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Comment

Toward Postmodern Risk Analysis[1]

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I am a stranger to risk analysis, having wandered into its territory from anthropology by way of Yucca Mountain, Nevada, where Kasperson, Slovic, and a good many others including me, are trying to assess the social and economic impacts that may follow from its nomination as a possible location for a high-radiation-level nuclear waste repository. I am out of my own territory and I have not been able to spend any time investigating the customs and usages of yours. If I had not been asked to comment on "The Social Amplification of Risk: A Conceptual Framework" you may rest assured I would not have said a word, and prudence might have suggested to me that I decline to do so in any event. It has been my experience, however, that children and strangers sometimes notice things that the locals and the grown-ups have forgotten or never noticed, and that sometimes their naive impressions are worth more expert consideration. Construe my remarks in such a light.

I make the assumption that Kasperson *et al.*'s essay, in pointing in the direction toward which they think risk analysis should move, also indicates, albeit in a very general and necessarily simplified way, the predominant orientation away from which they are moving. I commend the authors and whoever else among their colleagues joins them in attempting, by whatever methodology, to deal in increasingly adequate ways with the complexities of the *human* world. This is how I construe their general endeavor and my remarks are intended to abet it.

It is, I think, of more than academic interest that the contrast, discussed by Kasperson *et al.*, between "traditional" risk analysis, which, I gather, confines itself to assessments of the "probability of events and the magnitude of specific consequences" (expressed as a product in the mathematical sense) on the one hand and, on the other, more comprehensive analyses taking into consideration "social amplification," is an instance of the opposition between what Stephen Toulmin[1] calls "modern" and "postmodern" science. In his terms, "modern science," which was given its definitive shape by Descartes, is characterized by (1) an objectivity that radically separates the scientist from the system observed; (2) the division of knowledge and its pursuit into ever narrower disciplinary specializations, involving increasingly specialized methodologies; with the result that (3) little or no attention is paid to the integration of systems "as wholes" because systems as wholes are not in anyone's particular bailiwick. In contrast, postmodern science, says Toulmin, (1) returns scientists to the systems from which Descartes alienated them but in which, nevertheless, they (along with the rest of humanity) are increasingly important actors; (2) must be increasingly concerned with the integration of social-ecological systems; and (I would add) (3) therefore must be as concerned with information, meaning, and motive as with measurement and physical causation.[2]

The distinction between modern and postmodern science is, then, illuminated by comparing risk assessment, a technical activity of experts, with risk perception, the recognition and interpretation of danger signals *by participants* in the systems that those experts study. The formal methods of risk assessment alienate the experts from those systems. Their methods aim to produce an objective result purporting to be a "true measure" of risk. I doubt if anyone claims that these formal methods resemble the ways in which people ordinarily recognize and assess risk, and, as I understand it, there is no place in the formal analysis of risk for native evaluation of risk. Experts may regard the latter as little

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more than sources of error, which, unfortunately, the natives may prefer for irrational reasons to their own more accurate assessments. Postmodern risk analysis, taking the social-ecological system as a whole as its domain, would incorporate native perceptions into the analysis as more than external sources of misleading information. This is what I take Kasperson *et al.* to be attempting.

The specific model of social amplification presented by the authors seems to this foreigner a good and plausible approach to what will in all cases be extraordinarily complex processes, processes that in all cases will be in some respects unique. Every element of the model deserves lengthy and detailed discussion, which, I suspect, they will be receiving from now on. In the limited space available to me here I can only make a few general points.

First, social and cultural processing of information is intrinsic to all transmissions concerning risk. There is no such thing as a pure, objective assessment, that is, one free of "social amplification" in the authors' general sense. The most objective assessments, after all, are based upon the heuristics assumed valid by a particular scientific subculture. Such quantitative heuristics in their nature necessarily ignore, or at least deal only peripherally with, nonquantitative aspects of risk.

Second, to generalize from the ubiquity of social amplification, social amplification in its general sense has its ground in the assumptions of the assessors. These assumptions are, in the main, culturally established but tempered by individual experience. There is room here for both anthropological and psychological input. These assumptions are, moreover, affected by such characteristics as economic status, political position, ethnicity, and education. Enter the rest of the social sciences. This may be obvious but should be emphasized.

Third, it follows from the second point that social amplification does not commence with the transmission of alarm signals concerning risks already perceived but with the recognition of the dangers in the first instance.

Fourth, among the variables that will affect the attention paid to a message is the nature of the transmitter's authority. Although it is easy to make gross discriminations about the differential "authoritativeness" of authorities (e.g., a story about U.F.O. landings appearing in the *New York Times* would get more attention from most educated readers than one in *The National Enquirer*), much remains to be learned about this matter.

Fifth, a secondary risk or "metarisk" that becomes apparent in the consideration of social amplification iss the risk *for authorities themselves* attending their assessment of risk and their transmission, amplification, dampening, or editing of alarm signals. They run the risk, if their transmissions are later shown to be erroneous or deliberately falsified, of being discredited. Much more serious than discrediting of particular authorities is the risk to the social system generally of loss of confidence in its own information processes—its basic values, meanings, and understandings of the world, its trust in the probity not only of particular authorities but authority generally. There is, by the way, no real way to quantify consequences of this nature.

Sixth, it should be kept in mind that, in the first instance, the criteria against which the results of a "traditional" risk analysis (an objective calculation of the probability of an event and of the magnitude of its consequences) on the one hand, and a signal encoding the perceptions of members of a society on the other are to be assessed are very different. The criterion against which the results of a traditional risk analysis are to be assessed is accuracy (how you judge accuracy is another matter, of course). That is, we say it is a good analysis if we are persuaded that its numbers conform to "reality." In contrast, the criteria against which danger signals attended to by members of endangered populations are to be assessed are adaptive. Does attention to the signal whether or not it is in a strict sense accurate, enhance well-being or survival? It is obvious that accuracy and adaptiveness are not coextensive categories. It is easy to imagine the adaptive value of amplification or exaggeration under some circumstances, the adaptive value of playing down danger in others. In fact, some alarm signals—we can think here of many of those predicting environmental disaster—are adaptive for the very reason that they help to induce actions that lead to their own falsification. There are self-falsifying as well as self-fulfilling prophecies.

The fifth point above leads back to the general matter of postmodern science and its necessity. The movement from modern to postmodern science is crucial, says Toulmin, because both the complexity of the world and its coherence (the degree to which its components are articulated) are increasing, largely as a consequence of technical, economic, and politi-

cal developments and also because the capacity of humans to transform the world for better or worse for increasingly trivial reasons of their own (e.g., to destroy the ozone layer in the service of propelling underarm deodorants with fluorocarbons) is also increasing. It may also be crucial in risk analysis—I leave risk analysts to decide—because traditional risk analysis may not be able to provide realistic assessments (products of probability and gravity of consequences) in more than a very limited class of cases.

Although risk analysts may be able to calculate the probability of occurrence of a fairly broad range of events, their ability to predict and to calculate the magnitude or gravity of their consequences with any accuracy may be much more limited. For one thing, some consequences are not in their nature metrical, and attempts to develop metrics to indicate them really misrepresent them. I have already noted as an example that a possible cost of misrepresentation in the transmission of risk signals could be the corrosion of the confidence of members of a society in their own institutions. This does seem to me to be a grave consequence and it also seems to me to be intrinsically nonmetrical. Nonmetrical examples beyond number could be adduced. How, for instance, would anyone measure the gravity or magnitude of, let us say, the consequences of the development of mining in a previously economically undeveloped area in Papua New Guinea, as a function of which new wealth produced by wage labor became concentrated in the hands of young men who had previously been dependent upon older clansmen for money and valuables necessary to pay bride prices (still ubiquitous in that country and likely to remain so for decades or—I would not be surprised—even a century to come)? What is at stake here is not something metrical but something structural, namely, intergenerational relations upon which cultural continuity has depended. Or consider the risks inherent in the granting of mining leases to a foreign corporation by those same tribesmen to extract gold through open pit methods. Some consequences are obviously metrical—the acres of garden land that simply disappear into a large hole, the production lost, the money paid, some of the environmental impacts. But other consequences include the commoditization of land that previously had not simply been owned, but to which connection had been mystical in a very profound sense. The Maring, a Papua New Guinea people among whom I have lived, assume that their first ancestors actually emerged from clan lands and that, therefore, they and their land are of one substance.

The consequences in all three of the hypothetical examples adduced lie in the realm of meaning and value. The human world, the world inhabited by humans, is not constituted by physical, chemical, and biological processes alone. It is also constituted culturally. As such it is not simply made of trees and rocks and water and organisms, but also furnished with and by such conceptions as truth, honor, democracy, ancestors, and gods. These conceptions figure as largely in the motives of individuals and in the ordering and governance of societies as do trees and rocks and life itself. Threats to them are not simply figments of ill-informed people who will use such understandings to resist the realistic calculations of dispassionate experts (although they may so use them). They are real. Their consideration may be beyond the scope of Kasperson *et al.*'s initial program but not beyond the possibilities of the postmodern risk analysis toward which they point.

REFERENCES

1. S. Toulmin, *The Return to Cosmology,* Berkeley, University of California Press (1982).
2. G. Bateson, *Steps to an Ecology of Mind,* New York, Ballantine Books (1972).