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ORGANIZATIONAL MANAGEMENT OF LONG-TERM RISKS: IMPLICATIONS FOR RISK AND SAFETY IN THE TRANSPORTATION OF NUCLEAR WASTES

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Exhibit 2

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The Nevada Agency for Nuclear Products/Nuclear Waste Projects Office (NWPO) was created by the Nevada Legislature to oversee federal high-level nuclear waste activities in the state. Since 1985, it has dealt largely with the U.S. Department of Energy's (DOE's) siting of a high-level nuclear waste repository in southern Nevada. As part of its oversight role, NWPO has contracted for studies designed to assess the socioeconomic implications of the repository.

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INTRODUCTION

The transportation of high-level nuclear waste is often characterized as relatively straightforward, at least in terms of hardware; if wastes are to be transported safely, however, it will be neccessary to deal not just with hardware, but also with humans. Truly safe operations will require something close to perpetual vigilance, for each and every shipment of nuclear materials, over a period that will span decades of time and an unknown number of political and institutional changes. While nuclear waste management may involve little of the cutting-edge technical challenge, or the glamour, of the Manhattan Project or the Apollo Program, that very fact would suggest that such high-priority programs may offer less in the way of insights than may the experiences of organizations in managing potentially risky operations over very long periods of time.

While all residents of corridor states have reasons to hope that the parties assessing and managing the risks of nuclear waste transportation will do the best work they are capable of doing, Nevada has a special relationship to the transportation issue. The state has no commercial nuclear power plants, but if a repository were to be constructed at Yucca Mountain, Nevada would receive all or most of the nation's supply of high-level nuclear wastes. A particularly salient question, accordingly, is whether even "the best assessments currently available" are in fact likely to be "good enough." Even based on the good-faith assumption that the Department of Energy and its contractors will do the best job they are capable of doing, in other words, is it reasonable for the state and its citizens to assume the resultant risk estimates will be accurate, or would it be more prudent for risk-management activities to be based on the expectation that the *real* risks will differ substantially from the *officially estimated* risks of nuclear waste transportation?

By the time this report was being prepared, in early 1990, the relevance of this question had become too obvious to ignore. A long series of reports in the news media had indicated the numerous ways in which the U.S. Department of Energy (DOE), the very agency currently responsible for managing the nation's high-level nuclear wastes, had failed in its responsibility to protect the environment, and even to comply with relevant statues. Estimates of the costs of cleaning up the Department's past mistakes had already risen past a hundred *billion* dollars and were still climbing. At the same time, as if to indicate that neither DOE nor the federal government has a monopoly on mistakes in the transportation of potentially hazardous materials, the commercial petroleum industry had experienced a series of accidents in the transportation of oil by tankers, with the best-known incident being the grounding of the *Exxon Valdez*. Finally, as has long tended to be the case for transportation accidents more broadly (U.S. Office of Technology Assessment, 1986), many of the risk-management problems have continued to be blamed on "human error" of one variety or another, while virtually none of the technical experts who have been involved in the Department of Energy's risk assessments to date have had the advantage of graduate training or experience in the sciences that study human behaviors--the behavioral and social sciences.

Clearly, a set of examples--even examples as dramatic as the *Exxon Valdez* or the Federal Bureau of Investigation (FBI) "raid" of DOE's facility at Rocky Flats, Colorado--could not provide a sufficient basis for concluding either that a broader problem is present or that such problems are absent. Only a detailed, extensive, and expensive investigation could even begin to provide a definitive basis for such conclusions. On the other hand, one form of preliminary investigation can offer the potential to reach *preliminary* conclusions on whether or not the currently common approaches to risk assessment and risk management are likely to warrant a *sufficient* level of confidence as to be used for planning purposes: It is possible to examine the existing professional literature, as well as the accumulated empirical data base, asking whether there is reason to believe that the recent rash of incidents and accidents in the energy and nuclear weapons industries might reflect some larger, underlying factors, rather than being the result of "fluke" occurrences that are unlikely to occur again in the history of potential nuclear waste shipments. This is precisely the task assigned to Freudenburg and Associates and dealt with in this report.

The report comes in two parts. First, the main body of the report is a synthesis of the existing literature on human and social factors in risk management. It deals with a pair of interrelated problems that, while familiar to the social science community, appear to have received little attention to date with respect to the transportation of nuclear wastes: The human and social *attenuation* of *risk estimates*, and the *organizational amplification of risks*. Second, given the special opportunities for learning that are presented by the recent Alaska oil spill, in particular, the Appendix to this report examines the issue of *organizational foresight* in the context of the Exxon oil spill. The main body of the report has been prepared by Dr. William R. Freudenburg; the Appendix has been prepared by Dr. Lee Clarke.

Social Amplification and Attenuation.

Due in part to the publication of a paper on the topic by Kasperson and his associates (1988) and in part to the practical significance of the topic, "the social amplification of risk" has begun to receive increasing attention in the risk analysis community. Two particularly significant aspects of social amplification have received relatively little attention to date, however. The first is that human and social processes can lead to the *attenuation* of risk estimates, as well as to their amplification; the second is that the problems can include increased *risks*, not just amplified risk *perceptions*.

Clearly, the two sets of omissions can be interrelated; indeed, one ironic implication is that the *attenuation* of risk *estimates* may lead to the *amplification* of risks themselves, particularly when the attenuation takes place in the apparently precise estimates produced by members of the risk assessment community, given that systems seen as posing low risks tend not to be managed as carefully as those seen as potentially catastrophic. In the interest of simplifying the discussion, however, this report will deal with the two sets of omissions separately. The first section focuses on the social and organizational attenuation of risk estimates, identifying a pair of factors that can lead to the systematic underestimation of risk probabilities--"human error" in existing risk estimation techniques and the unrecognized statistical vulnerability of extremely low probability estimates. The second half of the report will focus on three sets of human and social factors that can lead to the amplification of risks themselves--problems introduced by individual-level or "traditional" human factors, the additional problems introduced by a broader set of institutional failures and problems that might be called "organizational" factors, and the still broader problems leading to the predictable atrophy of attentiveness. As will be noted, finally, *none* of these generally overlooked factors are dependent on the operation of deliberate or conscious biasing pressures. If such pressures exist--and they can scarcely be assumed to have vanished from the earth--they will often serve to create even greater difficulties. Even in the absence of conscious bias, however, the net effect of previously overlooked human and social factors, it appears, will be for "official" risk estimates to err systematically in the direction of underestimating the "real" risks posed by the operation of technological systems.

PART I: THE ATTENUATION OF RISK ESTIMATES

A. Human Error in Estimation Techniques

Recent years have seen an increasing awareness that many of the techniques used in probabilistic risk assessments, particularly in earlier assessments, are prone to systematic errors. Evidence of the problem continues to grow, although the *caveats* are often overlooked or forgotten once an "overall risk" estimate is produced. Several problems are particularly noteworthy.

1. *Overconfidence in the ability to foresee all possible failure modes.* Both nuclear-power accidents and transportation accidents in the past appear to have suffered from a misplaced belief that relevant failure modes had been adequately foreseen. The accident at Three Mile Island began when a valve failed to close, but the indicator on the instrument panel showed it had closed properly; the plant operators' reactions served to make matters worse (Flynn, 1984). The worst oil spill in U.S. history occurred on a calm, clear night when the *Exxon Valdez*, the largest and most expensive oil tanker owned by the third-largest corporation in the U.S., in spite of state-of-the-art navigational equipment, crossed over a "buffer" lane, another lane reserved for incoming tanker traffic, and an additional stretch of open water, before running aground on a well-known hazard that is marked clearly on navigational charts. Less than two months later, a train hauling 69 carloads of potash derailed in San Bernadino, demolishing a row of houses and most of the railroad cars, after a series of errors led to speeds of 90 miles per hour on a steep grade where the train's speed limit should have been one-third that high (Mydans, 1989). In a 1984 railroad accident, by contrast, a train's speed was within acceptable limits, but a series of upstream beaver dams had given way after heavy rains, washing out a gap in the railroad bed that was 53 feet long and 23 feet deep (Burkhardt, 1984).

A recent Associated Press story reported on a man whose house fire was caused by an incident probably not foreseen by even the most creative of insurance actuaries:

> If you don't like to mess with snakes, here's a good reason why.
>
> It happened to Al Fitzwater of Fort Wayne, Indiana. He'd been working on his car, and got inside to start it, when he disturbed a three-foot snake that had been napping under the seat.
>
> Fitzwater says the snake bit him on the ankle. Worse has yet to come. As Fitzwater stomped the snake, he hit the gas. The car, the snake and the driver then slammed into the house--which spilled a can of gas on the bumper while flames shot out of the open carburetor, setting the car and the house on fire.

From the frightening to the funny, these and many other such incidents illustrate the principle that even a lifetime studying natural laws does not give anyone the ability to *repeal* such laws--and that may include Murphy's Law.

This principle is often a source of frustration to risk assessors, sometimes because members of the affected public will insist on the impossible condition that technologies not be allowed to go forward until literally all potential accidents have been foreseen and planned for--and sometimes because overly zealous entrepreneurs and policymakers make the equally implausible claim that we *have*, in fact, foreseen all such problems (Dubé-Rioux and Russo, 1988; see also the discussion in Perrow, 1986). The problem, as many risk assessors are already well aware, is that the often-irreducible inability to foresee all possible instances of Murphy's Law creates a potential for any estimate to be insufficiently conservative (see also Fischhoff, 1977; Primack, 1975; Holdren, 1976; Freudenburg, 1988). As will be noted below, this problem is particularly noteworthy for relatively complex systems and for estimates of extremely low probabilities.

2. *Insufficient sensitivity to problems of small "sample" sizes.* Risk assessors are often placed in a situation of needing to produce estimates on the basis of incomplete or inadequate data. Under the circumstances, analysts are often forced to "make do" with whatever data happened to be available--e.g., while there is a relatively large data base on traffic accidents in general, relatively few nuclear waste shipments have taken place to date, complicating the matter of drawing conclusions about the dramatically larger number of shipments that are currently slated to take place in the future.

Usually, even isolated bits of data can provide an important basis for calculations and extrapolations, and even low-quality data may be better than no data at all. As scientists are all too well aware, however, the results of the first few trials, first few interviews, initial case studies, and so forth, often will wind up differing significantly from the results of a more thorough research program. This is the principle behind the well-known emphasis on replicability in scientific research, and it is often a cause of problems for risk assessments that are forced to rely on whatever data may be available. Even if an experiment is of high quality, the results from that experiment may provide little more than a sample of size N=1 from the universe of all possible experiments; even if the initial results are replicated in a second experiment, a "sample size" of two is not normally one in which a great deal of confidence would be placed.

This problem is exacerbated by the fact that "samples" may be not only small but biased. This is particularly likely to be a problem in "big science" (Clark and Majone, 1985; cf. Henrion and Fischhoff, 1985): the high cost of purchasing equipment, developing procedures, and so forth, may mean that only one or two laboratories in the nation or world will be capable of conducting a given experiment--meaning by implication that there will be little chance for unforeseen or underestimated sources of problems to be "caught" by other researchers. Particularly if it is important to anticipate all the ways in which something might go wrong, two heads probably are better than one, if only because of the possibility that a given laboratory will be subject to "blind spots" or to unknown or unforeseen sources of bias.

3. Failure to foresee system interactions and interdependencies. In general, one would assume or hope that, if errors are known to the persons designing and engineering the system for transporting wastes, the errors will be corrected. In many cases, however, the most troubling problems may be precisely the ones that were not foreseen. Virtually by definition, moreover, the "unforeseen" errors are unlikely to be taken into consideration in the calculation of risk probabilities.

In relatively clear-cut cases, it is tempting to refer to "designer error"--as when a designer misreads a specification, misplaces a decimal point, or makes a mistake in a calculation, and the error fails to be corrected before the system falls apart. In dealing with technological systems of increasing complexity, however, such examples of clear-cut errors may come to be less important than the more subtle "errors" created by the difficulty of foreseeing system complexity.

The importance of this problem in the context of risk estimates is that risk assessment methodologies tend to be and perhaps need to be analytical. The normal approach to the calculation of risk probabilities, in other words, is to break down the tasks in terms of the constituent components of a system--e.g., looking at each pump, pipe, valve, axle, relay, switch, drive-shaft, etc.--and calculating the probability of failure for each. Some of the most technological risks, unfortunately, are related instead to systemic or conjoint problems (e.g. the simultaneous occurrence of two or more problems that might not have been individually significant) and to factors that only exert an influence after a system has been operational for a significant period of time. Perrow (1984), in fact, has characterized the problems of complex interdependencies as being so pervasive, and so predictable, as to warrant the designation of "normal accidents." Perrow draws special attention to cases where systems are "tightly coupled," with little time being available for operators to respond to unforeseen combinations of events. (Three Mile Island would be a "normal accident" under Dr. Perrow's terminology, while the case of the *Exxon Valdez* might not be one.)

Part of the problem of dealing with many real-world risks is that the effort to delimit an analysis--e.g. by examining a single shipment of radioactive materials--is likely to divert attention away from errors that may exert a broader set of effects, such as an error in manufacturing or designing a given cask that would then affect all of the shipments in a cask or set of casks (Audin, 1987). A second part of the problem is that most of us have only limited ability to appreciate the complexities that exist outside our own areas of interdisciplinary expertise, while the real world is rarely sufficiently courteous or orderly to hand us problems that stop within the boundaries of our own disciplines. A third aspect is that the components of technological systems are often interrelated in ways that are not foreseen by any one person. For example, "the rupture of a liquid natural gas storage tank in Cleveland in 1944 resulted in 128 deaths, largely because no one had realized the need for a dike to contain spillage. The DC-10 failed in several of the early flights because none of its designers realized that decompression of the cargo compartment would destroy vital parts of the plane's control system running through it" (Slovic et al., 1984). A fourth part of the problem, which will be discussed in greater detail below, is what this report will call the "diffraction of responsibility." The complexity of an *organization* can compound the difficulty of managing a complex technology; the larger and more complex the organization, in fact, the greater may be the likelihood that we will encounter the familiar complaint that an important detail simply slipped through the cracks. In such situations, even the persons who sense the potential significance of a problem may fail to take the initiative to correct it: "That's not my department."

In the case of many technological risks, the interrelationships and interdependencies of greatest significance may be associated with systems that involve both humans and hardware. For many of the accidents that are blamed on "human error," for example, closer examination reveals a verdict that is not so clear-cut. The accident at Three Mile Island provides an illustration; while the accident is commonly blamed on "human" error, meaning "worker/operator" error, closer examination reveals a different picture. A key cause of the accident came when a valve failed to close, although the indicator light in the control room indicated (erroneously) that it had in fact closed. Within minutes the operators were subjected to an incredible sensory overload of alarms, warning bells and flashing lights--the President's Commission on the Accident at Three Mile Island (1979:11) referred to it as "a cascade of alarms that numbered 100." The operators did what any rational human being would have done, which is to turn off as many of the "extra" distractions as possible, all the while trying to figure out what was "really" causing the problem. In many cases, once the alarm had been shut off, there was no record of just which of the switches had been tied to which problems. Perhaps the main rationale for blaming "operator error" for the problems is that there were two instruments in the quite large control room that could have told the operators the source of the problem--assuming it is reasonable to expect even a genius to have been able to identify the two relevant stimuli in such a cacophony of confusion. One of the instruments, however, was on the opposite side of the control panel, not visible without getting up and physically walking around the panel; the other was obscured by a tag attached to a neighboring handle that indicated it was in need of repair (for further details/discussions, see Rubinstein, 1979; Marshall, 1979; Flynn, 1984; President's Commission on Three-Mile Island, 1979; Rogovin and Frampton, 1980). Even when some of the best minds in the American nuclear industry came to the control room over the next several hours in the effort to get the situation under control, moreover, the situation was "dangerously out of control for at least 48 hours," according to a report on the accident in *Science* (Marshall, 1979).

While this brief discussion is clearly an oversimplification, it does lead one to wonder whether, when even best experts in the country were unable to get the system back under control in a matter of many hours, the risk assessment community would be justified in blaming the Three-Mile Island accident on the operators who were unable to get the system under control in the first few seconds. At a minimum, the Three Mile Island example should illustrate the need for human behavior to be taken into account more systematically in the engineering and developmental stages of technological development. Before a given pump is installed in a nuclear power plant, it is generally expected to have gone through sufficient testing to provide reasonably reliable statistical data on such considerations as the mean time between failures of a given type. Perhaps the risk assessment community needs to begin now to devote enough more attention to the behavior of "human components" to begin to identify with increased accuracy the types of "failures" that are likely to occur in which types of persons, under what kinds of circumstances, and with what degree of frequency--not just in the laboratory, moreover, but under the often-perverse realities of real-world operation.

4. *"Calibration errors" and Cognitive Dissonance.* While members of the risk assessment community often refer to the difficulties experienced by members of the general public in calculating probabilities, it is less well understood that calibration errors--mistakes in estimating probabilities--also present serious problems for persons with scientific training. The most serious problems occur in the absence of reasonably definitive data--a situation that unfortunately characterizes much of a risk assessor's work.

As one way of illustrating the difficulty of calculating confidence intervals, even for scientifically trained persons, the author of this report has recently conducted a small, informal experiment. In a series of four presentations to technically oriented audiences with a combined attendance of approximately 700 persons, he has asked those in attendance to produce a simple estimate of a 90% confidence interval--a pair of estimates sufficiently far apart to guarantee a 90% probability that "the true number" is contained between them. The topic is one with which most persons have at least a reasonable level of common-sense familiarity: The risks of driving.

As might be expected, not all hours of the week are equally "risky" in terms of the likelihood of fatal accidents per mile driven. Common sense even provides a reasonable inkling of which are the safest and least safe hours: The riskiest hour is around 3:00 a.m. on "Saturday night" (Sunday morning), when a relatively high proportion of the drivers on the road are tired and returning from parties or other forms of social relaxation, often including the consumption of alcohol. The safest hour of the week is roughly nine hours later, around noon on Sunday, by which time party-goers have returned to their homes, and a high proportion of traffic consists of families who are on their way home from places of worship, presumably being on their best behavior. Each audience was asked to estimate the ratio between the safest and the riskiest hours of the week. Are persons 10% more likely to die, per mile driven, at 3 a.m.? Ten times as likely? Ten thousand times as likely? Readers are urged to pause for a moment here to decide what their own estimate would be.

Next, persons in attendance were asked to come up with a 90% confidence interval--a pair of estimates sufficiently far apart that each person in attendance had a 90% level of confidence that "the true number" would be found somewhere between these extremes. Given that, for most people, the bottom end of this confidence interval is "practically no difference," attention was focused on the upper bound. (Readers are urged to select their own upper 90% confidence bound at this point; Pick a number sufficiently high so there is a 90% probability that the empirical ratio between the safest and riskiest hours of the week will be lower than the upper bound you select.)

If in fact the confidence intervals so selected had averaged out to be true 90% confidence intervals, roughly 630 of the persons in attendance--90% of the 700 or so persons in the four audiences--should have picked upper bounds high enough to have included the "correct" number. In fact, only 34 of the estimates were even close. According to the best estimate available (Schwing and Kamerud, 1987), deaths are over 100 times as likely, per mile driven, during the 3:00 a.m. hour than at noon on Sunday. Only 34 of the 700 estimates--roughly 5% of the total--called for deaths per mile to be at least 100 times as likely. Virtually none of the estimates were sufficiently high to include the figure developed on the basis of empirical experience, in other words, and even under the relatively generous criterion of accepting as a "hit" any estimate that the ratio was at least 100:1, what was supposed to be a "90% confidence interval" actually wound up, in the aggregate, deserving approximately a 5% level of confidence. Even under a charitable reading, in other words, at least this set of 90% confidence intervals turns out, in the aggregate, to have been correct only about 5% of the time.

While the vast majority of the persons in these audiences held advanced degrees, usually in engineering or the sciences, and virtually all of them presumably had at least some experience with the risks of driving, it could be argued that the estimates would have been closer to the mark in cases where persons were making estimates of probabilities *within* their own areas of specialty. Unfortunately, the real world often refuses to stay within the disciplinary boundaries that are set up for intellectual convenience, meaning that risk assessors are often forced to tread outside their own specialty areas. Perhaps even more tellingly, available evidence suggests that even disciplinary expertise might not be sufficient to guarantee a higher "hit rate."

Scientists have tended to display an excessive confidence in estimates, even in the fields as well-developed as physics and when dealing with a quantity as fundamental and as carefully measured as the speed of light. A compilation of the 27 published surveys of the speed of light between 1875 and 1958 that included formal estimates of uncertainty found that the measurements differed from the official 1984 value by magnitudes that would be expected to occur less than 0.0005 of the time, by chance alone, when using the original estimators' *own* calculations of the uncertainties in their estimates (Henrion and Fischhoff, 1985). The absolute magnitude of the errors declined significantly over time, with improved measurement techniques, but there was *not* a corresponding improvement in estimates of the remaining uncertainty. The straightforward conclusion is that "the respective investigators' uncertainties. . .must [have been] significantly underestimated." The 1984 estimate of the speed of light (which has since been used to calibrate the length of a meter, rather than vice versa) falls entirely outside the range of standard error (1.48 x "probable error") for all estimates of the true speeds of light that were reported between 1930 and 1970 (Henrion and Fischhoff, 1985, Figure 2).

Other examples can be reported for scientists who range from engineers to physicians. One study asked a group of internationally known geotechnical engineers for their 50% confidence bands on the height of an embankment that would cause a clay foundation to fail; when an actual embankment was built, not one of the experts' bands was broad enough to enclose the true failure height (Hynes and VanMarcke, 1976). Another study followed a group of patients who were diagnosed on the basis of an examination of coughs to have pneumonia; of the group listed by physicians as having an 85% chance of having pneumonia, less than 20% actually did (see Christenson-Szalanski and J.B. Bushyhead, 1982; see also De Smet, Fryback and Thornbury, 1979; Lichtenstein, Fischhoff and Phillips, 1982). Other studies of the ability to assess probabilities accurately--the problem of calibration--have found that calibration is unaffected by differences in intelligence or expertise (Lichtenstein and Fischhoff, 1977), but may be increased by importance of the task (Sieber, 1974). Overall, one would expect that only about 2% of the estimates having a confidence level of 98% would prove to be surprises, but nonspecialist assessors may have a "surprise index" on the order of 20% to 40% (see for example Fischhoff, Slovic and Lichtenstein, 1977; see also Lichtenstein, Fischhoff and Phillips, 1972; for a broader discussion, see Wynne, 1982).

In general, scientists may be subject to an understandable problem of doing a good job of "predicting" more of something that is already familiar, while missing the likelihood of surprises. For example, Henshel (1982) notes that "while demographers have often done a reasonably good job of "predicting" populations during times of relative stability, they have generally failed to anticipate "surprises" such as the baby boom of the 1940s-50s or the birth dearth of the 1960s-70s.

Other studies suggest a more troubling implication: It may be that the underestimation of risks is particularly likely when people are reflecting on the risks of the activities in which they normally engage. Weinstein reports a pervasive "it can't happen to me" (or "it probably won't happen to me") attitude when people are estimating the risks of a wide range of everyday behaviors, ranging from automobile accidents to arteriosclerosis (Weinstein, 1972, 1984, 1988). The underestimation of risks also appears to be widespread in "risky" occupations, ranging from fishing (Tunstall, 1962) to high steel work (Haas, 1972, 1977), to coal mining (Fitzpatrick, 1980; Lucas, 1969), to police work (Skolnick, 1969), to offshore oil work (Heimer, 1988), extending even to recreational parachute jumping (Epstein and Fenz, 1967) and professional gambling (Downes, 1987). While it is possible to hypothesize that experience will breed accuracy, in short, the limited evidence available to date suggests just the opposite; in fact, familiarity appears to generate at least complacency, if not exactly contempt. The "cognitive dissonance" perspective (Festinger, 1957) would suggest a stronger conclusion: It may be that persons in "risky" occupations could generally tend to ignore, minimize, or otherwise underestimate the risks to which they are exposed, thus reducing the "dissonance" that might be created by focusing on the risks implicit in one's occupational choice.

B. Statistical vulnerability of low-probability estimates.

A particularly troubling problem has to do with low-probability estimates: Most of the more severe accidents that are possible in the transportation of high-level wastes have been seen as having very low probabilities of occurrence, but low-probability estimates appear to be especially prone to error. The following discussion, which draws from Freudenburg (1988), notes just some of the more notable problems.

1. Nontestability. Particularly for some of the more bizarre or unthinkable sets of threats to a transportation system, ranging from terrorist attacks on casks for transporting nuclear waste to catastrophic, multi-vehicle crashes involving fires and explosion with caustic materials, we may never be able to know what the "worst" possible threat would be. In most senses, this is quite a good thing. Most of us would rather not "find out the hard way" that there is something even worse than whatever we consider to be the worst "credible" set of circumstances likely to be faced by the system. Unless challenges to the system of at least such a magnitude actually occur, moreover, such considerations are largely academic. Still, while it is to be hoped that there will be no "opportunities" for such tests to take place, hope is not the same as knowledge, and until or unless the systems are actually tested by "unlikely" or "impossible" events, we are actually placing faith in untested assumptions.

When we consider new or previously untried technological systems, such as the proposed but not-as-yet implemented system for transporting wastes, there is a potential for a special case of nontestability. As Weinberg pointed out in his classic essay on "trans-scientific" questions (1972), some technologies are likely to be so complex and expensive to develop that there really is no way of "testing" them before they are built; the cost of a test would be comparable to the cost of building the system itself--potentially running well into the billions of dollars--and in the case of a technology having relatively unique requirements, such as an underground nuclear waste repository, there may be so few potentially acceptable sites that it would be politically unacceptable to devote one of those sites to a testing program that might later make that same site unsuitable for a repository itself. Quite literally, there may be no way to test our full set of assumptions about a disposal site and associated transportation system without actually building and operating the system. The decision to build may or may not be justified in the absence of the testing that would normally be desired, depending on values and other considerations; in any case, however, such a decision needs to be made only with the full recognition that the assumptions have not had the opportunity to be tested before being applied in a real-world test.

2. *Nonfalsifiability.* In at least one respect, those of us with training in probability theory may be subject to a potential bias that is rarely found among the general public.

Nearly all of us have had the frustration of attempting to explain to the lay public, or even to our own students, that events with one-in-a-million probabilities are not impossible; a low estimated probability is not necessarily called into question if an unlikely event does in fact occur. Of all the events that are expected to occur only once every thousand years or so, we explain, some can be expected to occur each year, and a tiny proportion may even occur more than once per year. Yet our very understanding of these principles may sometimes cause our hypotheses about extremely low probabilities to become effectively non-falsifiable.

The familiar statistical problem of Type I and Type II error--of rejecting hypotheses that are ultimately found to be true, on the one hand, or failing to reject those that are actually false, on the other--can take on a new complexity in cases of incidents that are expected to occur once in a million reactor years, for example, but that actually occur twice in a single year (Marshall, 1983). If empirical operating experience is limited, we have no scientific basis for "proving" whether the estimated probabilities are too low or too high. If we stick with our original estimates, we avoid discarding our estimates on the basis of what may prove to be isolated experiences, but in doing so, we make a *de facto* decision to trust estimates that may be incorrect. While many areas of risk assessment provide us with enough experience to permit the identification of such errors, events that are truly rare--or technologies that are still new or untried--may provide too little information to permit such corrections.

3. The statistical power of the hidden flaw. Low-probability estimates are especially vulnerable to the inaccuracies created when our calculations fail to take into account even a small number of unforeseen events. Contrary to what some might see as a "common sense" expectation, the failure to recognize a problem in one portion of a probabilistic analysis is often not offset by an exaggerated conservatism in another portion of the analysis.

Consider a technology estimated to have a one-in-a-million chance of failing. For simplicity's sake, assume that risk assessors had succeeded in identifying all potential risk factors but two--one that made the technology safer than the official estimate, and another that made it less safe. To keep the example simple, imagine that the technology would still operate at the one-in-a-million level of risk in 80% of operational circumstances, but that 10% of the time, the real risk would be one in a thousand, and 10% of the time the risk would be one in a billion. Then the true risk of the technology would be ($.1 \times 10^{-3} + .8 \times 10^{-6} + .1 \times 10^{-9}$)--that is, 10% times 10^{-3} (one in a thousand), plus 80% times one in a million, plus 10% times one in a billion, respectively--for an overall probability of .0001008001, or slightly more than one in ten thousand. Rather than being offset by the presence of the unexpected safety factor, in short, *the unexpected problem dominates the ultimate risk probability*. Indeed, even if the risk assessment had been so conservative in other respects that the "real" risks were no higher than one in a *trillion* except for the 10% of the operating experience where the one-in-a-thousand estimate would hold, the overall probability would still be higher than one in ten thousand--more than a hundred times greater than the supposedly "conservative" estimate.

PART II: THE ORGANIZATIONAL AMPLIFICATION OF RISKS

As noted at the beginning of this report, human and social factors can lead not just to the underestimation of risk probabilities in waste transportation, but also to the amplification of "real risks." To some extent, of course, the amplification of risks is implied by the underestimation of risk probabilities, in that unexpected (or "under-expected") risks are unlikely to be adequately taken into account in the normal process of planning for risk management. In addition, however, there is a clear possibility that human and organizational factors will lead *directly* to increased risks. There appear to be at least four sets of factors that have received insufficient attention in the literature to date. As will be discussed here, the four include "standard" or individual-level human factors, organizational factors, the atrophy of attentiveness, and the imbalanced distribution of institutional resources.

A. Individual-Level Failures and "Human Factors"

Subsection A of this Section will deal with three categories of human factors--the errors of individual culpability that tend to receive at least occasional mention in risk assessments, the errors that are predictable only in a more probabilistic sense, and the actions of persons who are external to the systems normally considered in risk assessments to date. The broad range of "human factors" that are traceable to the actions of organizations, rather than individuals, will be discussed in Subsection B, below.

1. Traditional Human Factors. "Human error" is a value-laden term, one that has often been used to describe situations that might more appropriately be blamed on mismatches between people and machinery (cf. Egan, 1982; Flynn, 1984; Freudenburg, 1988). In addition, to the extent to which human behaviors have been considered in the risk analysis literature to date, the focus generally has been limited to problems of individual workers. These problems range from insufficient levels of competence (due to limited intelligence, inadequate training, and absence of necessary talents, etc.) to factors that are often associated with low levels of motivation (laziness, sloppiness, use of alcohol/drugs, etc.).

As a rule, these traditional human factors have four commonalities. First, they are often seen as the "fault" of the individual workers involved, rather than of any larger organizational systems. Second, they tend by their nature to be preventable and/or correctable. The third and fourth points of commonality may tell us something more. The third is that, in official investigations conducted after accidents and technological disasters, such "human errors" are perhaps the causes identified most frequently as the core problems, but fourth, in the risk assessments that are done *before* the accidents, the same set of factors tends to be mentioned only in a vague and/or passing way.

At the risk of emphasizing the obvious, it needs to be noted that the potential range and significance of human errors could scarcely be overemphasized--but can readily be overlooked. As the common saying has it, "It's hard to make anything idiot-proof--idiots are far too clever." The problem is particularly pernicious in the case of systems that are estimated to have extremely low probabilities of failure, as noted below. Even the literature's limited degree of attention to such "standard" human factors, however, is more than is normally accorded to the organizational and systemic elements of technological systems and their risks. Given that traditional human factors receive at least some degree of attention in the existing risk literature, this report will move instead to other categories of human behavior that appear to require greater attention in the future.

2. *"Stochastic" human factors.* Aside from the fact that certain individuals may indeed have insufficient capacities and/or motivations to perform the jobs they are expected to do, there is limited but growing evidence that many of the technological systems involving both humans and hardware may systematically (and perhaps unnecessarily) lead to what might be called "stochastically predictable" problems. Even among workers who are intelligent, properly trained, and highly motivated, there is a potential for fatigue, negative responses to stress, occasional errors in judgments, or prosaically predictable "bad days." This category of problems can be described as "stochastically predictable" in that virtually anyone with even a modest familiarity with human behavior knows that such problems are likely to "happen," as the recent bumper sticker puts it, but that the exact problem/mistake, person committing that mistake and time of commission can be "predicted" only in a probabilistic sense. Accidents are more likely to occur in the five hours after midnight than in the same number of hours before, for example,

but the *specific* problems and their time(s) of occurrence appear to be almost completely chaotic or random.

If there is an exception, it is in the way in which much of the work in technological systems is structured. Intriguingly, it is possible that typical or "engineering" responses to this problem may tend in fact to make it worse: There may be something like a generic difficulty for humans in maintaining attentiveness to jobs that amount to little more than routine monitoring of the equipment that "runs" a system except in times of emergency--as in the kinds of jobs sometime described, with reason, as involving "99% boredom and 1% sheer terror." Yet these are precisely the kinds of systems often developed in response to failures of human attentiveness. The limited available research on human/technological systems that have avoided error more successfully, such as aircraft carriers (Rochlin, 1987; Rochlin et al., 1987), generally suggests instead that most people do better if the systems they operate require them to remain attentive, even at the cost of considerable tension or pressure. The issue of organizational factors that contribute to empirical risks will be considered further below, after a brief discussion of "human factors" that originate from outside of the organizations that have responsibility for the management of technological systems.

3. "External" human factors. As noted elsewhere (Freudenburg, 1988) and occasionally considered at least in a qualitative way in risk assessments, problems can also be created by the actions of persons external to a technological system itself. The most commonly considered examples of "external" human factors have to do with terrorism and/or sabotage activities, whether instigated by disgruntled former employees, social movements that are opposed to a given technology, or other types of actors. While the United States has been quite fortunate to date in avoiding most forms of overt terrorism, closer examination might reveal that the odds of such deliberate intrusions are too great to be safely ignored--perhaps being on the order of several percent per year, rather than posing a one-in-a-million chance (Freudenburg, 1988).

Other possibilities for external human intervention range from acts of neighbors to acts of Congress. At least some observers have concluded that the infamous Love Canal incident, for example, was due not just to the actions by Hooker Chemical Company, which filled a trench with its waste chemicals, but also to later real estate and urban development. After filling the trench, Hooker Chemical Company covered the site with a layer of clay and then deeded it to the local school district for $1.00; it was after that time that construction and even excavation for homes and highways may have led to considerable water infiltration, which later led to the "leaking" of the chemicals from the waste site into neighborhood homes.

Perhaps somewhere in the middle of the continuum of culpability, between deliberately malicious actions by terrorist groups and relatively naive actions by ignorant neighbors, would be actions motivated by political and/or economic forces. Each session of Congress, for example, makes dozens of decisions on appropriations and related questions that can have direct effects on transportation safety. Otherwise routine budget cuts may result in inadequate maintenance of the interstate highway system and associated bridges or in reducing the number of federal inspectors below the number needed to enforce regulations pertaining to hazardous materials shipments. Other actions, such as the recent proposal to slash the investigative staff of the National Transportation Safety Board, could severely reduce the ability of the federal government to investigate transportation accidents, directly affecting the availability of the data needed for risk assessment and risk management.

At the same time, some of the Congressional decisions that affect nuclear wate transportation are shaped by broader policy preferences, and they are often anything but routine. A recent example is provided by the Nuclear Waste Policy Act of 1982, which established a national policy for disposing high-level nuclear wastes. While the Act was originally passed only after long, careful, and highly public debate, the process of amending the Act has proved to be far less careful, and it has received much less attention. In December, 1987, the Chair of the Senate Appropriations Subcommittee for Energy and Water Projects engineered the passage of the "Nuclear Waste Policy Act Amendments" by adding an amendment to an appropriation bill that needed to be passed to keep the government in business. This bill, which was dubbed by Nevada's governor as the "screw Nevada bill," also "amended" the process of site selection, discarding the official process of studying three sites extensively before picking the "best one." Instead, it directed the U.S. Department of Energy to proceed with the study of a specific site in Nevada and not even to consider other sites until or unless this first site were to be found unsuitable. In the next two Federal fiscal years, in legislative maneuvering that received even less attention outside the state of Nevada than the 1987 Amendments, the same Senator imposed severe constraints on what, under the original legislation, was supposed to have been an "independent" study program. The appropriation for fiscal year 1988--which was passed less than two weeks before the start of the state fiscal year to which it applied--cut by 50% the level of support for the state studies that had already been negotiated between the State of Nevada and the U.S. Department of Energy; the surprise amendment even named specific studies for which the state was forbidden to spend more than statutorily given amounts. The appropriation for fiscal year 1989 effectively cut even this reduced appropriation by roughly 90%.

Against a backdrop such as this, it may not be prudent to assume that, where the safety of a facility or site will depend in part on actions to be taken by elected or appointed officials many years in the future, the policies in existence at the time when a risk assessment is done will be the policies actually followed at those future times. The exact probabilities may be impossible to calculate, but they will almost certainly be less than 100%.

As a relatively straightforward illustration, imagine you are a Senator and the year is 2010. The Federal budget deficit is a major issue--still. Your constituents are demanding a variety of services, ranging from plans to build new jet-ports to the need for retirement/health care facilities for aging baby-boomers. You face a tough re-election campaign next year. Taxes are "already" too high--still. In this context, when an official from the future "Department of Environmental Remediation" testifies reluctantly that her agency will need an additional $82.5 billion "to fulfill a promise we made to the American people back in 1990"--for example, to clean up the messy results of a series of mistakes in a far-away western state--which would you choose to do: fulfill someone else's ancient promise to that far-away state, or fulfill your recent campaign promise to bring more jet-ports to your own? At a minimum, it appears, the likelihood of future fulfillment of promises should be taken as something less than a certainty; under certain conditions, in fact, the probability may prove to be well under 50%.

B. Organizational Failures and "Organizational Factors"

In addition to the actions of individual humans, however, the actions of *organizations* can have a far greater influence on real risks than has been recognized in the risk assessment literature to date. Partly to preserve a symmetry with the common discussions of "human factors"--most of which have to do with characteristics of individuals--this report will refer to the next set of considerations as "organizational factors." As will be noted, there are a number of ways in which such organizational factors need to be seen as *expected*, rather than as "excepted" from our analyses. Our organizations, it appears, are faced with a perplexing panoply of systematic organizational/institutional factors, the net result of which will be to increase, rather than decrease, the "real" risks posed by technological systems.

1. Organizational Variations in Commitment to Risk Management. Just as individuals can differ greatly in terms of personality, competence, motivation, and so forth, so too can organizations. Some organizations manage to operate nuclear power plants efficiently, safely, and with a high level of availability; others are less successful. Some organizations make a genuine commitment to worker safety and environmental protection; others do little more than go through the motions. All of this is hardly new information for the risk assessment community; unfortunately, it is information that is largely ignored in our analyses.

While informal discussions often center around the problems of organizations having less-than-impressive levels of commitment to safety and risk management, most of the quantitative risk analyses produced to date would appear to describe an unknown world where all such problems have magically been banished. In general, we fail to acknowledge in print what almost all of us have at some time acknowledged informally: organizations' standard operating procedures are sometimes more likely to be ignored than to be followed, particularly when it comes to procedures that are intended to improve the safety of an operation rather than to boost the rate of production.

This collective oversight is more than a matter of mere academic or incidental interest; in some cases, in fact, the lack of organizational commitment to risk management may be one of the *predominant* sources of "real risk" from the operation of a technology. Particularly in the case of "technological" failures that have received widespread public attention, such organizational factors are so common that the field can no longer afford to ignore them--if indeed it ever could. To return to some by-now familiar cases, the President's Commission on the Accident at Three Mile Island (1979) began its investigation looking for problems of hardware, but wound up concluding the overall problem was one of humans--a problem of a pervasive "mind-set" in the nuclear industry at the time, which contributed substantially but unnecessarily to the likelihood of accidents. At least according to some of the reports in the popular press, the accident at Chernobyl took place while the plant was operating with important safety systems disabled. The explosion of the space shuttle *Challenger* has been attributed in large part to the "push" at NASA, the space agency, for getting shuttle missions launched on a regular schedule (see e.g. Vaughn, 1989). The recent accident with the *Exxon Valdez* has been described even by the *Wall Street Journal* as reflecting a relatively pervasive lack of concern by both Exxon and Alyeska with the companies' own risk management plans (McCoy, 1989).

This list could be expanded, but the purpose here is not to point fingers at specific cases of organizational failure; rather it is to raise the point that, if we wish our risk analyses to be guided by scientifically credible, empirical evidence, rather than by our own wishes about the way the world would look, we cannot responsibly accept any risk analysis that treats such common problems of organizational errors as if they simply did not exist.

2. *Bureaucratic Attenuation of Information Flows*. In addition to factors that may affect only some organizations, however, there are also factors that appear to influence virtually all organizations, particularly in the case of larger organizations. One of the simplest has to do with the attenuation of information flows. To consider a recent accident that "should not have occurred," the explosion of the Space Shuttle *Challenger*, a number of reports called attention to the fact that the people with technical know-how had expressed concern, sometimes quite forcibly, about the potential dangers of launching the *Challenger* under low-temperature conditions, but the persons at the top of the organization reported never having heard anything about such concerns. This report, in

turn, prompted any number of responses, most of which were variations on the question, "How could that be?"

For anyone who has studied organizations, at least part of the answer is quite simple, and it does not even require us to conclude that any conscious cover-up actions were involved. The basic fact is that communication is always an imperfect process, and the greater the number of "links" in a communication chain, the greater the likelihood that important pieces of the information will fail to get through. The common illustration of rumor transmission provides an example: If a "secret" is whispered to one person, who then transmits it to another, who transmits it to still another, by the time the message gets all the way around the room, it is often virtually unrecognizable. A quantitative example is provided by Freudenburg (1988): If we assume there will be a .7 correlation between what any given person in an organization knows and what that same person's supervisor will know about the same issue, this means that just two organizational "layers" would reduce the correlation between the specialists' understanding of a technology and their supervisors' to less than .5 (.7 x .7 = .49), and seven layers would reduce the correlation to less than 0.1 (0.7^7 = .082).

The next step, however, is to ask whether all kinds of information are equally likely to "get through" an organizational chain of communication. The answer appears to be negative--and in a way that has further implications for the prudent assessment and management of risks. To put the matter simply, "bad news" is particularly unwelcome. While organizations may no longer literally follow the practice of executing the bearers of bad news, there is nevertheless a well-known problem of supervisors who claim to want the full story but nevertheless seem to persist in hiring "yes men." At a slightly broader level of abstraction, the problem has to do with the fact that most people do not enjoy hearing bad news, and the disinclination to be confronted by discouraging words may be especially high in organizations characterized by a strong commitment to goals.

Goal commitment is generally helpful or functional for an organization--it helps people to work harder and in a more coordinated fashion, for example--but it tends to be correlated to an unfortunate problem with respect to risk management. "Don't tell me about problems," supervisors are sometimes heard to say, "Tell me what we can do about them." Unfortunately, in the case of many areas of risk management, what the organization *can* do about a risk is often something the organization would rather not do. To return to the *Challenger* accident, technicians who suspected there would be problems with the O-ring seals, particularly at low temperatures, could have suggested (and did) that the launch be delayed for warmer temperatures. Such a step, however, would have put the agency further behind in its ambitious launch schedule; completely redesigning the problematic seals, as the agency is now doing, would have created both delays and costs of a magnitude that clearly would have been considered unacceptable--until *after* the agency experienced the unfortunate alternative. Not just the *Challenger* disaster, but other experience with organizational behavior, suggests that when the problems being identified are serious and unpopular, and when the available "solutions" are even less "acceptable," the likely outcome is that there will be a systematic filtering of bad news and a corresponding "emphasis on the positive" in the news that is actually passed on to superiors and to those superiors' superiors.

3. Diffraction of Responsibility. In addition to creating a possibility that a given piece of known information will fail to get through, organizations can create a significant possibility that an important piece of information will remain unknown or unrecognized. In essence, complexity can help to create the organizational equivalent of Catch-22: The specialized division of responsibility creates not just the possibility that a single weak link will cause the entire "chain" to fail, but also creates the possibility that one or more links will have been forgotten altogether. Not only is each office or division expected to do its job properly--to make its own "link" of the chain adequately strong--but each is freed of responsibility for other links of the chain. The common and generally understandable excuse becomes. "That's not my department."

The problem appears likely to be especially severe in the very kinds of large and complex organizations that have been evolved to manage "advanced" technological systems. Catton (1985: 264) refers to what he calls "corporate gaps" in providing this account of Air New Zealand 901, a sightseeing flight that flew directly into the north face of Mount Erebus, an Antarctic volcano. While "pilot error" was the first explanation offered by authorities, the facts of the case proved to be more complex:

> When the plane collided with the mountain, killing everyone on board, it was flying in clear air, beneath a cloud ceiling that diffused the daylight in a way that made the upward sloping white surface of the mountain directly ahead indistintuishable from the horizontal white expanse all five pairs of eyes in the plane's cockpit had every reason to suppose they were seeing. According to the destination coordinates the pilot had been given in his preflight briefing, they were on a safe route down the middle of ice-covered McMurdo Sound. Due to changed destination coordinates the airline's Navigation Section had inserted into the aircraft's computer, they were instead flying toward a point lying directly behind the mountain (Mahon, 1981).
>
> It was not the job of the person who had "corrected" a supposed error in the flight plan to notify the pilot that a change of coordinates had been made. It was not the computer programmer's job to inspect the pilot's navigation chart to see if his preflight briefing had agreed with the information going into the Inertial Navigation System computer. It was not the responsibility of the Williams field controller to ask the pilot whether his preflight briefing and his computer held the same information. It happened from the division of labor and it was nobody's fault. Two hundred fifty-seven lives fell through the cracks.

In fact, the diffraction of responsibility may something close to a *generic* problem in the management of technological systems. In some cases, observers may detect something closer to a deliberate denial or abrogation of responsibility (cf. Bella, 1987) and may react to it with a form of indignation; the existence of the gaps created by the widely scattered assignment of responsibility, after all, can create problems that are technically "nobody's fault," as well as freeing each of the individual actors or departments from bearing responsibility for the collective consequences of their combined actions. As Catton (1985: 254) notes, musical humorist Tom Lehrer once succinctly criticized just such a problem in military thinking: "'Once the rockets are up who cares where they come down? That's not my deparment,' says Wernher von Braun."

The effort here, however, is to point out that important considerations can "slip through the cracks" unintentionally, as well, and in two ways. First, given that the complexity of techological systems can make it virtually impossible to foresee all of the ways in which problems might arise, the obvious implication is that managers of the system can be placed in a situation of being unable to assign responsibility for each of the components of the system that might prove later to be crucial. Second, *the complexity of the organization* can itself create difficulties, oversights, omissions, and lacunae of responsibilities. As the supervising engineer of a large project once explained to this author,

> The damned plant has got so many ways of going wrong that *nobody* really knows what they all are, not even me. Back when I first started, if I worked on a car, I not only knew how my part of the system worked--I knew how the whole thing worked. On this project, we're lucky if the various teams even know how their parts of the plant are supposed to work, and nobody but God Almighty really knows how the whole thing fits together.

4. Displacement, routinization, and the concern for efficiency. Virtually all organizations also have some difficulty with means/ends displacement, or goal displacement: While an organization may have been set up originally to protect health, clean up pollution, or find and develop oil reserves profitably, the persons in the bureaucracy are likely, over time, to devote an increasing share of their attention to "bureaucratic" concerns. Over time, in short, the "real" goals of a department can come to be what were originally seen simply as means to an end--increasing the size of the departmental budget, for example, or attempting to purchase new equipment for a given office. A second form of displacement takes place when organizational accounting comes to focus on *resources expended* rather than results accomplished: Particularly in government agencies having hard-to-measure goals, such as "improving health care for the aged" or "protecting the public health and welfare," an emphasis on accountability often becomes an emphasis instead on accounting measures--number of clients seen, rather than the improvement of health for each, or number of regulations issued, rather than improvements in the actual level of operating safety of power plants, transportation systems, and the like. While the problem of displacement is well-known in studies of organizations and in evaluation research, it has not yet received proper attention in risk analysis, particularly with respect to systems whose safety is likely to depend in part on the exercise of long-term monitoring by future organizations.

A particularly important form of means/ends displacement has to do with the importance of routinization and the concern for efficiency. Some of the most important challenges and opportunities from the *Exxon Valdez* accident lie in the area of forecasting the most likely organizational responses to the accidents that do indeed occur. The responses are likely to be extensively conditioned by routinization--a problem that is likely to be especially severe for the accidents that are the "least routine" or most rare.

In the case of the Alaska oil spill, the "drills" on emergency preparedness conducted before the spill might have suggested to astute observers that greater attention to spill response would have been in order; neither the equipment nor the organizations worked as planned, and the drills "sometimes were near-disasters themselves" (McCoy, 1989: A4). Such lessons, however, were evidently overlooked. As noted cogently in Clarke's Appendix to this report, at least five contingency plans were in effect at the time of the spill; among other commonalities, all the plans envisioned not only that rescue and response equipment would be at the ready, but that materials would be deployed in a carefully coordinated manner, with "an efficient and effective division of labor among organizations" being instituted almost immediately. Communication channels among previously competitive or even adversarial organizations would be established readily, interpretations of the communications would be unproblematic, and each organization or agency would take precisely the right step at precisely the time that would best fit the need of other organizations.

The reality, of course, could scarcely have been less similar to the plan. Confusion appears to have been far more commonplace than communication; a number of important steps either failed to be taken or else fell through the interorganizational cracks; and rather than coordinating their activities as effectively as the components of a well-designed computer program, the various organizations with a stake in the spill and the clean-up often seemed to have more interest in blaming one another than in working with one another. Particularly during the critical, first few days, virtually the only effective response to the spill came not from any of the organizations having contingency plans, but from the fishermen of Cordova; rather than worrying about which organization or office ought to take responsibility for what part of the clean up, the fishermen simply ignored bureaucratic niceties, the fishermen simply went to work--locating, learning about, and then deploying oil "booms" to protect critical hatchery and habitat areas (see the fuller discussion in the Appendix)

As Clarke suggests, it may not have been an accident that the most effective early response to the spill came not from established organizations, but from elsewhere. Rather than indicating a "lack" of organization, Clarke suggests, the ineffective response to the spill from Exxon, Alyeska, and state and federal agencies may in fact reflect a case of "over-organization:" "One of the reasons we build organizations is to simplify decisions. It is in the nature of organizations to institute routines for handling decisions. These routines then become the templates through which organizations filter information, and hence organize action. Organizations are, in fact, organized to be inflexible. This means that organizations are organized to do some things well and other things poorly. . . . Exxon is well-prepared for Arctic exploration, oil management, and political influence. It is less well-prepared for crisis management. . . . If organizations were infinitely flexible, they would generally be ineffective in day-to-day operations" (Clarke, Appendix: 26-27).

Clarke's point is a critically important one, and readers are urged to reflect on its applicability to familiar organizational contexts. Virtually all of the persons within an organization are likely to have complained at some time of being "overworked," or of having too many demands placed upon them, relative to the resource with which they are provided. In fact, it is essentially part of the job description of an efficient manager to get the department to do more with less; if such complaints are *not* forthcoming, some observers would take this as indicating that the manager might not be pushing the department hard enough. When the available resources provide "not quite enough to go around," however, the best guess is that functions seen by the department as less central or more peripheral--such as keeping oil spill clean-up equipment at the ready, as opposed to filling the oil tankers quickly--will be among the first to slip.

Like the research scientist who "already has enough to do" when a departmental chair or a dean suggests the instigation of a new research project, the various branches of an organization are likely to feel they are already fully committed when a new challenge suddenly bursts on the scene. Firemen may (or may not) be ready for the unexpected when it occurs, patiently waiting for the opportunity to put out the next fire or emergency, but few other organizations or persons would be likely to fit such a description. When new challenges and emergencies do arise, moreover, they are likely to be viewed not just with an eye to the organization's stated goals, but also with an eye to the implications for next year's budget, the ongoing turf wars with competing or complementary organizations, and/or the question of what other person or division might be induced to take on the added work load instead.

5. *Loyalty, vulnerability and amplified risk-taking.* As Heimer (1988) has noted, a number of less notorious but often still tragic industrial accidents have occurred when workers were taking risks that everyone, at least in retrospect, would agree the workers should not have been taking. As she correctly recognizes, the recurrent nature of this phenomenon suggests the utility of asking whether some underlying factors might be at work. Drawing on the significant literature on risk perceptions, which indicates that most people are extremely reluctant to take risks in pursuit of financial gain, she suggests that there is very little reason to believe that the workers were taking such risks in hopes of increasing their salaries. Instead, she suggests, these workers were taking risks for much the same reason as many other people take risks--to avoid losses, in this case specifically including the "losses" represented by being laid off or fired (Heimer, 1988).

Note that it is decidedly *not* necessary for persons at the top of an organization to have issued orders to ignore or override safety concerns for persons lower in the organization to *behave as if* precisely such orders had been given. One of the basic assumptions of a corporate control structure following the U.S. model is that persons at the top of an organization establish the policies and those lower down have the responsibility of deciding how such policies and goals are to be brought to fruition. Excessively loyal subordinates, such as the Reagan administration's John Poindexter, may even go so far as to discern the value for their superiors of "plausible deniability"--the ability to say, if the need ever arises, that they had no awareness of the specific steps being taken to implement their overall directives (cf. Bella, 1987).

While the actions of John Poindexter and Oliver North have been the subject of considerable debate, there is little debate about the value of loyal employees in general, nor specifically about those who "know what needs to be done, and do it without asking." In many cases--for example, in the face of the deadline or an impending storm--there literally may be no time to clear one's actions with supervisors before proceeding. In such cases, the workers may reflect briefly on the reactions that their actions are likely to inspire later, during times of calmer reflection; *even if no one in the organization has ever suggested the workers should take on unnecessary risks*, however, it is easy to imagine whether the worker is more likely to fear being criticized later for erring in the direction of doing too much for the organization or not doing enough for it. As if to encourage or support such decisions by their workers, moreover, corporate leaders do in fact rarely complain about employees who do too much for the firm, while frequently voicing complaints about those who do too little.

C. The Atrophy of Attentiveness.

At a still broader level of abstraction, there is a need to consider what would be expected to happen to an organizational commitment to risk management over time, particularly in the case of rare or "unexpected" problems. Even in an organization with an above-average commitment to safety, and one where managers seek not to put pressure on workers to cut corners, it appears that the normal expectation will be for attentiveness to atrophy over time. There are at least two key mechanisms behind this expectation--the predictability of complacency and the reality of cost control concerns.

1. Complacency and boredom. At least since the time when an iceberg got the best of the "unsinkable" *Titanic*, most ships' crews have presumably been operating at an increased level of caution and alertness for at least their first trips through iceberg-infested waters; adrenaline, however, does not keep on pumping forever. While the ships coming in and out of the Alyeska pipeline terminal in Valdez admittedly had not been totally immune from problems, the *general* pattern of experiences up through 11:59 p.m. on March 23, 1989, was scarcely of the sort that would have raised concerns about catastrophic failure for most observers. Over 8,000 tankers had gone in and out of the port, over a period of more than a decade, without a single catastrophic failure. Based on the empirical track record up to that point, most observers presumably would have seen little reason to expect reasons for any particular concern.

Neither, unfortunately, did the crew of the *Exxon Valdez*. Five minutes later, however, the incredibly sophisticated tanker had an incredibly stupid encounter. Despite an array of navigational devices having a level of sophistication that early sailors could scarcely have imagined, the ship managed to hit an obstacle that was literally miles out of its course--one that had been known by even the earliest sailors in the area, who had named it after the same Captain Bligh who was later to achieve a different kind of notoriety as the victim of a mutiny on the HMS *Bounty*. The accident, coincidentally, took place not just on a Good Friday that happened to be the 25th anniversary of the 1964 earthquake that destroyed the former city of Valdez, but also during the 200th anniversary year of the mutiny.

In many ways, the eras before and after the stroke of midnight on Good Friday, 1989--years of reasonably successful operation, followed by the largest oil spill in the history of United States--could scarcely seem more disparate. In another sense, however, perhaps they could not be more closely related. It is entirely possible that the accident of Good Friday, 1989, could not have occurred but for the tragic complacency engendered by the dozen good years that had passed before. More specifically, it may have been the very "success" of earlier trips in and out of Prince William Sound--literally thousands of them--that helped to make possible a situation in which the captain had retired to his quarters, the ship was under the control of a third mate who clearly would not have been expected by most risk assessors to have been at the helm, the Coast Guard personnel on duty were not bothering to monitor even the lower-power radar screens that remained at their disposal after cost-cutting efforts a few years earlier--and 11 million gallons of crude oil fouled nearly 1000 miles of once-pristine shoreline.

2. *"Non-productive" status of safety measures.* Virtually all institutions, public or private, are likely to face periodic pressures to control costs. The sources of pressure may include competition, a desire to increase profits, responses to cost overruns, political or other pressures to "cut down on waste and inefficiency," or simply a desire to do more with less. Whatever the original source of the pressure or the nature of the organization, at least one of the responses is likely to be consistent: Within the limits of means/ends displacement discussed above, organizations will generally seek to protect what they consider to be their core functions and to cut back on those they consider peripheral.

There is a tremendous range of variation across organizations in what the "core" functions are considered to be--from building cars to busting criminals--but there is virtually no organization for which increasing the safety of its *own* operations is the primary or central goal. Instead, protection of health, safety and the environment tend to be secondary or "while" concerns: Organizations seek to produce energy "while" protecting the environment, operate submarines while providing an adequate level of protection for the crew, dispose of wastes "in an environmentally acceptable manner," and so forth. Almost never is risk management included in the first half of the sentence, at least in the description of overall organizational goals, as in "increasing the level of safety for workers and nearby communities 'while' maintaining adequate profit margins"--unless it is when risk management professionals use such terminology to describe their activities to other persons in their organizations.

The consequences of occupying organizationally peripheral positions, unfortunately, also show up in ways that are not just linguistic. To return to the *Exxon Valdez*, a series of reports in major news outlets (e.g., Bartimus et al. 1989; Church, 1989) reveal what can happen. From one report:

> Dozens of interviews with former officials and safety officers, along with a study of state records and original contingency proposals, indicate that a plan to avert a tanker disaster was developed a decade ago and then gradually dismantled piece by piece. . . . Factors include
>
> *rejection by the Coast Guard and Alyeska Pipeline Service Company. . . of a 1976 state study that forecast tanker accidents and urged such requirements as double-hulled tankers and tug boat escort beyond Bligh Reef. . . .
>
> *two cutbacks in recent years that probably removed an extra pair of eyes that might have spotted the off-course Valdez. In 1978, the Coast Guard reduced the distance that local pilots had to guide departing tankers and, since 1984, the Coast Guard has cut its radar staff in Valdez to 36 from 60, reduced the radar wattage and decreased the distance required for radar monitoring. . . .
>
> *Disbandment in 1982 of the Emergency Response Team for Alyeska. . . . Spill-fighting equipment on hand was below the minimum required; even the barge designated to carry containment booms and supplies was in dry dock. . . .
>
> *Carelessness by the state agency charged with keeping Alyeska in compliance. The crash in oil prices in 1986 forced state budget cuts that reduced the work week at the Department of Environmental Conservation to 4 days (Bartimus, 1989:1, 15).

While this is only an example, it does illustrate several points. First, neither a straightforward reliance on "private enterprise" (Exxon) nor on "public servants" (state, federal agencies) would appear to offer much reason for comfort. Both private- and public-sector actors cut "unnecessary" costs for risk-management activities that might have helped to avert the disaster. Alyeska Pipeline Service Company, the consortium of the oil companies that runs the pipeline, might be expected by the uninitiated to provide a somewhat higher level of commitment to risk management, in that its employees would be administratively removed at least to some degree from the "pressures for profit" that would be more likely to characterize the oil companies themselves; if anything, however, Alyeska has come in for harsher criticism than Exxon itself. This may not be entirely accidental; in the words of one resident of southeast Alaska (a critic of both Exxon and Alyeska), "Alyeska isn't the place where any of the oil companies send their best people.

You're never going to become the president of your company by way of working for Alyeska." It may also be worth noting in this context that corporate presidents often come from sales, production or even legal or accounting branches of a firm, but rarely if ever from the in-house office of risk management.

D. Deliberate Biasing Pressures.

At the risk of stressing the obvious, literally all of the factors identified in this report thus far are of the sort that would be expected to lead to the underestimation of "real risks" *even by scientists who are well-meaning, honest, and not aware of any pressures* to produce (deliberately) biased results. Those of us who work for "establishment" organizations often criticize the objectivity of critics who work for environmental or "public interest" groups, arguing that the political positions and interests of such groups may have influenced the findings and the arguments of the scientists they employ, but if there is a potential for biasing pressures in one direction, there may be similar pressures in the other direction, as well. Given the above-noted tendency for scientists to work for industrial interests far more often than to work for opponent groups, moreover, the "visible" pressures toward bias may in fact be likely to make the problem worse (Dietz et al., forthcoming; see also Schnaiberg, 1980; McConnell, 1970; Kloppenburg, 1988).

At the risk of offering another observation about which "everybody knows" already, it is unfortunate but true that the real world is often not so free from "unscientific" pressures on scientists as we might like. Perrow (1986) provides a relatively critical examination of cases where industrial representatives have understated known risks (see also Clarke, 1988a; 1988b; Stoffle et al., 1988). The publications put out by environmental groups often focus heavily on cases in which industrial or governmental representatives have lied about or covered up credible evidence about risks to the public health and safety; while such organizations would presumably have an interest in encouraging a high or even exaggerated sense of the likelihood or frequency of such incidents, recent examples of "unfortunate" organizational behaviors originally brought to broader attention through the efforts of such activist groups include the Federal Bureau of Investigation (FBI) probe of the Department of Energy's facility at Rocky Flats, Colorado, resulting eventually in a raid on the facility by federal enforcement agents and the filing of criminal charges (discussions of such problems in the risk assessment literature are understandably rare, but see Sterling and Arundel, 1985).

CONCLUSION

The point of this report, of course, is not to dwell on such clear-cut cases of unscientific behavior by scientists, regrettable though each of them may be. Instead, the purpose has been to examine whether or not it would be prudent for the citizens and the state of Nevada simply to assume that even "official" DOE risk assessments, such as the transportation risk assessment in the 1986 Yucca Mountain Environmental Assessment, or future estimates of the risks of nuclear waste transportation, can safely be assumed to be reasonable approximations of the "real" risks. On the basis of the accumulated evidence, the answer to that question appears to be negative. The most reasonable conclusion that can be drawn from the existing professional literature is that official Department of Energy risk assessments, like many of the other assessments that have been done in the past, will systematically overlook both the factors likely to lead to the *attenuation of risk estimates* and the factors likely to lead to the *amplification of risks themselves*. It is critically important, therefore, that the state of Nevada devote resouces to independent assessments of both sets of factors in its own transportation risk studies in the future.

It needs to be noted that this entire report has been written from the perspective of a loyal member of the risk assessment community, one who of course wishes to minimize the likelihood of such unprofessional behaviors by scientists but who believes, on the basis of first-hand observations, that the vast majority of scientists are indeed careful, honest and scrupulous, often to a fault. The problem, in short, is not that the scientists involved in risk assessment are bad or biased people; in general, they clearly are not.

Unfortunately, the problems identified in this report are in some ways more serious, and more difficult to counteract, than if deliberate or even conscious sources of bias were in fact involved. Instead, the problem is that a variety of factors that are far more subtle--unseen, unfelt, and yet unfortunate in their consequences--exert an influence that could scarcely be more disturbing even if cases of deliberate malice were involved. Systematically and repeatedly, the factors operating on risk assessors that have been identified in this report serve both to *attenuate* the *estimates* of risks and to *amplify* the risks themselves. Given the state's need not only to have the most accurate possible information for its decision-making, but also to protect the health and welfare of its citizens, such a systematic set of biasing factors is one that the state cannot afford to ignore.

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APPENDIX:

ORGANIZATIONAL FORESIGHT AND THE EXXON OIL SPILL

A Report From

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The research on the Exxon spill was funded by grants from the Hand Foundation, the Natural Hazards Center at the University of Colorado, and Rutgers University. This report has been improved by several helpful suggestions from William Freudenburg, Robert Halstead, Patricia Roos and Michal Tamuz, but the views expressed in this paper are strictly those of the author. Nothing herein should be taken as representing the Hand Foundation, the Natural Hazards Center, Rutgers University, the Nuclear Waste Transportation Research Center, the University of Nevada, Las Vegas, the Nuclear Waste Projects Office, or the U.S. Department of Energy.

ORGANIZATIONAL FORESIGHT AND THE EXXON OIL SPILL

The problem of organizational effectiveness in mitigating hazards is increasingly vexing, and indicates a growing dependence on organizations as agents of rescue. Now, more than ever before, organizational anticipation of mishaps and misfortunes shapes both the likelihood of untoward events and productive responses to them. This is especially true for accidents and unforseen conditions regarding technological risks. Organizations and modern technology provide much of what we value and cherish, but they are simultaneously responsible for terrible hazards. It is little solace that we simultaneously depend on organizations for protection. Hence any assessment of important risks that ignores organizational processes, and the social systems in which those organizations are set, will be seriously flawed.

In this paper I use evidence from the *Exxon-Valdez* oil spill to address one such organizational process--that of foresight. I intend this term to include risk assessment, but also to refer more generally to the ability of organizations to predict the future. All predictions of the future, whether made by organizations or individuals, are inexorably tied to the past and present. Except in dreams and mad minds, expectations about the future are extrapolations from the present, which itself reflects the past.

The ideas developed here are rooted in research on organizational effectiveness (Yuchtman and Seashore 1967; Cunningham 1977; Goodman and Pennings 1977; Weick 1977; Connolly, Conlon, and Deutsch 1980). While traditional approaches to effectiveness failed to account adequately for how different interests shaped organizational goal setting and attainment, modern scholarship explicitly recognizes the importance of asking "Effective for what, or whom?" (Perrow 1977; Mohr 1982). Although posing the question in this manner is more useful than theorizing effectiveness as disembodied from concrete actors, it nevertheless ignores the element of time. That is to say, the issue is not only effective for what or whom, but also when (March 1981). Put more concretely, a legitimate question to ask is "How far into the future must an organization competently plan for to warrant the label 'effective'?" Although I cannot answer this question here, simply posing it suggests the pivotal nature of time horizons in organizational foresight and planning.

This paper is part of the effort of FREUDENBURG AND ASSOCIATES to assist the state of Nevada in addressing problems associated with nuclear waste transport and disposal. First I review the Exxon spill and some of the organizational responses to it. Next I use the Alaska case to consider three main barriers to effective organizational foresight--professional heuristics, over-organization, and spurious consensus. This list is in order of aggregation or generality. Professional heuristics refer to important mechanisms of decision making on the parts of experts and organizational directors. Over-organization refers to organizational structure *sui generis*. Spurious consensus refers to the premature or erroneous imputation of agreement on risk assessment and acceptance. Together, these three barriers can seriously distort or hamper useful organizational foresight. In the final section I consider the relevance of these barriers to the issue of issue of radioactive waste transport.

OVERVIEW OF THE EXXON SPILL

A little after midnight on 24 March 1989 the *Exxon-Valdez*, a supertanker carrying over 50 million gallons of North Slope crude oil, grounded on Bligh Reef off the southern shores of Alaska in Prince William Sound. Before the *Valdez* stopped bleeding, nearly 11 million gallons of oil contaminated the waters and shoreline of Prince William Sound and the Gulf of Alaska. By the end of the first week, oil covered

900 square miles of water; by the end of the second, almost 1,100 miles of shoreline had been oiled. The spill was responsible for considerable loss of wildlife. Official figures of wildlife deaths are vast underestimates, probably missing their mark by 70-90 percent, because most oiled animals sink in the cold Alaskan waters, while those on land become carrion. Further, a large part of Alaska's 1989 salmon harvest was ravaged, and indeed had it not been for the spill, 1989 would have been an extraordinary season. The spill was even worse for the two native communities whose land was polluted; the oil prevented (at least for the first season after the spill, and probably for the subsequent two or three) subsistence fishing and hunting, the cornerstones of those communities' livelihoods and cultures. The Exxon spill was also an aesthetic disaster, spoiling a very large amount of shoreline for an undeterminable number of years. Although some, especially in Exxon, expect the worst of the damage will disappear with the 1989-1990 winter storms, oiled beaches and waters from the spill are likely to remain so for years.

The immediate response to the spill has been a focal point of controversy. Before exploring that controversy for instructive lessons, we must first detail what the responses were. Within hours of the grounding, officers from the Alaska Department of Environmental Conservation (ADEC) were aboard the stricken vessel. To the extent possible, ADEC officials surveyed the damage and began the process of inciting numerous organizations to action. It was a dark night, so officials could not really tell the extent of damage to the ship, but they knew the spill would be large. "The oil was three or four feet deep *on top of the water*," an official told me, "You could have just put a hose in the stuff and sucked it up."[1]

[1] I spent 10 days in Alaska during July 1989 conducting interviews with key informants, collecting documents, and observing. I conducted additional telephone interviews before and after the research trip. Any quotations, unless otherwise noted, are from my interviews.

One of the first ADEC officials to board the *Valdez* used the ship's radio-telephone to wake the president of Alyeska Pipeline Service Company. Alyeska is the incorporated organization representing the seven oil companies that own and operate the Alaska pipeline.[2] The ADEC official told Alyeska's president that the spill was "a bad one" and that airplanes with oil dispersants should be readied immediately.[3] The tone of the ADEC official's call was especially urgent, because some of the airplanes and dispersants were in places such as Arizona.

Notifying Alyeska seemed an appropriate reaction, and is indeed the prescribed action, because Alyeska bears the greatest organizational and legal responsibility for immediate oil spill response. This response, or set of responses, is detailed in oil spill contingency plans, considered below. Alyeska's contingency plan (c-plan) called for two key organizational reactions: booming the leaking oil to prevent its dispersion and deploying aircraft with dispersants to dissipate the slick. But there were problems with the plan and its implementation. Alyeska's president allegedly thought the alarm from ADEC too unbelievable and went back to sleep. Alyeska's safety barge, the only available mechanism to carry boom to the *Valdez*, was scheduled to be replaced and its 7,000 feet of boom were unloaded at the time. The reloading operation delayed the barge reaching the grounded vessel by 10 hours. By then, booming the spill was as futile as trying to gather the radioactive cloud from Chernobyl.

It should be noted that some believe the spill could have been effectively boomed during the first two days when the waters were "as calm as glass." That the water was indeed that calm was an indication of the generally excellent weather in the Sound. The

[2]These include Exxon, ARCO, British Petroleum, Mobil, Phillips, Hess, Unocal.

[3]Dispersants break oil into fine particles which are then more likely to dissipate.

problem, according to this argument, was that no organization was willing to move quickly enough to contain the spill. Alyeska's full emergency crew, for example, did not arrive at the spill site until at least 14 hours after the grounding, and the *Exxon Valdez* was not boomed for another 21 hours. Moveover, from this view, the organizational chaos then prevailing prevented prompt use of dispersants, which many claim are the single most important tool in mitigating adverse consequences from a spill. The problem, apparently, was that no one was willing to make important decisions on their own initiative. As one official close to the case put it, "It was as if a pilot in a 737 lost one of his engines and got on the radio to the CEO of the airline to ask what to do."

It is doubtful, however, that even large amounts of organizational will would have constituted an effective response. For even if organizations had all the time in the world, there were only 4,000 gallons of dispersant on hand the day of the accident; to respond effectively would have been required nearly 500,000 gallons. More importantly, there is good reason to be skeptical that any amount of boom would have been able to contain the oil, as the expanding slick was simply too large a task for even state-of-the-art technology. In any case, by the third day of what was quickly becoming a political, environmental, and organizational crisis, 70 mile per hour winds were whipping through Prince William Sound, making it impossible for either aircraft or watercraft to attend the *Exxon Valdez* safely. By the time the storms came up, it was too dangerous, and too late, to apply dispersants to the oil.

There is much to be learned from detailing the problems with initial organizational responses (or lack thereof), and indeed many argue that the problem in Prince William Sound was ineffective implementation of a basically sound strategy. While there is some evidence to support this view, here I will argue that, (1) the c-plans themselves were fundamentally flawed, (2) because they were completely inappropriate for a large oil spill, (3) although all parties, including the regulators, had approved the c-plans as acceptable representations of the risk of, and response to, a catastrophic spill.

The Contingency Plans

There were five, arguably six, contingency plans available at the time of the accident, each with its own ostensible contribution to mitigating the worst consequences of a major oil spill. These included:

- The National Oil and Hazardous Substances Pollution Contingency Plan,
- The Coast Guard's Captain of the Port Prince William Sound Pollution Action Plan,
- The Alaska Regional Oil and Hazardous Substances Pollution Contingency Plan,
- The State of Alaska's Oil and Hazardous Substances Pollution Contingency Plan,
- The Alyeska Pipeline Service Company's Oil Spill Contingency Plan for Prince William Sound.

(The sixth, some might argue, was Exxon's contingency plan. I ignore this plan here, as it pertains mainly to cleanup rather than the immediate response to a spill.) Each c-plan delineates different functions and procedures to be performed by myriad organizations in the event of a minor, moderate, or major oil spill. These plans have been developed over a number of years, some dating back to the beginning of the trans-Alaska pipeline in the early 1970s. Each plan is very detailed, and some contain explicit scenarios of major groundings and founderings of large tankers. These plans also attempt to map out an appropriate cleanup response from many agencies from all levels of government. One of their chief functions is to serve as blueprints for interagency coordination. Such planning seems to be quite sensible, and indeed parts of the plans are fine models of anticipatory hazard mitigation. Yet the c-plans, taken together, were clearly inadequate to respond to a major spill effectively.

I will not discuss all the c-plans but rather will focus on one, Alyeska's Contingency Plan for Prince William Sound (Alyeska Pipeline Service Company 1987, hereafter Alyeska 1987). This plan was most applicable to the Exxon spill because it was, and is, the one that most clearly addresses the problem of spills in Prince William Sound. It states, in no uncertain terms, Alyeska's commitment to assume responsibility for immediate response to a spill in the Sound. Alyeska's c-plan is a vague, general document of 248 pages, perhaps two-thirds of which consists of maps and lists of equipment. Maps and lists are, of course, necessary for responding to a spill, but what is striking about Alyeska's c-plan is the sparsity of detail regarding projected spill scenarios. The lack of significant detail is odd because it is precisely in such detail that organizational assumptions about substantive mitigation systems are embedded. To consider other industries for a moment, when nuclear power plant operators train in simulators they are engaging in active contingency planning. The same is true of airline pilots, workers in chemical plants, and high schools in earthquake-prone California (although such planning is absent in earthquake-prone Tennessee). It should be noted that sparsity of planning detail (and real-time drills) is not uncommon in emergency plans for transporting radioactive materials, perhaps reflecting weak regulatory guidelines.

One of the distinctive facts about the Exxon spill was *not* in fact a lack of available structure that would coordinate strategy. As noted above, there was a wealth of contingency plans in place at the time of the spill. These plans included (and include) a set of directives for arranging more than a dozen organizations. For example, the National and Regional plans (mentioned above) prescribe the activation of an interagency task force called the Regional Response Team, which includes all the major actors with interests in a spill. It is numbing to list all that is supposed to happen once the Regional Response Team is activated; it will suffice to note that there are elaborate plans and designs for organizational action.

These observations suggest the thesis that there was *too much* organization after the Exxon spill, not too little. A strong form of this argument would be an overstatement, but to state the thesis strongly highlights some ignored but important aspects of the case. There are two senses in which over-organization was a problem. First, the disqualification heuristic, discussed in the last section, was not only a set of ideas adopted and advanced by professionals. In fact this heuristic became institutionalized in organizational structure. The notion that "it can't happen to here" was more than a paradigm that ordered realities of risk, it also became standard operating procedure to presume the risk innocent until proven dangerous. While some have argued these are signs of massive stupidity, such a naming of the problem merely begs the important issues.

One of the major reasons for formal organization is to simplify decisions, and it is in the nature of organizations to institute routines for handling decisions (March and Simon 1958; Simon 1976). These routines then become the templates through which organizations filter information, and hence organize action. Organizations are, in fact, organized to be inflexible. In other words, organizations are organized to do some things well and other things poorly. My university, for example, services 35,000 undergraduates every year. Add to them graduate students, faculty, staff, and administrators and one has a sizable organizational problem. Just to arrange parking for this many people requires considerable organizing effort, great attention, and resources. And yet most of us find parking spaces, and the system works fairly well.[4] On the other hand, trying to secure a refund for any type of over-payment entails more effort than the refund is worth.

To return to the problem at hand, Exxon (and Alyeska) is well-prepared for Arctic exploration, oil management, and political influence. It is less well-prepared for crisis management. Or, considering all the organizations involved in Alaska, the risks of tanker failure are insufficiently covered by any organization or set of organizations. If organizations were infinitely flexible they would generally be ineffective in day-to-day operations. Further, even infinite flexibility would not guarantee effective response to off-standard demands like massive oil spills.

The barrier of over-organization played a vital role in inhibiting organizational foresight in Alaska. Perhaps a dramatic example will help make the point. There are a number of very sensitive, and very important, salmon hatcheries in Prince William Sound. Many of these hatcheries were directly in the path of the oil slick. One of the most important salmon hatcheries is called Sawmill Bay. This is one of the bays Alyeska's contingency plan says will be boomed within six hours of a 4,000 gallon spill. Unfortunately, for the first several days after the accident, no major organization moved to boom Sawmill Bay, or any other hatchery for that matter. The fishers of Cordova, along with other fishing villages, asked first Exxon, then ADEC to boom Sawmill Bay, to no effect. In frustration, the fishers banded together, pooled their monies and purchased boom from as far away as Japan and Europe, saving the Bay from oil .

[4]Undergraduates would disagree sharply with this analysis. One never escapes distributional issues when organizations are involved.

contamination. Officials responsible for mitigating the effects of oil spills were apparently immobilized by their own organizations. The fishers, unencumbered by organization, were able to exploit their resources to win what is referred to locally as the Battle of Sawmill Bay.

It bears pointing our that, from the point of view of any specific tanker trip, the Exxon spill was a low-probability event. Indeed the probability of any *specific* spill remains low. But from the point of view of exploring oil in Alaska, the probability of oil spills must be considered fairly high (although I would not venture a precise prediction). The point here is not simply that reality and the future are complex, although that is true enough. Rather the point is that in the main organizations do routine things well, and since catastrophic failures are (fortunately) uncommon, we should be surprised when organizations *do* respond well to problems like the grounding of the *Exxon Valdez*.

Spurious Consensus

Perhaps the most politically intractable problem in issues of risk estimation and acceptability is that of social consensus. There is now a great deal of research on the problem of consensus in technical disputes (e.g., MacRae 1976). Here I shall follow the bulk of that work by discussing the issue of public consensus. It is important to note, however, that equally central topics of research involve consensus among organizations, or among organizations and groups. For present purposes, the important points about consensus among experts were noted above in the section on professional heuristics.

Consensus is not a static property of situations, but is instead fundamentally a product of *interaction* among groups, organizations, and professions. Hence consensus is ineluctably tied to agreement, persuasion, and coercion. Further, several basic asymmetries usually characterize instances in which dissension prevails, but consensus must be negotiated. Although most work on consensus concerns the general public, it is in fact organizations and their elites that are the major players in creating and responding to danger. Organizations, not the public, are also the major actors in setting the terms of debate over risk acceptability.[5] The mechanisms available to organizations for fostering consensus on their own terms are inherently more powerful than those available to disparate members of the general public. The general public is not powerless, of course, once it organizes, secures recruits, and gains media attention (Clarke in press; Mazur 1988). Once such a process occurs, however, the sociological relevance of a *public* consensus becomes less relevant than the capabilities of social movements.

For organizations charged with ensuring or enhancing safety, the stability of consensus is very sensitive to the initial conditions of its formation. In particular, the more organizations determine the early conditions of consensus, the higher the intensity of future public opposition.[6] I do not propose this principle in a mechanical way, but rather present is an hypothesis. For it is certainly not the case that, as with a strong house, if the infrastructure of consensus is carefully constructed then later misfortunes will not befall it. However if such an infrastructure is poorly constructed, later opposition will be stronger and more acrimonious than would otherwise be the case.

This suggests the problem of spurious consensus in technical disputes. A spurious consensus is when organizations and decision makers assume that other,

[5]For related issues see Offe and Wiesenthal (1980).

[6]In truth, there is a probably a curvilinear relationship between the degree of such organizational determination and intensity of public opposition. Here we will be concerned only with one side of the curve.

particularly non-organizational, players (e.g., the public) share their estimation and acceptance of risk. A spurious consensus is thus an elemental barrier to effective organizational foresight. For when crises in legitimacy arise, organizations (from their view at least) have failed to foresee a set of problems that hinder operations. There are no mechanisms guaranteed to produce genuine consensus, but we can analyze some instances of spurious consensus and then try to derive some lessons from them.

In Alaska, those involved in controlling and developing oil operated under several assumptions from the early 1970s, some of which are now being questioned as if they had never been publicly considered before. And for good reason: they have not. The assumption that there was no choice but to develop the oil was not so far off the mark as to compromise later legitimacy. There was enough poverty in Alaska, and enough lasting public consensus, to move ahead with exploration and development. This part of the consensus regarding oil in Alaska is still generally in place, and reinforced by a resource constraint: 85% of the state government's budget derives from oil.

But organizations and decision makers in Alaska erroneously made several other assumptions. These assumptions formed the basis of projected probabilities of failure, and as such seriously distorted, albeit unwittingly, organizational foresight. These assumptions include the following:

- Given the enormous financial gain (for either corporations or governments), any risks associated with oil development were acceptable,
- Estimating the risks of the pipeline exhausted the range of legitimate risks that needed to be considered, and
- Responsibility for responding to water-borne oil spills was clearly assigned and well-understood.

Several general mechanisms contributed to the creation of a spurious consensus in Alaska, and hence seriously impaired organizational foresight. One such mechanism is the tendency of organizations, especially those involved with technological risks, to continually claim control over what may in fact be uncontrollable. Many of our most politically charged risks--nuclear meltdowns, toxics leaching into water tables, ozone destruction, large oil spills--seem to be ones that defy control. Yet our organizations are the most controlled and controlling devices we know, and publics are baffled and angered when organizations seem incapable of adequate response. The problem is that organizations rarely admit their failings (though they are generally ready to point out the failings of others). When the often substantial gaps between what organizations *say they can do* and what they *can actually do* become public, institutional legitimacy is threatened and the probability of popular distrust increases.

By pretending to more control and understanding than they actually possess, organizations increase the probability of engendering public distrust. This distrust is exacerbated when organizations fail to acknowledge the ambiguity inherent in big decisions about risk, as they almost invariably do. In Alaska, there was never a point when the risk of tanker spills was seriously, publicly debated. Rather, attention was directed almost solely upon the vast benefits of oil development: the enhanced tax base, more jobs, and so on. One result of constructing public consensus around oil in this manner was that organizations assumed that lack of significant public opposition indicated a wide-spread consensus regarding possible environmental costs of oil exploration. That assumption has cost industry (and state government, although to a lesser extent) dearly in the wake of the Exxon spill.

CONCLUSION:
ORGANIZATIONAL FORESIGHT AND NUCLEAR WASTE TRANSPORT

Many moments in making decisions about the future are vulnerable to distortion and mistake. These moments are central components in any organizational system: information processing, organizational perception, choice of technologies, program implementation, and perhaps most difficult, anticipating the reactions of outsiders to organizational decisions. Failure of any of these components can cascade into organizational failure more generally. Here I have folded these moments into a single category that constitutes a particular type of organizational failure--one of foresight. I have not focused on failures of foresight (as opposed to successes) because I think organizations are inherently worse predictors of the future than other social actors (e.g., individuals, nations). To the contrary, organizations are generally *better* predictors of the future than most other actors. My focus has been on failure for the simple reason that we usually learn more from critical examination than praise.

By way of conclusion, I shall reiterate the key points of my argument and briefly discuss the argument's implications for nuclear waste transport. I have argued that organizational foresight of oil spill risks in Alaska was attenuated by several barriers. It is important to remember that, in Alaska, these barriers to organizational foresight operated in a situation that was well-suited to overcome them. As noted, much is known of the threats posed Alaska's environment; much is also known of what can go wrong with shipping systems. Nevertheless, the institutional system charged with managing and responding to oil spill risks in Alaska failed to anticipate both the likelihood and the consequences of a large oil spill. It is, therefore, particularly appropriate to ask the question: How well can we expect organizations to anticipate nuclear waste transport problems--at least some of which are likely to remain unclear for many years into the future--given how well, in Alaska--with much less uncertainty regarding key variables--they predicted major oil spills?

Before addressing this question, it is worth noting that we lack the comparative data needed to answer it with certainty. To be sure, most books on civilian nuclear power consider the risks of the nuclear fuel cycle in general and usually mention the specific problem of moving highly hazardous materials from reactors (for example) to their final (or semi-final) resting place. But thorough treatments of transport problems are rare, and indeed most attention regarding hazards management is directed to either the generation of radioactive material or its disposal (e.g., Lipschutz 1980). We can, however, draw upon other aspects of the nuclear industry to make some comparisons.

The Barrier of Professional Heuristics. The history of nuclear power in the United States has been significantly shaped by the disqualification heuristic. One result has been to foster the conviction, within the industry, that technical solutions can be found to social and political problems (Woodhouse 1983:172). Further, partly because civilian nuclear power was long geared to military purposes (Clarke 1985), through the years the industry tended to neglect possibilities of catastrophe. At least until recent years, this (sometimes deliberate) neglect thrived on a lack of outside scrutiny. In any case, the disqualification heuristic helped hinder debate over alternative reactor designs, safety equipment, and disposal technology by disqualifying contrary perspectives. For example, at the 1977 licensing hearings on Three Mile Island, environmental groups critical of the plant were allowed only one expert witness, while 55 experts were allowed to testify in support of it (Woodhouse 1983:160). A professional, disqualification heuristic places inordinate value on the opinions of experts and technical advances while tending to disregard critical data and viewpoints. It was the paucity of early debate that would later draw charges of mis- and malfeasance toward those claiming to protest public interest.

The implication of this analysis for nuclear waste transport is that there should be extensive, explicit consideration of realistic worst-case scenarios. Although such analyses do not guarantee that all contingencies will be considered adequately, they at least create the opportunity of calling into question assumptions that any risks (or consequences) of transport failures will be minimal. Further, decision makers should take explicit account of real-life events in which failures of organizational foresight have attenuated satisfactory anticipation of future risks (and consequences).[7] Such exercises force a direct confrontation of realities that seem unlikely or even absurd in imagined scenarios (who would have believed a huge ship on autopilot would be allowed to fly onto a well-known, huge underground rock? who would have believed it was standard operating procedure to test for air leaks next to a nuclear reactor core with lit candles, as was the case at Brown's Ferry). Worst-case thinking is not alarmist or unreasonable. It is, to the contrary, essential to reasoned estimation of probable organizational failures.

The Barrier of Over-Organization. Organizations usually institute new procedures when responding to new problems; such procedures are based on past experience. Put differently, when organizations prepare for uncertainty they draw on routines constructed for recurrent problems. This observation suggests why preparation for rare, catastrophic events is inherently vexing for organizations. It is difficult indeed to develop routines for responding to nuclear transport accidents or major oil spills primarily because such events do not happen frequently enough to permit accumulation of knowledge regarding them. Thus does a structurally induced ignorance prevent

[7]This paper is one such exercise.

extensive preparation for untoward events. Problems of uncertainty are exacerbated when organizational jurisdictions overlap. For example, in 1984 a truck loaded with U.S. Navy torpedoes spilled its cargo near Denver. Although no one was killed, "the accident happened at the intersection of several freeways during rush hour," and nine hours passed before the city of Denver could find any information on the proper way to respond (Peterson et al., 1987:63). Part of the difficulty in than incident was a lack of clearly delimited jurisdictions that would have created a division of labor among organizations so that an effective response could be executed. That many technological risks, such as those associated with nuclear waste (Gould 1983), are chronic threats further hinders developing continuing, effective response.

Not only change, but also time, conspire against organizational readiness, as time horizons are frequently unclear for organizations. When the constraints posed by far-away time horizons are given scant attention, the stage is set for future failure and hence, future criticism of organizational programs and decisions. In shortening the time horizons defined as within their legitimate purview, organizations tend to concentrate on short-run benefits, even though the long-run costs may be very high indeed. An apt example is the case of Nuclear Fuel Services in West Valley, New York. That case involved the construction and operation of a nuclear waste reprocessing plant, with all organizations involved failing to accord sufficient attention to time horizons. The plant is now abandoned (at least for reprocessing), although the problems created by its construction will remain for some time.

Problems of uncertainty and time are closely tied to the barrier of over-organization. I have argued that organizations are by their very nature somewhat inflexible. Indeed their particular strengths are found precisely in the arrangement of organizational resources to do some things well, even though such arrangements damn them to do other things poorly. Were the organizational world constructed otherwise, the most frequent call would be for more bureaucracy.

At the same time, there is a special onus on organizations charged with planning for and responding to serious risks, like those that attend nuclear waste, and oil, transport. In effect, we expect organizations to go against their nature when we ask them to be flexible enough to be able to respond to events whose sequences and timing cannot be known, and whose frequency cannot, even in principle, be estimated. Nevertheless, there are several implications for nuclear waste transport.

Precisely because it is possible to know the limitations inherent in formal organization (i.e., over-organization, short time horizons, erratic or unreliable responses to uncertainty), it is possible to construct mechanisms that help counter those limitations (although it is not likely they can actually be overcome). Since organizational solutions are tightly bound to (though not, I should emphasize, completely determined by) past practices and problems, decision makers should seek out and develop networks of advice and criticism that do not have an immediate or direct relationship with their organizations. This would be a difficult recommendation to institute, as it means deliberately fostering scrutiny of organizational missions by parties who may have an interest in seeing that mission founder. On the other hand, this risk is no more perilous than the technical problems with which these organizations must deal. Hopefully, such outsiders would have some special facility to ignore immediate constraints to stretch the time horizons considered in the process of making difficult decisions.

The Barrier of Spurious Consensus. The history of nuclear waste siting provides instructive lessons for the consequences of a possible spurious consensus regarding nuclear waste transport. The nuclear industry today is beleaguered in large part because of a lack of widespread consensus regarding the risks of nuclear technology. One key area of dissension concerns disposal of radioactive waste, especially spent fuel rods and other contaminated materials from civilian reactors. The United States now faces a $130 billion dollar nuclear waste problem (Lemons et al., 1989).[8]

This is one of the areas in which the public probably knows little about the technical aspects of the risk, although most people are able to voice an opinion on it. Reviewing a plethora of studies on public attitudes toward nuclear waste, Nealey and Hebert (1983:97) note that although nuclear waste is an "obscure topic for a significant segment of the public," strong opinions on its risks began to develop after a 115,000 gallon leak of highly radioactive liquids at the Hanford Reservation in Washington. "A substantial part of the public," they write, "doubts that adequate waste disposal techniques are now known, and a growing number mention radioactive waste problems as a reason for halting further nuclear development" (Nealey and Hebert 1983:97). In fact one of the increasingly vehement critiques leveled against the nuclear industry over the past 15 years is that the industry (including regulators) has failed to adequately estimate problems associated with waste siting (Nealey and Hebert 1983). Part of the reason for this vehemence is that the industry and regulators have assumed their view on solutions to the waste problem was consonant with that of a wide variety of other actors. A spurious consensus on the risks of nuclear waste transport (and siting) could cripple future efforts to develop disposal technologies.

In addition to considerable dissension and distrust from below (i.e., on the public's part), there is mounting dissension from above (from experts, organizations, and

[8]This is probably a conservative estimate.

governments--particularly the state of Nevada) concerning problems of nuclear waste disposal and transport. Indeed it seems highly likely that the consensus the Department of Energy (DOE) has been trying to construct will almost certainly end up being a spurious one. DOE's recent announcement that the planning process in Nevada will be begin anew reflects the precarious foundations upon which its attempted consensus was constructed (Wald 1989).

Congressional legislation, although designed to assuage the concerns of states and citizens who might be the recipients of a nuclear waste depository, set the stage for the lack of technical and political consensus among experts and organizations regarding depository siting, and also, by extension, nuclear waste transport. In 1982 Congress passed the Nuclear Waste Policy Act (NWPA).[9] The mechanism in NWPA that would create the hoped-for consensus was the requirement that DOE seriously consider several potential sites, with the final result being one repository in the east and one in the west. NWPA further required DOE to negotiate directly with the states and Native American tribes who had direct interests in the outcome of the decision process. Although these stipulations ensured delay of decision and political disagreement, they were steps in the direction of creating a genuine consensus. Unfortunately, the NWPA also allowed DOE to sidestep the National Environmental Policy Act's requirement of an Environmental Impact Statement for such decisions. As might be expected, the Department of Energy has stated that any nuclear waste site will be in full accordance with NEPA, although it assiduously avoids providing the kind of proof required by that legislation. This is precisely the stuff that spurious consensuses are made of.

[9]For the history of this act, and its subsequent amendment, I am relying on Lemons et al. (1989) and Lemons and Malone (1989).

Fiat is probably the most unstable basis on which to build consensus. Although decision by fiat is efficient, it is a primary condition under which consensus disintegrates and organizational legitimacy is lost (Bella 1987; Bella et al., 1988a, 1988b). Although DOE was not required to prepare Environmental Impact Statements for the sites it was considering, it did have to prepare formal environmental assessments. By 1986 DOE "had narrowed the number of candidate repository sites from nine to five and then issued final statutory [environmental assessments] in accord with NWPA procedures" (Lemons et al., 1989:29). These environmental assessments then provided ample fodder for criticism, partly because they were not as stringent as EISs and partly because they made more explicit the technical assumptions (and faults) that underlay DOE's choices. The next step in the selection process was to winnow the list from five states to three--Nevada, Texas, and Washington. These states were chosen largely on the basis of political and economic criteria, which, while certainly legitimate criteria on which to base decisions, were not the criteria with which the decisions were justified. This gave an aura of capriciousness to DOE's decision process.

Congress, with DOE input, then placed the final nail in the coffin of consensus regarding nuclear waste siting in Nevada. In 1987 Congress amended the Nuclear Waste Policy Act, mandating that only one site be developed--Nevada. There were a number of reasons for the amendment, but the important point here is that technical decision by fiat, when consensus will later be required, sets the stage for considerable future dissension. The amendment stipulated that Nevada be compensated for having a nuclear waste dump within its borders, but only if it waived veto power over the Yucca Mountain site, actively participated in DOE's program, relinquished any oversight role in the depository's management or even "the right to contest the site's suitability before the NRC in license proceedings" (Lemons et al., 1989:35-36). It would be difficult to imagine a better recipe for dissension.

I would be wrong to claim I knew how to construct a non-spurious consensus, especially regarding the risks of nuclear waste transport. At the same time, the history of decisions concerning nuclear waste siting affords at least a few positive suggestions. Because a spurious consensus is partially based on the assumption that many different actors share the same perspective, some way must be found to question such an assumption, perhaps even to annul it. For a variety of reasons, this is unlikely to emerge from within the organization with the primary responsibility for responding to risk. Outside criticism and thorough scrutiny is mandatory for any realistic hope of developing genuine consensus. Hence decision makers should develop mechanisms to recruit and develop points of view with which they may disagree. Further, some mechanisms should be developed that will foster compromises from all interested parties--simply having different opinions will not create the consensus necessary to agree on how to plan for and respond to nuclear waste transport risks. My suggestions will perhaps sound impossible, or maybe even obvious. I doubt they are impossible, although I would concede they will be difficult to implement. If they are obvious, there is little or no evidence that they have occurred to major decision makers--the Department of Energy or Congress.

▲ ▼ ▲

In this paper I have examined several barriers to organizational foresight. The analysis suggests that developing organizational foresight, and avoiding organizational folly, requires: 1) changing decision making procedures so that a wide range of heuristics are used, 2) deliberately encouraging criticism of existing organizational arrangements to increase the likelihood of transcending institutionalized solutions, and 3) avoiding premature closure on consensus. Acting otherwise invites political and technological catastrophe.

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