemed sufficient to limit or "bound" some uncertainties, e.g., to estimate reasonable upper limits and test the significance of many variables. Lab and field research and conservative engineering could reduce many uncertainties. In spite of residual unknowns, OSTP concluded that knowledge would be adequate to assure isolation for up to a few thousand years.

Attacked for rationalizing a wish to proceed rapidly, OSTP backed off from these techniques. It admitted that the methods had inherent limitations and should not be used uncritically, but no other approach to risk was available. OSTP insisted that its analyses, unlike previous ones, would not be about idealized repositories, but about actual sites. These planners seemed oblivious to OSTP's conclusions in FDS, that risk analyses could not yet be applied to individual dams, a familiar technology with many examples. This myopia is not surprising, since OSTP treated dams, earthquakes, and nuclear waste as unique types of problems and transferred few lessons from one to another.

Typical of its analytical approach, OSTP even classified the kinds of uncertainties in these analyses. Uncertainties arose from five types of problems: a lack of data, of experience, and essentially of imagination to identify all scenarios of release. A fourth problem was due to the variability in nature and a fifth to an inability to predict natural processes and "social evolution." The greatest uncertainty arose about social behavior or "future institutions" but could be reduced by choosing a site without valuable resources assuming that humans will always want only the resources valued today.

OSTP then fell back to a new position. In its conclusions on the status of knowledge, it dropped all references to risk analysis.

Instead, it emphasized that new information during each step in creating a repository, from excavation until it was "decommissioned," would resolve uncertainties and would "permit" reevaluation of risks. Yet even after a repository was sealed, some uncertainty would always remain. It concluded: "Thus, in addition to a technical evaluation, a social judgment that considers the level of risk and the associated uncertainty will be necessary."[17]

This statement suggests that the planners retreated from probabilistic knowledge to a social judgment as a way of handling residual uncertainties that escaped technical characterization. Did they mean that all members of society should make decisions and share, as was suggested in EHR? Did they intend that those at the top would use a kind of artistry to synthesize social judgments with technical ones, as the FDS panel suggested? Or did they use this phrase only rhetorically, to mask despair? There is little evidence for the first two possibilities. These planners ignored, except in principle, strong public demands for more participation. They did not knowledge artistry in engineering or anywhere else, but relied on the mainstream model of decisions based on technical considerations and formal procedures.

How would a social judgment work in practice? We propose a kind of thought experiment. If a repository site is ever selected, under what conditions would the public have any say? On the basis of what knowledge? From what we know of bureaucracy, we may expect that the public's role would be tightly circumscribed and its knowledge would be limited.

Imagine this as a plausible scenario. EPA uses risk analysis not just as a tool but as a decision rule. It will require a risk analysis for environmental impact reviews as part of licensing procedures on any site and will compare the summary number on the level of risk to its standard for acceptable risk. These analyses will perforce be highly speculative. But few if any technical people will be able to critically assess the assumptions and methods hidden behond elaborate mathematical models and couched in technical terms drawn from many specialized fields.

Moreover, the analysis will be based on general design assumptions since the engineered barriers are to be tailored to a particular site. But DOE will insist that with conservative engineering the repository performance will meet or exceed EPA's standard by conservative engineering, and will support this assertion with fogging language that confuses even insiders. The public and its representatives will do as they must with seismic risk maps -- accept technical assessments and expert opinions on faith. Ultimately, various kinds of politics will be used to force all to sign off and DOE will proceed to construct with bureaucratic momentum.

But what of OSTP's claim that new information during construction and repository filling "will permit" the reassessment of risks? Who will listen to the construction workers who have direct access to new data? What will become of information acquired after formal reviews are completed, especially if it undermines more optimistic assumptions in the analyses and in the design? The story of Teton suggests that even if there is evidence of high risks to workers or the public, this knowledge will be lost or suppressed.

Was the phrase "social judgment" inserted just as another fogging strategy, to sugarcoat OSTP's despair about the magnitude of technical uncertainty? Perhaps. The planners certainly believed that "in principle," licensing procedures would provide some safeguards. But they failed to visualize how the process might work in practice; they ignored the combination of institutional factors that allow technicians and bureaucrats to make important decisions.

Underlying the limited understanding of these planners appears to be a conceptual model about the social reality, parallel to OSTP's split model of bureaucracy and that of engineering artistry. Here, scientists and policy makers are at the top, set apart from all others, especially from those outside bureaucracy. This is the sort of model of man and society that FEMA espoused, about knowledge and responsibility exclusively at the top for making decisions about technological hazards.

At the top, scientists are also free to do basic research. The knowledge flows down and is disseminated but only to leading decision makers and public officials. Ordinary people below cannot understand the nature of hazards, will not attend even to their own safety, will panic if they are not told what to do, and expect Big Government to protect them. Therefore tthe public should not be informed of hazards, such as impending earthquakes, until bureaucrats have made elaborate plans and institutionalized controls.

Even those who had advocated shared responsibility for earthquake hazards decided later that it was useless to give the public a voice on acceptable levels of seismic risk, because if only one person died in an earthquake, people would declaim their earlier decision, and attack engineers and public officials. Essentially, the experts should keep their knowledge secret and make their own decisions on risks until authoritarian programs were in place to control the public reaction. This is a frightening prospect.

To conclude this first half of our analysis, we have seen how the variable nature of the physical reality thwarted expectations of using generalized knowledge and scientific tools for prediction and control. Engineering rules of thumb and procedures of bureaucratic organizations exacerbated risks. Rational planning and management strategies fell afoul of bureaucratic dysfunctions and fell back on politics.

Planners operating on mainstream models of science, engineering, and bureaucracy were tripped up by new problems and trapped in dilemmas. Many become confused, as American scientists were in China, or frightened, as FEMA was by the California program, or in despair about inadequate knowledge for isolating nuclear waste. But new institutional arrangements proposed may only suppress other kinds of knowledge, social arrangements, and planning useful for dealing with technological risks. We now turn to examples of alternatives in the case studies.

Alternative Kinds of Knowledge

In diagnosing the problem as gaps between expectations and what is delivered by mainstream science, engineering, and bureaucracy, the question becomes how to fill these gaps. Scientists might shovel in basic research, but if each question answered raises a dozen more, theirs is an endless task. Engineers might build bridges, but these will collapse if gaps widen. Bureaucracy would erect walls to hide the gaps and order people away. A partial remedy is of course to lower expectations and recognize that knowledge can never be perfectly certain or complete and the past is an inadequate guide but all we have.

But more is needed. This section will discuss other kinds of knowledge, their nature, purposes, and conditions under which they are acquired. These conditions seem to involve particular kinds of social arrangements, then discussed. Such social arrangements facilitates a kind of planning more appropriate to dealing with technological risk.

Our case studies revealed other kinds of knowledge that may fill gaps or span chasms. In the messy reality at dams were four kinds of useful knowledge: the intimate knowledge of familiar phenomena acquired over time by local people, the feel of skillful workmen in handling what cannot be observed directly or known with certainty, the disaggregated knowledge of workers, which was never assembled nor tapped, and the ability of non-professionals to use imagination and critical judgment on technical matters.

Competent technical professionals, such as dam and seismic engineers, also use skill and artistry, imagination and critical judgment, but the content of their knowledge tends to be abstract and general. They rely on impersonal instruments and representations on paper rather than on direct sensory impressions of fine-scale natural phenomena. Although OSTP recognized that intuition and judgment are required for risk analysis, scientists often downplay the skill and judgment gained by experience, such as seismographers use to distinguish meaningful patterns from noise.

All sorts of people were seen to possess these kinds of knowledge. For instance, senior engineers at nuclear power plants could have intimate knowledge of vital matters not documented in records. Experienced building inspectors who liked their work were said to be able to sense infractions before they saw them. Both knew more than could be put into words. So did ordinary people in various contexts, such as the New Hampshire farmer with intimate knowledge of occasional springs acquired over time, or the housewise speculating on an analogy between a pressure cooker and a nuclear waste repository, or Chinese villagers about the behavior of their wells and animals.

The characteristics of such knowledge are often the antithesis of what is expected of mainstream science. Global generality is replaced by local particularity, and "rationality" by feelings and intuition. The knowledge of mainstream science is itself enriched when specialization is supplemented by aggregation, analysis by synthesis, and when speculation and criticism replace claims of certainty.

But how can such reliable knowledge be acquired? One way is through close direct observation of particular phenomena characteristic of empirical methods of science. NW planners, for instance, recognized that a remedy for incomplete general descriptions was the direct inspection of particular conditions at unique sites. But such knowledge would be incomplete and lack predictive value, and be conditional on further investigation.

The most reliable kind of knowledge seems to come from discovering patterns in some combinations of specific factors, often at different scales or from different sources. A good example was the recognition by seismic engineers that the frequencies of earth tremors amplify normal vibrations in structural members to become a major cause of failure. At a larger scale is the example of the Chinese, who recognized that converging patterns of anomalies indicated a coming quake.

A value of statistical methods is their ability to show patterns among sets of variables; another value is that the knowledge gained admits to limited certainty. But statistics, like controlled experiments, remove variables from contexts, as scientists and engineers do in taking sample cores from sites, often changing the nature of phenomena, as exposure to atmosphere does to some elements.[18] Statisticians also tend to ignore what can not be quantified, so that the results may be less reliable than what is implied by stated levels of confidence. In contrast, the grouter, through repeated fine-scale interactions, appreciates the qualitative nature at phenomena.

The ability to combine information from multiple sources and discover patterns was essential in early science. One example was Mallet, who assembled ordinary people's "felt reports" and observations of structural damage to discover circles of decreasing seismic intensity. Reid also combined data from many field surveys to discover a pattern of discontinuities over time. Both men were open to surprises and serendipity and the back talk of local phenomena and people.

The earth scientist Terry Sieh used this kind of approach. He had to be intimately acquainted with the particular local reality in order to know where to dig. Like the Chinese who used knowledge old and new, indigenous and foreign, and from folklore and science, Sieh combined knowledge from geology and modern seismology and methods and instruments old and new, as in a shovel and a modern Geiger counter. He was not afraid to dirty his hands in direct interaction with the earth. Like the Chinese, he did not seem interested in building theory but he discovered a pattern, left by the past, useful for anticipating future quakes and protecting lives.

The cognitive processes of Sieh and the early empiricists seem reflected in social processes at a larger scale. For instance, to arrive at the magnitude of a distant quake requires more than skillfully interpreting objective data from instruments. In some cases, seismologists must interact informally, communicating with contemporaries and comparing data from different locations but also drawing upon the knowledge of predecessors, the field geologists who studied and mapped the terrain through which the tremors travelled. Thus the Richter scale of magnitude can reflect a collective judgment, combining knowledge old and new, from particular points and of the larger context, arrived at through social interactions deep within the institutions of that community of scientists.

Informal relationships across the boundaries between disciplines are also valuable, as the FDS planners realized in recommending the formation of interdisciplinary teams, using curiosity, in dam site investigations. This approach encourages questions and attention to odd details, and precludes their being ignored or facily explained by a single discipline.

Indeed, in the earth sciences neither geophysics nor any one theory, past or present, seems adequate to explain the diversity of field phenomena. As the Chinese recognized, predcting unusual events in the earth requires a social process even more than a theory, so their approach was to aggregate all kinds of knowledge in order to discover local patterns through a massive empiricism that transcended disciplinary and institutional boundaries.

Engineers have also acquired knowledge empirically through direct interactions with the physical world and informal social arrangements. One example is John B. Jervis, who build Croton-on-Hudson, the first large dam for the public purpose of supplying water to New York City. He knew and loved the landscape. He worked with his men and like Sieh was not afraid to dirty his hands, gaining an intimate knowledge and a feel for the site. He also learned lessons from failure.

Engineers have filled gaps in knowledge by learning from multitudes of past failures, both of dams and buildings in earthquakes. One of the greatest bridge engineers, John A. Roebling, learned from understanding why earlier crude suspension bridges failed. On the other hand, recent engineers have responded to failures by simply adopting higher standards without critical judgment, as the Corps did for spillway design. Some nuclear waste engineers would have depended on experience in simple salt mining, overlooking how unprecedented combinations of factors could lead to failures. But they could also acquire some knowledge about radionuclides in soils from past failures in leakages at storage sites. Unfortunately, the IRG lost the opportunity to experiment with spent fuel. It also failed to treat low level waste sites as opportunities for small-scale experiments.

The history of seismic engineering offers an example of how engineering knowledge advances. Both physical proximity to failures in California and social proximity or informal social arrangements in academic organizations in that state were helpful. Initially small groups of faculty and students collaborated in an easy exchange of ideas at a few universities in California and could study failures nearby. They even learned from errors in well-engineered buildings, as when the falling of new light fixtures revealed the importance of the duration of tremors. They also learned to be leary of new materials and structural innovations, while the best among them recognized the need for good judgment.

Bureaucracy was especially uncomfortable with knowledge that could not be formalized or put on forms, as New Hampshire people learned about their social concerns and as USGS field workers mentioned before Congress. Dam officials made this apparent when they protested that anyone who had knowledge that a dam might fail should speak up; it is unlikely that they would accept speculation, feelings, or judgments not backed by technical data.

Our cases provided one example of how bureaucracy could obtain local knowledge, from the bottom up, in the final guidelines for national dam inspections. These guidelines were multileveled, generalized at the top, as were OSTP's plans. But instead of imposing stricter controls on those below, they gave states discretion to spell out more specific guidelines, not consistently for all dams but for each particular one. At the lowest level, relatively inexperienced people, much like forest rangers, would use discretion in making visual inspections. As in the Chinese example, people with special training would intervene only if something significant were suspected. Thus judgment was required at the lowest level and responsibility flowed up, synthesizing and re-embodying the artistry that mainstream engineering dismembered.

When adequate knowledge cannot be gained through the usual procedures, joint inquiry requires those with formal expertise to place a certain trust in the knowledge of those without extensive training in theoretical matters. On the contrary, NEPEC distrusted even trained field workers who came to a consensus on patterns of anomalies and would make a short-term prediction. Seismologists dismissed the insights of Denver citizens and implied that they themselves had discovered "by chance" the role of fluids in generating tremors. This distrust is ironic, since openness and trust are essential in building science, where no hypothesis can ever be completely confirmed and where specialists themselves must suspend disbelief in the judgments of others outside their own fields.

Formal education and training may not be essential for acquiring or using knowledge to handle risks. As early empiricists and engineers demonstrated and the Chinese recognized, equally important is a combination of human faculties, simple instruments, and a feel for the patterns of phenomena in the field. Forest rangers at old dams and even hydrologists at new ones use little more to recognize warning signs or groundwater patterns.

These Chinese scientists demonstrated a simple method of training when they intervened minimally on a case-by-case basis with new recruits, responding to requests to confirm the existence of anomalies, criticizing only to correct errors, but sustaining efforts with commendations. Amateurs thus refined their skills in observation and became experts in particular kinds of local knowledge. In this country, only two examples were found of amateurs contributing to local inquiry, a teacher tending instruments at Parkfield under elaborate contractual arrangements with the USGS and a Connecticut housewife closely supervised in monitoring seismic sounds. Yet seismology could benefit vastly from volunteers who might, for instance, help field geologists dig holes to replicate Sieh's work. But involving laymen in scientific research would require inventing new social and institutional arrangements, the topic that follows.

Alternative Social Arrangements

New models of organizing science and engineering are needed to facilitate acquiring knowledge for anticipating natural hazards, for building reliable artifacts, and for taking precautions against disasters. Such models must accommodate the fact that humans live in a diverse and ever changing reality. Even members of small local groups live in different worlds and bring fresh perspectives and different cognitive skills to the group that facilitate problem solving.

This view of reality differs from that underlying bureaucracy or expected under a model of economically rational man. Shared concerns and some trust and openness can be expected to replace competition, secrecy and suspicion. It is the model behind the EHR planners' view that success in dealing with hazards depends on decisions -- the ability to understand and willingness to act -- by individuals and groups at all levels, continuously sharing responsibility for a safe environment.

This pluralism was exemplified in California's preparations for earthquakes. Even schoolchildren could learn about their effects and what to do. Other people, the elderly, for instance, might not understand or be able to act and would need help. After a disaster, some people might be looters but many more could be expected to aid their neighbors.

When hazards do not immediately threaten, what kind of social relations in formal organizations facilitate the acquisition of useful knowledge and its application in constructing reliable artifacts? Our stories suggest clues in the organization and operation of two agencies linked to the federal government, the USGS in science and the Tennessee Valley Authority in dam engineering.

USGS, a pioneer in science in the federal government, has avoided the bureaucratic practice of top-down policy making, of imposing programs on others below or outside, or of constructing huge projects to control nature. Instead, for over a century, it has concentrated on describing natural phenomena, responsive to the needs of other agencies and cooperating with the States. With the recent exception of seismology, the USGS appears less interested in building theory than in studying phenomena in the field, assembling detailed descriptions of local conditions to meet practical needs of engineers and others inside and outside government. It therefore felt that it was more qualified to do geologic research for nuclear waste than ERDA, which had received large sums "just to kick around a few rocks."

Internally, USGS's style of operating has been somewhat unique and less bureaucratic than that of other agencies. For instance, it frequently organized researchers in small teams, as in the field near Teton, and also rotated senior researchers into administration positions, helping them to understand the relationship between acquiring knowledge and applying it. Thus responsiveness, cooperation, and internal flexibility marked its social arrangements.

USGS acquired knowledge useful for warning of natural hazards or engineering problems and it has also been willing to take a critical stance. For instance, it warned the AEC about the burial site in Idaho, discredited ERDA's assumptions on salt, and battled DOE about the geology and hydrology of proposed sites, as on the one at Hanford. In other words, it supplied critical judgment on programs within bureaucracy itself.

The TVA has been an example of social arrangements that facilitate sound engineering. Although also linked to the federal bureaucracy, like the USGS, it maintained independence, for example, by locating its main offices outside of Washington in the center of its water management system. For the TVA in a single river basin, like the early seismic engineers, physical proximity in a small and fairly concentrated area facilitated communications and awareness of potential hazards.

Like the USGS, the TVA also has had a unique operating style. For instance, rather than contracting with outsiders for new construction, the TVA hired and trained local people who knew the valley intimately and cared about its future. Its flat and two-tiered organization was what the FDS panel had in mind and no doubt contributed to informal two-way communication among designers, constructors, and managers.

They could readily meet face-to-face to discuss construction and operating problems and share understandings of how to deal with them.

The temporal continuity of the organization also facilitated intimate knowledge and communication. Operators and inspectors would be able to talk with designers and construction people, no doubt learning things to supplement written documents about how a structure was actually built. Constructing a sequence of dams in a similar geologic setting and working with them over time facilitates peer review and makes possible a kind of learning curve for design.

Another feature of TVA is the visibility of aging artifacts. Old dams dominate the landscape and enhance the imageability of failures and thereby stimulate actions. Local control and revenues also have given TVA financial independence and may explain why it was the first of the agencies in FEMA to invest in enlarging spillways. It was also first to adopt emergency plans, conservative ones that might inconvenience people in needless evacuations but were preferable to no plans at all.

A notable social characteristic of TVA, in contrast to bureaucracy, are overlapping, combined, or multiple roles. People who live in the valley and work for the TVA act as creators, beneficiaries, and potential victims of TVA dams. Even top officials who do not live directly below a dam may personally know many people who do. Multiple roles transform the chain of responsibility into a closely woven fabric from which knowledge about risks is more likely to emerge and be acted upon. Similar conditions existed in older California communities where residents were the first to impose limits on growth. Since then others have become aware of the mixed blessing of their landscape and the hazards of nearby faults, and are voluntarily taking precautions in workplaces and homes. Sharing awareness of hazards and responsibility for risks in a small area may also have contributed to TVA's recognizing a stopping rule on the construction of new dams (turning however to nuclear power).

In summary, a combination of organizational factors: a flat structure, spatial limits, continuity in time, and local control, contribute to a collective understanding and a shared sense of responsibility for engineering safety. Internal flexibility and responsiveness to the requirements of outsiders, in a larger social contest, appear to be valuable characteristics of organizations doing science. Vital too for obtaining usable knowledge and useful criticism is placing people on teams, involving them in multiple roles, and encouraging social interactions among them as co-equal partners. Such social arrangements seem to stimulate caring about the quality of knowledge and work to prevent the creation of new hazards.

Planning

In dealing with existing hazards and planning or preparing for disasters that cannot be predicted, what sort of actions are useful and how can they be organized?The following pages describe planners and planning markedly different from OSTP's, drawing on examples in the California EHR program.

Although OSTP created flat organizations for free discussion from diverse perspectives to prepare written plans, it restricted the membership on these and imposed time constraints. An alternate model of planning to produce a document was glimpsed in the time-consuming "messy process" of preparing seismic safety elements for a few California communities. The public, local officials, and staff, after conflicts and compromises, produced plans that even elected officials accepted as legitimate. We do not know who organized these processes, but we suspect that the plans were effective because the many people who participated would act as watchdogs on implementation.

We do know who were the entrepreneurs for effective planning within the federal bureaucracy, especially in the second "conspiracy" in FEMA. Both EHR conspiracies were led by mid-level people, who were in a better position to understand the whole organization than those at either the top or the bottom. Thiel also had the task of transforming general policies or plans into specific programs at the cutting edge of OSTP's double image of bureaucracy. This position no doubt enabled him to recognize the myth of a layer cake model of the federal system, as dintinct from the marble cake model of management, e.g. that management must pull together resources from different levels for appropriate actions. Local communities can do some things better than the federal government.

In both conspiracies, small groups of concerned individuals communicated informally across institutional boundaries. They were often critical of one another's ideas. They collaborated inside bureaucracy, as in the USGS-NSF gentlemen's agreement, but sought opportunities to collect support from outsiders. This operating style was anathema to the institutional separation of powers, to a formal structure for planning such as OSTP had imposed, and indeed to the rational order expected of bureaucracy. Apparently for such reasons, the NW planners neglected to work with friends in Congress and with local communities interested in hosting waste sites and lost opportunities to aggregate support for their policy.

These EHR planners soon achieved consensus on a limited problem, the need for a small local experiment, as early engineers might have done. Instead of splitting technical from non-technical matters, they combined scientific data (later deemed unreliable) on a possible Los Angeles quake with the opportunity of a local prediction plan and with intimate knowledge of how state legislators behave and of where bits of federal money could be found. They transformed antagonists into supporters by political persuasion, games, and ploys, enriching their knowledge and other resources. This synthesis of strategies, constituencies, and resources was, as Thiel had been told, the antithesis of the analytical approach of science.

In contrast, the NW planners focussed on the most contentious issue, and excluded a concerned and vocal mass of people by their "neutrality" on nuclear power. They persisted in a conflict between small practical experiments and obtaining knowledge from science, rather than seeking ways to combine both in a more robust approach. In EHR the conflict between building science and taking practical action has also persisted, as it has to some extent in dam engineering.

Temporal continuity was vital to the EHR effort, and was reflected in the planners' refusal to accept setbacks as permanent. They converted defeats to successes, for example when they got the President to endorse a prototype program, and later when the Seismic Safety Council fired the disastrous SCEPP director, cut ties with FEMA, and reorganized for more local autonomy. SSC even benefitted from the governor's competing program, so that planning for one region merged with statewide action.

At the local level, SCEPP did not produce general written plans nor detailed formal procedures but incrementally built informal support. It wrote agreements with local jurisdictions on a case-by-case basis, designed to be flexible. It did not try to create a leader nor a center nor a big organization for implementation; even the Governor and Hollywood stars were mere symbols to provoke widespread interest in the cause.

Yet this program was becoming institutionalized at the bottom, as SCEPP got into the "doingness" of changing the way people were thinking and acting in response to the threat. Many preparations required modest knowledge or simple practical actions, as when, for example, individual homeowners learned how to shut off gas lines, stockpiled food and water, secured household objects to walls, or bolted homes to sills. Neighborhood groups were organizing to share resources should a major quake occur. Other actions required semiskilled amateurs to learn to work together as teams, for instance for shortwave radio communication. A few preparations required highly skilled people to cooperate in the use of special technology, such as helicopters for rescue missions.

The model of professionalism underlying SCEPP, unlike that for dam engineering, involved complex social interactions. The professional staff not only worked as an interdisciplinary team, but collaborated closely in a kind of co-inquiry with myriads of individuals and local groups in the messy social environment, receptive to back talk and criticism. Like old time professionals, they did not rely on grand theory but on a kind of charisma to gain support for a cause.

This example illustrates how planning can work in a diversified social reality, through many people sharing responsibility, acquiring and using different kinds of knowledge, and being willing to act in a variety of ways over sustained periods of time. The California EHR program is more than a random collection of spontaneous individual actions. It has become a kind of social movement. The movement is consistently redesigned by and energized by caring "professionals," in a traditional and almost obsolete sense of that term. This ends our interpretation of the case studies. We now conclude this dissertation with a review of the literature discussed in the first chapter.

The Literature Revisited

We can now compare our position to perspectives taken in the literature. We have tried to demonstrate how planning for the management of technological risk in these cases failed because it relied on generalized models of knowledge, incorporated in scientific theories, rules and procedures of engineering, and general plans and policies of bureaucracy. Management failed because of reliance on institutional actions inappropriate to the physical and social reality from which natural hazards and technological risks arise. Both planning and management failed ultimately because the planners at the top were too far from and out of touch with this reality.

We have seen how generalized models of knowledge and institutions interacted. Both subdivided functions and tasks in organizations and among specialized fields of science and engineering, as OSTP itself did in treating each project as distinct. This process of rationalization, decomposition, and specialization also marked bureaucratic decision making, splitting broad decisions on policy and plans apart from actions, and interposing research, precise criteria, and chains of formal procedures on tiers of bureaucrats, thereby preventing appropriate and timely practical action. While small elites claimed reliable knowledge, that of all others was ignored or suppressed and their actions subjected to command and control. This was our diagnosis of OSTP's failure.

The search for a remedy was informed by Perrow's characterization of "normal accidents" in man-made systems or artifacts, which arise from unforeseeable combinations of rather normal variations in events or processes. But his diagnosis, particularly the mechanistic model of man-made systems, such as dams and other artifacts set into the earth, seems too simple. He also failed to see how a combination of inadequate knowledge and organizational glitches may have precipitated the failure of Teton.

In spite of the faulty diagnosis, we would extend Perrow's model of unforeseeable combinations of factors to natural disasters, such as earthquakes. Faults snap and tremors may occur elsewhere because of unique combinations of events at particular places, including the movement of magma deep in the earth, atmospheric changes and/or configurations of planets high above, interacting with fine-scale elements in hidden rocks. Rather than the regularities expected, such subtle couplings necessitate a more organic model of nature, as well as of artifacts. Such rare combinations of events or unique processes defy knowledge by analysis, statistical methods, or characterization by any one field of science, at a single scale, or under any one theory.

In relation to the literature reviewed in Chapter I, our diagnosis is not inconsistent with that of the radical theorists, who state that science and technology have become the dominant ideology, embedded in major institutions. Nor can we disagree with moderate mainstreamers who see conflict about technological risks undermining the authority of government and threatening economic growth. Certainly multiple defenses increase the costs of engineering projects much as liability insurance increases costs for professional services. But it makes sense to question the authoritarianism of bureaucracy and the hubris of professionals who claim exclusive expertise.

The alternatives are not stronger controls as a remedy nor is stalemate the only alternative outcome, as mainstreamers suggest. Some of Perrow's remedies are better: to simplify some systems and to stop others altogether. We would add the remedy of slowing down the process of building artifacts, both individual ones and collectively, while searching for small non-technical solutions at the local level. The results might be slower technological change but increased opportunities for learning.

In terms of institutions, our prescription is for change, not globally as the radical reformers suggest, but by building on a fundamental institution in our society, political democracy. The prescription of conservative reformers, of new institutions, is appropriate, but not at the top to elaborate an already overblown bureaucracy, but at the bottom -- many small-scale local ones to detect and warn of hazards and take precautions or remedial actions.

Our prescriptions are consistent with those political scientists who would involve the public more in decisions on technological risk, or as Smith said in FDS, give them a more active role. But their language on the conditions for participation implies an underlying mainstream model: the public should be "given" "real choices" through procedures with "unbiased" management that distribute expertise and give equal weight to social and technical matters.

In contrast, because moral views underlie all others, as Wildavsky points out, unbiased management is hard to find. Distributed expertise belies the expertise of ordinary people on particular matters, which may be more valid than the experts' generalized knowledge. As we have seen, social and technical matters are deeply interwoven, and their relative weights can hardly be quantified. Real choices may not exist. But most of all, both Nelkin and Smith make participation seem like a gift, not a right, and one that must be circumscribed by formal procedures to avoid a confusing babel of voices, conflicts, and social disorder.

Our prescription picks up on the radical approach of dialogues, open to participation and criticism by all. But not necessarily on an ideological level; global arguments are difficult to settle. Disputes can more readily be resolved by attending directly to case-by-case specifics. Local dialogues are needed, directed at whatever hazards community groups choose, such as those from a local dam, old buildings, or toxic or hazardous wastes.

Dialogues should be open to all in the chain or network of responsibility, including those with technical expertise. Ordinary people need help in understanding technical terms, in grasping the laws of probability and appreciating formal reasoning on matters of chance, as social psychologists have pointed out, but they must also trust their direct observations and knowledge. The technicians' role is not to educate people to the insignificant nature of "real risks" they face, but to act as co-equals, making accessible technical aspects of problems, as we have tried to do, and also respecting others' insights and judgments. By collaborating, all may achieve a fuller understanding, make incremental decisions, and find one voice, as OSTP would have done, but in a tentative manner on locally focussed issues. Nor is verbal dialogue enough. Intimate knowledge and a feel for phenomena require direct interactions with the physical world in order to detect errors, to correct them, and to take precautionary actions.

As Wildavsky says, risks as well as reality are socially constructed. Risks are constructed not only conceptually in the things people recognize as risky, but in the sense of physical artifacts and of decisions, as when people freely choose to live below dams or in seismic areas, if they have a choice. In contrast to Wildavsky's simple general theory of types, we found a complex and pluralistic society, for instance, entrepreneurs within bureaucratic hierarchies and lowly construction workers who were knowledgeable and critical about technical matters. Such exceptions to stereotypes open up opportunities for innovative social arrangements in dealing with risks.

If, as Wildavsky says, our technological world contains more risks than anyone can be aware of, and if natural disasters and man made failures arise in subtle and often unforeseen ways, there are plenty of hidden hazards for people to choose from. But many local groups and individuals also exist to attend to them. The majority of people may continue to hold mainstream views, but pluralism creates opportunities for many other individuals and groups to address and work to reduce particular hazards they dread.

To cope with uncertainty, or in this case to cope with unprecedented hazards, Weick would advise organizing in new ways. We would prescribe small flat organizations as substitutes for large formal hierarchies. Even in these, those at the top and bottom could cooperate with mid-level people and focus on small practical actions instead of on diffuse worries. The aim of organizing is not to create elaborate written plans but to devise original sketch maps showing qualities hitherto edited out. Instead of recipes, these could be guides to local experiments and ongoing social processes in which all have opportunities to express their voices and take appropriate actions.

From the perspective of technical rationality, approaching technological risks in this way may be like stepping through a looking glass; everything appears upside down, inside out, or reversed. But instead of treating the two perspectives as distinct and opposed, we need to combine both images, and indeed many perspectives.

We conclude with some speculations raised earlier about science. Is reduction of technological risks a matter of more and better research? Or, in spite of what scientists claim, is there something inherent in science itself that precludes knowledge for limiting these risks? Some combination of positive and negative answers seem valid. "In principle," science supplies useful general knowledge that is often reliable to describe and predict normal events. But such knowledge too often is "faulty" in practice, when specific data are poorly understood and when particular phenomena are elusive and subject to change. More research and better theory based on past observations can be of little help.

New kinds of knowledge are needed, open to understandings based on direct observations and also open to intuitions, hunches, and feelings. These may not always be expressible in words but may nevertheless be as rational as public "dread" may be. In addition, there is the need to combine and balance knowledge old and new, indigenous and foreign, from folklore and science, in particular cases. This requires artistry and new forms of social and engineering arrangements.

Social rationality enriches understanding for decisions on natural and man-made hazards, but if solutions cannot be found, it at least facilitates a diversity of coping actions. It also offers the comfort of social bonding to compensate for the irreducible uncertainty and technological risks, and the ultimate dread of individual mortality, the only real certainty. Social rationality offers the hope that society, transcending individuals, will survive.

NOTES

1. Degenkolb, "Earthquake Engineering," 117-129.

2. Alan DeMarr interview.

3. Wesson and Filson, "USGS Program."

4. Frank Perkins interview.

5. Panel, 27.

6. Issues Draft, 16.

7. "Staff Notes #9 for the Earthquake Hazard Reduction Group (WG), October 12, 1977."

8. Greenwood's "Notes on Meeting November 2, 1977," of DOE and USGS called by OMB on Nuclear Waste Management. For instance, news of USGS concerns about salt had not "filtered up" to waste managers in DOE.

9. Karl Steinbrugge in interview in Berkeley, Calif., November 24, 1972, said he made this expectation clear to the Working Group.

10. Comment in "Proposed Outline for Issues Paper," May 29, 1978, pointed out that three criteria, the redundancy of the alternatives, timing (now or later), and greater or lesser conservatism would create an unwieldly set of eight alternative strategies.

11. Greenwood, "Nuclear Waste Management," 23.

12. Ibid, 21.

13. Martin Rein suggested the germ "fogging."

14. IRG Report, 70.

15. Greenwood, Ibid. 2.

16. Hays, Program and Plans, Glossary.

17. IRG Report, 42.

18. Luth mentions one compound of interest commonly found in salt that is transformed into a liquid and evaporates in the presence of atmospheric humidity; another compound, anhydrite, greatly increases in volume in the presence of moisture.

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