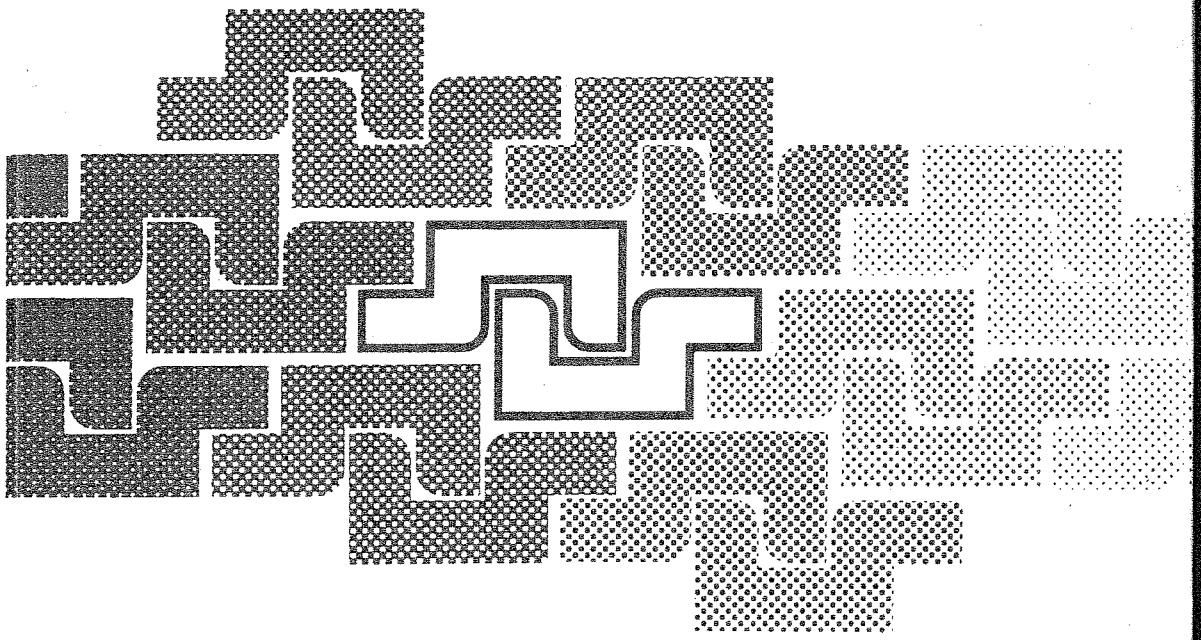

Environmental Impact Assessment, Technology Assessment, and Risk Analysis

Contributions from the Psychological and Decision Sciences

Edited by
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PREFACE

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Three Types of Risk Assessment: A Methodological Analysis[1]

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INTRODUCTION

The study of risks and the impact of new technological systems in our society and environment is now accepted as a legitimate subject of research.

As a consequence of the dual character of technology, a benefit, but also a threat, policy debates on innovations in technology are increasingly focused on "risk." This is seen most clearly in the case of the nuclear industry, where long and expensive enquiries are now necessary before new installations are permitted (as is currently the case with Sizewell B in the U.K.).

The issues are frequently divisive, involving heavy costs, both economic and political. In such debates, the established methodology of industrial risk assessment (or "Applied Science Methodology") is stretched beyond its limits of applicability and effectiveness. The techniques whereby a particular installation or process can be analyzed and its hazards identified and logically displayed, do not extend to systems which are novel, large, complex, and few in number. The hazards and environmental impact of these systems are incapable of description, measurement, or forecast with the precision experts have, until a few years ago, assumed to be possible and necessary for decisionmaking.

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Under these new circumstances, quantitative statements about risks and safety, which by their form seem the product of a rigorous scientific methodology and mathematical techniques, are frequently challenged as dubious or biased, and, sometimes, can be proved totally erroneous.

Now it is not only militant environmentalist groups that challenge the scientific character of risk assessment. Leading experts and theoreticians echo their doubts; the result is a severe loss of confidence in the whole field. The optimism of earlier years has given way to confusion and pessimism among many experts. This new mood may make it more difficult to achieve those genuine results of which the field is capable. This work is intended to remedy this situation by identifying the different types of practice in Risk Assessment, with their characteristic limitations and strengths.

STATE OF THE ART

Because of the lack of an accepted methodology that is both successful and reliable, almost all research work published in the theory of Risk Analysis is about "Foundations." Basic terms such as "probability" and "risk" are argued everywhere (and it is difficult to find the same definitions twice); consequently the estimation techniques differ considerably. Mandl and Lathrop provide an example of this problem in the context of Liquefied Energy Gas Terminals. (1)

> The best way to develop a definition of risk is to start by quoting some of the definitions from the risk assessment literature.
>
> SAI-USA: "Risk is the expected number of fatalities per year resulting from the consequences of an accidental event."
>
> CREM-UK: "Risk is the probability of an injurious or destructive event, generated by a hazard, over a specified period of time."
>
> BATTE2-OTH: "Group risk is defined as 'the frequency at which certain numbers of acute fatalities are expected from a single accident.' The risk to society as a whole is defined as

> 'the expected total numbers of acute fatalities per year resulting from accidental events in the system.'"

Although they emphasize the differences among the measures, we may note that three different terms for "probability" are used, each implying its own underlying theory of the measurement.

The optimistic expectations that sophisticated probabilistic techniques would solve the problem (2) have not been fulfilled; on the contrary, the experience of the last years has been of increasing uncertainty and debate. The concept of risk in terms of probability has proved to be so elusive, and statistical inference so problematic, that many experts in the field have recently either lost hope of finding a scientific solution or lost faith in Risk Analysis as a tool for decisionmaking. A recent authoritative report by an American committee comments (3):

> While, as emphasized in the first part of this report, analysis has a limited role in decisionmaking, it still can be a powerful tool for categorizing risks, assessing them, evaluating a given set of alternative coping strategies, and devising new alternatives. The actual role of risk analysis in particular situations is variable, as are the methods used. There is also the fact that in some decisionmaking on risk, analysis has no role; in other situations analysis is useful only if it is designed so that it provides information that the decisionmaker cannot obtain.

These confusions and disappointments are familiar to many experts in the field; it is enough for us to indicate them briefly. One recent theoretical response to them was an advocacy of "Baysian statistics," where the "subjective probabilities" based on the professional experience of experts, are assigned to those hypothetical events which are incapable of effective empirical study. These are then (in theory) modified by a formal calculus, when experience changes (4). But the variability of such expert judgments (5) and the inadequacy of the formalism in the case of very-low-probability judgments (which are the ones of concern) render this approach less fruitful than was originally hoped. Further, there is the discouraging experience that the serious accidents that do occur have tended to be of the sort where the mathematical theory of risks is irrelevant (Seveso), inadequate (Three Mile Island), or misleading (Salem, New Jersey).

Another response to this crisis in risk assessment has been to re-emphasize the "human" and managerial aspects of the creation of hazards. However, there has also been a tendency to a multiplication of traditional efforts in formal studies of risks. Both are represented in a comment by the New York Times (6):

> The (Nuclear Regulatory) commission has two ways of analyzing safety - field reports of utility performance and probability studies of risk. Recently it has been emphasizing risk assessment, which predicted the automatic scram system would fail once in 33,000 reactor years - hardly a red flag for the problem. But the Salem plant had been cited three times for below-average management. Risk assessment may help compare risks; for real-world accidents, empirical evidence would seem a clearer guide.
>
> Nuclear power is essential to an energy policy that seeks diverse sources. But the public will not respond in measured fashion to another core damage event; it may simply reach for the ax. The safety margin that prevented disaster at Salem is a thread too thin for a whole industry to depend on.

The same point is put even more forcefully in a leader in the New Scientist in London, a journal generally committed to the cause of civil nuclear power. Under the heading "Nuclear Incompetence," it says

> Perhaps the best thing that could happen to the world's nuclear power industry would be the closure of every nuclear power plant in the United States. Then we might see an end to the series of stupid mistakes that cast a shadow over nuclear power in other countries.
>
> Is it any wonder that some level-headed Americans aren't too keen on nuclear power, especially if it is in the hands of tin-pot utilities that cannot even keep track of their mail?... We can only hope that the U.S. does not drag every one down with it in a complete meltdown of nuclear power's already shaky credibility (7).

Such reflections give rise to renewed consideration of the cultural aspects of risk management. While it is only common sense to be cautious about installing nuclear reactors in such places as Zaire or Belfast, it is a sobering thought that the

United States of America may not be an appropriate milieu for what was until recently accepted as a basic, universal energy technology. For an understanding of this phenomenon we need a theory of regulation, which is beyond the scope of this present study (8).

Thus the "art" of the assessment of risks of complex technological systems is at an impasse. The early hopes that it could be reduced to a science are frustrated. And the crisis of confidence among the public in the assessment of such risks inevitably extends to the institutions that regulate the industries and also to the industries themselves. Although some individuals and agencies redouble their efforts in the old ways, others are tending to introduce the "human" and "cultural" factors. The question now becomes, to what extent should these predominate, to the reduction or exclusion of the "scientific" aspects? For, as we shall see, if the perceived phenomena of "risks" are interpreted as lacking all objective content or being merely a small part of some total cultural configuration, then there is no basis for dialogue between opposed positions on such problems. For this reason alone, such "relativist" arguments must be taken seriously. We analyze them in the next sections.

THE SUBJECTIVIST FALLACY

The very first issue of the first journal to be devoted to Risk Analysis contained as its main theoretical contribution a paper on the quantitative definition of risk. Appearing in such a position, the paper inevitably carries the authority of the community of experts who are represented by this journal. As a philosophical contribution, it therefore has an importance for policy debates which does not depend on the scholarly credentials of its authors. We therefore shall discuss it by the criteria of philosophical analysis, although the authors themselves may not have intended it as a contribution to a dialogue on the metaphysics of risks.

By "radical subjectivism" we mean the position which goes beyond the use of subjective probabilities and Bayesian statistics. Its proponents believe that there is a need to justify philosophically the use of subjective techniques, and they think that the justification would consist in showing that risk is a subjective phenomenon. An example is provided by Kaplan and Garrick (9).

> Connected to this thought is the idea that risk is relative to the observer. We had a case in Los Angeles recently that illustrates this idea. Some people put a rattlesnake in a man's mailbox. Now if you had asked that man: "Is it a risk to put your hand in your mailbox?" He

would have said, "Of course not." We however, knowing about the snake, would say it is very risky indeed.

Thus risk is relative to the observer. It is a subjective thing - it depends upon who is looking. Some writers refer to this fact by using the phrase "perceived risk." The problem with the phrase is that it suggests the existence of some other kind of risk - other than perceived. It suggests the existence of an "absolute risk." However, under attempts to pin it down, the notion of absolute risk always ends up being somebody else's perceived risk. This brings us in touch with some fairly deep philosophical matters, which incidentally are reminiscent of those raised in Einstein's theory of the relativity of space and time.

Their argument can be summarized as follows:

1. Example, <u>then</u> Risk is relative to the observer.

2. Risk is relative to the observer, <u>then</u> risk is subjective.

Let us consider first argument 1. It is clear that the letterbox being a hazard depends on the presence of a snake (unless everything is a hazard, making trivial the concept of hazard). Putting a hand in a letterbox will be risky given the presence of a snake; information about the presence of a snake will change the judgment about the situation and will, consequently, produce different behaviors. The letterbox with a snake is "risky" independently of the information possessed by a particular observer. What <u>is</u> dependent on the information available to a particular situation, given that his attention has been drawn to a particular situation, is his judgment and subsequent behavior, <u>not the situaton itself</u>. The snake is in the letterbox, and not in the mind of the victim. Thus this particular example does nothing to establish "relativity" in the notion of risk, at least with the meaning that Kaplan and Garrick assign to "relative."

A simple change in the example provided will show that there is no difference, in principle, between "risk" and "weight" as attributes. Instead of a letterbox, let us assume we have an empty dumbbell. If someone replaces the hollow dumbbell with a similar one, but filled with iron, we have the same situation in the two examples. If you are asked: "Is the dumbbell heavy?" the answer would have been, "Of course not" (to paraphrase our authors). We, however, knowing about the iron, would say it is very heavy indeed.

In the two cases there is a hidden factor, snake and iron. The aprioristic guesses about "riskiness" and "heaviness" will be based on experience and a state of ignorance of the observer about the real situation. An experiment will settle each question; for example, to look inside the letterbox, or to weigh the dumbbell. The aprioristic judgment does not necessarily have any predictive value, whereas the second kind of estimation could be used as a firm basis for future behavior.

We should notice that the experiment need not be a quantitative measurement. Just as we can speak of the "heaviness" of an object without weighing it precisely, so we can speak of the "riskiness" without going through the procedures necessary for a quantitative estimate.

We can now consider the second argument: "If x is relative to the observer" then "x is subjective." The following example will show its falsity.

We have planet 1 (red) with an observer 1 and planet 2 (blue) with an observer 2; there is an astronaut in a rocket, outside the visual range of the observers; both observers are "Ptolomeic," whereas the astronaut is trained in Analytical Philosophy. For observer 1, planet 2 orbits around planet 1, and vice versa. For the astronaut, the problem of which planet is orbiting around which is a pseudo-problem, but as a matter of convenience we could decide that the blue one orbits the red one.

The movement of planet 2 is relative to the position of the observer 1 and vice versa; each observer will judge the situation differently and will behave accordingly, but the movement of the planets is not "subjective."

THE "SOCIAL-REDUCTIONIST" HAZARD

"Social-reductionism" is a methodological position which assumes that every debate over technological risks is really a conflict among contradictory "ways of life" and that the awareness of this would be enough to settle the question. An example is provided by B. Wolfe (10). Someone who believes that the future welfare of society is dependent on new domestic energy supplies will see large advantages to the development of nuclear power, off-shore oil resources, and new sources of coal, even at some risk and inconvenience. Those who believe that society suffers because we already use too much energy will not accept even minimal risk or inconvenience in order to

supply more energy. A public discussion of energy development between groups with these opposing views is like a discussion of pork processsing among farmers, meat processors, and orthodox Jews and Muslims. One may talk about humane slaughtering techniques, but the underlying issue is whether or not pork should be eaten. The issue may be couched in technical terms of "spent-fuel disposition," but in fact it is an argument over the morality of eating pork.

The issue could be, of course, "an argument over the morality of eating pork" but the general public and authorities are also concerned with the possibility of "trichinosis" and this concern is shared by the pork industry because cases of disease from eating pork are (at the very least) bad publicity and financial loss for themselves. One may talk about "humane" slaughtering techniques and sometimes "the underlying issue is whether or not pork should be eaten," but it is also about public health, honest or dishonest trade, and controls.

This relativistic position is unlikely to provide a useful solution to the problems faced by risk assessors and decisionmakers. It has not solved the methodological problems of anthropologists, social scientists or philosophers either, and it is in these fields where it is strongly opposed (11). Moreover, it can be used as an excuse to deny the possibility and need of a dialogue and a rational debate on basic problems of technological risks and choices.

The most significant contribution to this approach is the recent book by Douglas and Wildavsky (12). Combining the approaches of political science and anthropology, they produce a powerful case against the pretensions of a "scientific" approach to the management of the many risks to which high-technology civilization is subject. They show that a program of managing all possible risks is impossible; that many significant risks are ill-understood; that the selection of risks for popular concern in America is definitely <u>not</u> done on a scientific basis; and that those who campaign most intensively on particular hazards are motivated by a crusading zeal for a vision of a radically different civilization.

So far their argument expresses, in a particularly erudite and insightful way, the dilemmas of environmental risk management, as now agreed by nearly all who are involved in it. We may say that risks are now revealed to lie outside the province of Science, and are more like those of politics, or of life in general. But Douglas and Wildavsky press their analysis further; they wish to explain <u>why</u> this strident environmentalist movement arose at a particular time in the United States. For this they construct a theory of three sorts

of social institutions, "bureaucracy" and "market" (occupying the "center") and "sects" (occupying the "border"). The "center" institutions have failings of their own, familiar to social critics and well detailed here. Less familiar are those of "sects;" their peculiar constructed-world of moral absolutes is derived from the psychodynamics of closed voluntary societies. The history of the classic closed religious communities in North America is here used as the sole evidence of this argument, summed up in the assertion (p. 189), "Border theories emanate from its social predicament of social voluntariness."

The conclusion from this analysis is that "sects" enter into the political process only sporadically in response to a crisis or scandal where the center cannot cope; and there is never any true dialogue between the mutually exclusive cultures. The policy implications of this conclusion are not made explicit by the authors, for their concluding discussions are of philosophical relativism and of "resilience" as a strategy for coping with present and future risks. However, their use of the term "sectarian" for the organized critics gives a hint, as does their comment (p. 11), "The remedies most easily proposed in such organizations are to refuse to compromise with evil and to root it out, accompanied by a tendency towards intolerance and drastic solutions." It is hard to see how they could disagree with the analysis and conclusions of B. Wolfe, mentioned above. Nor would it be easy to escape the practical conclusion that these new "sectarians," like all others in that historic category, are not entitled to respect as participants in a policy debate. For they join it for ulterior motives, and given their absolute and intolerant commitments, will only abuse its procedures for their own ends.

These conclusions (nowhere asserted but nowhere denied by our authors) could have serious consequences for environmental politics, and so deserve careful thought. They also run counter to the recent American experience of pressure-group politics, on the environment and other issues. On this latter ground alone, we are entitled to scrutinize the authors' models as a reflection of social reality. Of course the three sorts of organization are "ideal types," and their authors analyze their real-life manifestations with great insight and subtlety in respect of their internal workings. But their theory of the psycho-dynamics of "sects" is based on closed communities that isolate themselves from society. What resemblance do these have to the politically active national organizations that rely on a mail-order membership, or even local activist groups which are only NIMBY ("Not in My Back Yard") coalitions? Not very much at all, except sometimes in a sort of style of rhetoric, which on occasion is even shared by the United States Congress (p. 163)!

Although the authors discuss the activist groups, their concentration on their own model inhibits any real insight into politics as it is practiced in America. While they analyze the interactions of the two sorts of institutions of the "center," they have very little to say on those between "center" and "border." The reader is left with a picture of total non-communication between a "complacent" center and an "alarmist" (or rather, fanatical) border.

In this way the authors have missed the opportunity to analyze real roots and stylistic tendencies in American political life in relation to their problem. Better than "sects," we can think of such terms as "populist," "evangelical," "crusading," "factional," and "litigious" to describe the traditional style of American reforming politics. Such great historic campaigns as anti-slavery, free-silver, and prohibitionism had their "sectarian" beginnings, and rose to positions of significant strength. More recently, some radical compaigns have failed (as on the hazards of recombinant DNA research), while others have gained surprising strength (as opposition to nuclear weapons strategy). This is the stuff of politics in America; all the skills of political science and cultural anthropology would be relevant to its study. But the authors are quite unconcerned with this level of reality, and their analysis is correspondingly abstract and impoverished.

Seeing the risks agitation nearly as a unique sectarian phenomenon, they tend to become influenced by the cross-cultural study of risks. Fascinated by the bizarre collections of beliefs about risks which are the stock-in-trade of anthropology, they convey the impression that all risks are equivalent, on the classic lines of "pollution is in the nose of the beholder." Thus asbestos hazards are described as follows:

> Why is asbestos poisoning seen to be more fearsome than fire? Asbestos was developed to save people from burning; asbestos poisoning is a form of industrial pollution whose toll of deaths by cancer justified a particular anti-industrial criticism more strongly than does loss of life by fire. (p. 7)

In view of the well-known history of willful neglect of the hazards of asbestos, industrial and environmental, such an analysis is more characteristic of a "border" (though <u>not</u> the one criticized by the authors) than of a "center."

Only an important book merits such detailed criticism; the authors have made a real contribution, through their theoretical insights and case studies. Their solution to the problem of civilized dialogue of risks is, in our view, excessively skeptical. But the problem exists, and they have laid down a challenge to all those who believe in the possibility of a solution. We hope to make a beginning on this in our three-fold division of risks problems in the next section.

A SOLUTION: THREE TYPES OF ENQUIRY

Although no one asserts now that Risk Assessment applied to complex technological systems is a Science, science continues to be the desideratum, as a model of practice, among a majority of experts. But, is the methodology so successfully used in research science suitable to the sort of enquiry found in Risk Assessment? There are two reasons why it is not. First, it is well known that the ideal of a science consisting of exact quantitative "public knowledge" (13) is frequently unattainable in this sort of work. This failure is commonly ascribed to the degree of complexity, uncertainty, and ignorance in crucial problems. But the difference between this field and research science is not merely one of degree. Here, a second dimension, containing the so-called "value aspects," is an essential component of the problems. In this respect this field is qualitatively different from research science.

The "values" dimension does come into scientific practice, as in peer-review of research projects or in general setting of priorities. However, outputs of research as realized in papers, are formally at least uni-dimensional, purely factual. Any methodology that pretends to be successful in Risk Assessment has to take into account dimensions, facts, and values.

We think we can solve the difficulty created by the contradiction between the ideal of "public knowledge" science and the characteristics of the problems encountered in Risk Assessment, without falling into sectarian relativism or social reductionism. The first step is to distinguish among the different types of problems in Risk Assessment and then to apply an appropriate methodology to each kind. The problems would be analyzed as functions of the two dimensions mentioned above: "systems uncertainty" on the technical side, and "decision stakes" on the values side, both varying independently.

"Systems uncertainty" contains the elements of inexactness, uncertainty, and ignorance encountered in the scientific and technical studies; whereas, "decision stakes" involves the costs and benefits to all the interested parties, of all the various available policy options, including delays in decision of some definite or indefinite duration. It also includes possible costs to the consultants themselves of advice which is unwelcome to the client!

The diagram (which should be seen only as a convenient sketch) indicates the realms of application of three different methodologies and the names in it indicate them (Fig. 1). When the uncertainties of the system and of the decision stakes are small, and the data bases are relatively large and reliable, we have the sort of problems where the "Applied Science" methodology has proved to be successful. Even some relatively rare events, involving quite high decision stakes, can be brought under control by "applied science;" aircraft accidents and disasters are a good example of this. One reason for this success is the availability of a good database, in the records of the extremely large number of movements and the large number of recorded incidents of varying degrees of severity. This is supplemented by empirical knowledge of the ratio of non-reported incidents to those reported. In such a case, it is possible to construct a "risks pyramid," giving ratios of empirical frequencies of related sets of incidents, accidents, and disasters. By their means, control-measures may be devised to reduce incidents and their more serious consequences in salient cases (14).

When both the systems uncertainty and the decision stakes are considerable, but where professional expertise can still operate, we will define a different model of practice, the "Technical-Consultancy" methodology; it still involves the use of quantitative tools, but these are explicitly supplemented and interpreted on a qualitative basis by experienced judgment. The result is not intended to function as an element of "public knowledge" science; it is not designed to be fully testable and reproducible, nor to be applied mechanically to other similar problems; and in this sense, this kind of enquiry fails to satisfy the epistemological criteria for research science. Rather, it is an input to a process of decisionmaking in which risks are one among several factors. The values at issue condition the enquiry at every stage, because of its function in a particular decision process.

Outstanding examples of the "technical-consultancy" methodology are the two studies on the petro-chemical installation on Canvey Island, by the Health & Safety Executive (15). There they dealt with a particular site, in terms of its special problems; they claimed no general results, and gave cautions

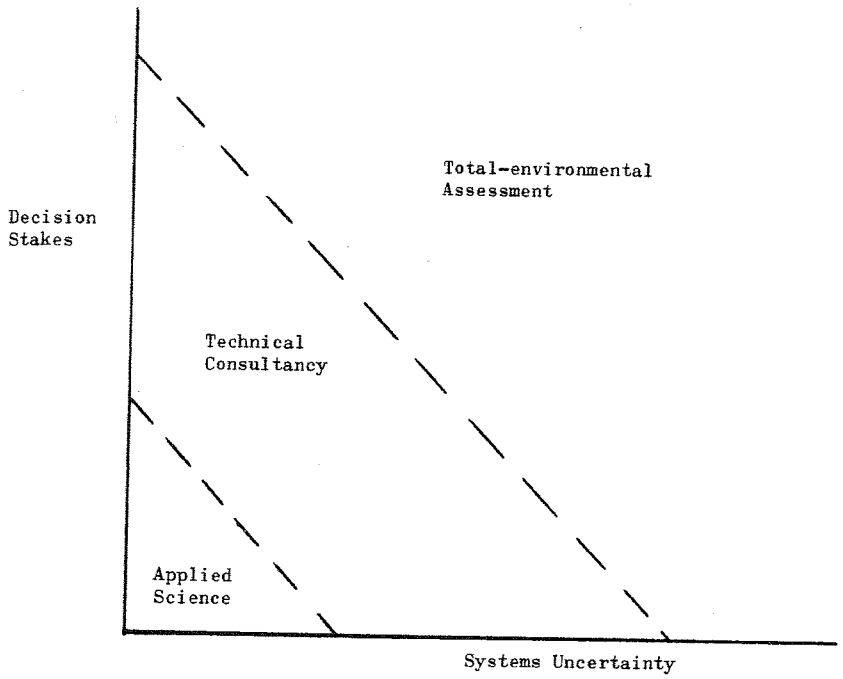

Fig. 1 : Classification of types of enquiry involved in Risk Assessment.

about the inexactness of their probabilistic conclusions, in terms of the varying degrees of reliability of their data. Although they concluded with a policy recommendation, they had sufficient detail on many local hazards for their prescription to be applied to a drastic improvement of safety on the island.

Nowhere would the reports claim that the island could be made "safe," or that their conclusions were "scientific." Indeed, the two reports were commissioned and received in a highly-charged political atmosphere, and criticized in proceedings of an adversarial nature. But in terms of the stated tasks of the enquiries, and also as examples of how such work is to be done, they stand as solid accomplishments.

Finally, when decision stakes and systems uncertainty are very high, we have a "Total environmental assessment," which is permeated by qualitative judgments and value commitments; its result is a contribution to an essentially political debate on larger issues, though no less rational in its own way for that. The enquiry, even into technical questions, takes the form largely of a dialogue, which may be in an advocacy or even in an adversary mode.

Although only a small proportion of risk assessments fall into this category, they are frequently those of greatest practical and political significance. It is to them that the analysis of "social reductionism" is most plausibly applied.

It is very important to realize that a "total-environment" problem is, by definition, not simple or static. Although value-commitments may shape arguments and enquiries on many important aspects, the debates are not all in the realm of metaphysics, nor are the decision-stakes purely in the area of sectional power-struggles. Thus, to take the example cited above, the issue of asbestos hazards is not simply a case of disliking industry more than fire. Even on more complex and speculative issues, as energy supplies, there is a steadily growing area of scientific knowledge and technical expertise, which complements the value-considerations, reducing uncertainties on particular critical decisions, and perhaps securing agreement among experts on all sides concerning particular technical problems. The hearings on the PWR at Sizewell is an example of how, in spite of an overtly adversarial style of debate, common accumulated experience yields an evolution of the issues, if not policy agreement then at least to consensus on salient areas of debate. This last is all we require for a problem in this domain to be excluded from the "social reductionist" model of environmental debates. This evolution, toward rationality and dialogue, may indeed take years to accomplish; in its early stages a "total-environmental assessment" may really seem to be a clash between incommensurable world-views. But such debates tend to stimulate the production of knowledge, of relevant facts and of value-commitments, which eventually enable such problems to be resolved by political debate rather than by civil war. It is important to note that the classification is not static but dynamic, in the sense that any changes in any (or both) of the dimensions could push a particular problem completely from one realm to another, consequently imposing a change in the way it is handled. The most notable recent example of this (welcome) tendency is the problem of possible physical and psychological harm to children resulting from atmospheric pollution by petrol-derived lead.

We should also observe that all these types of problem-solving are different from the practice of "safety management" or "loss control" in an installation that is in operation. There the "scientific" work of survey, analysis, and design is organized around the influencing of human behavior by various means. The problems of this field have been analyzed in terms of a "risk triangle," illustrating the varying valuations and perceptions among those who impose, endure, or regulate the risk (16).

CONCLUSION

This study began with a review of the crisis in confidence in the assessment of technological risks. The earlier optimism in the power of scientific method to assess all risks has given way to confusion and loss of confidence in any risk assessment. This tendency to despair can have serious consequences for the urgent tasks of social management of the technological risks of the present and future. Attempts to identify new philosophical foundations for risk assessment have proved fruitless. We have therefore concluded that the construction of a general theory applicable to all sorts of risks is not the appropriate way forward.

Our path to a solution lies in a distinction among the different types of risk. It's not merely that some are inherently more difficult to assess. Rather, the tasks of assessment for the various cases are qualitatively different. By analyzing the appropriate methodology for each case, we are able to identify the areas that are particularly problematical and challenging, and we can also explain and reinforce successful practice where it exists. Also, by showing what sort of "knowledge" is actually needed from any particular type of Risk Assessment, we avoid the error of excessively severe demands and high expectations; and we thereby prevent the disappointment and confusion that occurs when these are not fulfilled.

We have shown that there is a large and important area where scientific techniques can be successfully applied to complex technological risks even where important policy decisions are at stake; we have called this "technical consultancy." Its success, when properly done, can be understood when we appreciate the nature and function of its outcome. Its conclusions are not intended to be a weaker version of the "public knowledge" resulting from research science. Rather, they are an input to a decision process, constrained by the particular problem set by the client, and validated both by the

scientific techniques and by the skill and judgment of those who apply them.

Even in the case of "total-environmental assessment," the problems are not an undifferentiated mass of scientific speculations and cultural values. Such issues always include components capable of study by "technical consultancy" or even "applied science." Also, public debate on such problems stimulates research whereby the "scientific" component is strengthened, perhaps eventually to the point where the value-commitments themselves are changed in the light of new evidence and new experience.

The assessment of technological risks is a very new field. It is only natural that it should go through a cycle of early over-optimism followed by disappointment and pessimism. Our work has been intended to build toward the next phase of this practice, involving a more mature appreciation of its real strengths and limitations. We have done this partly through identifying and reinforcing successful professional practice where it exists, and using this as the basis for further study.

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