Scientific Method, Anti-Foundationalism, and Public Decision-making

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Abstract
An examination of the legitimacy of attacks on lay assessments of environmental or other technological Risk. The case is made that rational policy requires an epistemology in which what we believe about Risk is bootstrapped onto how we should act concerning Risk.

Keywords
technology, assessment, fear, Risk analysis, choice, confidence
Scientific Method, Anti-Foundationalism, And Public Decisionmaking

Kristin Shrader-Frechette*

Introduction
The U.S. Department of Energy (DOE) recently awarded $85,000 to a Washington psychiatrist to help "counter the public's 'irrational fear' about nuclear power." Robert L. DuPont, a former director of the National Institute on Drug Abuse, received the funds for a study that has been described as "an attempt to demonstrate that opponents of nuclear power are mentally ill."1

DOE's fears about public irrationality regarding technological risks, however, are not atypical. At least three groups of persons maintain that citizens' worries about environmental risks, from carcinogenic pesticides to loss of global ozone, are irrational. Industry spokespersons, risk assessors, and contemporary social scientists have all attacked the environmental fears of laypeople as irrational. Edith Efron, for example, author of THE NEWS TWISTERS and THE APOCALYPTICS: POLITICS, SCIENCE, AND THE BIG CANCER LIE, maintains that both citizens and scientists have attempted to incriminate industry in the name of "cancer prevention," even though the evidence

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that certain substances cause environmental and human harm is typically questionable. Public fear of cancer, she says, derives from "warring attitudes toward the American industrial system" and "fantasies" about "industrial mass murder."

Risk assessors, often experts in the employ of industries responsible for the hazardous technologies they are paid to evaluate, constitute a second class of persons critical of alleged citizen "irrationality." Norman Rasmussen, for example, author of the most famous risk analysis, of commercial nuclear reactors, has accused the public of "inconsistency" in its attitudes toward hazards. On his view, anyone who travels by automobile, but opposes commercial nuclear fission, is inconsistent in accepting a large risk but rejecting an allegedly smaller one.


Although society's intuitive evaluations of given risks do not provide sufficient grounds for arguing that these risks ought to be evaluated in a certain way, many risk assessors maintain that correct societal evaluations are consistent with tenet (3) and the linearity assumption. Hence, in response to alleged counterexamples
Contemporary social scientists, especially sociologists, constitute the third main camp of persons who are critical of lay attitudes toward technological and environmental risk. Wildavsky and Douglas argue in their study that Americans are biased, witchhunting opponents of technology. They claim that laypersons are dominated by superstitions about environmental risks and by fundamentalist desires for unrealistic environmental "purity." Even more surprising, they allege that these contemporary superstitions and biases are no different in kind from those of pre-scientific, primitive people.5

The Failure of Foundationalist Positivism

How legitimate are these three attacks on lay accounts of the acceptability of environmental/technological risk? Dismissing the hazard evaluations of the public is highly questionable both for specific and for general reasons. On the specific level, the attacks are problematic because they are premised on experts' highly stipulative, question-begging definition of risk, as reducible merely to an average annual probability of fatality. Most experts also presuppose, following Bayesian accounts of decision theory, that societal or public risk aversion is a linear function of the average annual probability of fatality associated with the hazard.6

5 A. WILDAVSKY AND M. DOUGLAS, RISK AND CULTURE (1982).
6 For an overview of risk assessment and its Bayesian foundations, see K.

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Laypeople, however, typically do not restrict risk to probability of fatality, but include other factors, such as benefits obtained by taking the risk, in their evaluations. Hence their risk aversion is not a linear function of probability of fatality, since it incorporates numerous other parameters such as degree of knowledge of the hazard, equity of risk distribution, and so on.\textsuperscript{7} If one accepts a more inclusive, citizen definition of risk, then there are few grounds for alleging that laypeople are irrational simply because they deny that environmental risk aversion is a linear function of probability of fatality.\textsuperscript{8}

Experts' dismissing the hazard evaluations of the public is also questionable from a more general point of view because there is no accepted algorithm for theory choice, either in pure science (if there is such a thing) or in assessment of technological or environmental risk. And if there is no algorithm, the possession of which guarantees the certainty of experts' judgments about science and technology, then there is no completely firm basis from which to discredit similar judgments made by laypersons. Ever since the failure of Carnap's enterprise, epistemologists like Sellars and Quine have realized that the positivist goal, of finding a specific rule or method to guarantee the rationality of science or knowledge, is incapable of being achieved.\textsuperscript{9} As a consequence, epistemology in the late nineteen-eighties has been naturalized. To greater and lesser degrees, philosophers have concluded that there is no absolutely reliable method for delivering certain knowledge, just as alchemists finally concluded, centuries before, that there is no recipe for turning base metal or earth into gold.

\\textsuperscript{8} For arguments to this effect, see supra, note 6, Ch. Six.
But if there is no sacrosanct method that always guarantees certainty, whether in science or environmental policy analysis, then our alternative is to naturalize knowledge. On at least one version of naturalism, this means that we must replace the positivists' question, "how ought we to arrive at our beliefs?" with the naturalists' question, "how do we arrive at our beliefs?" since the latter is more tractable. Even Hempel, one of the grand old men of positivism, has moved to a somewhat naturalized epistemology. He has admitted that there is no specific rule or method to guarantee the complete objectivity of theory choice, and he has said that science can be guided only in a general sense and only by attempting to provide theories that embody values such as predictive power and explanatory fertility.

On this naturalized account, although scientific knowledge is not completely relative, it is unavoidably value-laden. No scientific theory can be said to be wholly objective, but one theory is more objective than another if it leads to better predictions or has more explanatory power. Such judgments of objectivity depend on numerous epistemic value judgments about particular predictions or explanations. Some of these epistemic or methodological value judgments concern, for example, whether the data is extensive enough or representative enough to support a given prediction. Other epistemic value judgments affirm, for example, that ignoring certain parameters does not jeopardize the explanatory power of a particular account.

10 See id. for discussions of naturalized epistemology. D. SHAPERE, REASON AND THE SEARCH FOR KNOWLEDGE (1984), also presents a well known naturalistic position.

There is no space here to argue for or against any one of the many variants of naturalized epistemology. Nor is there time to provide an anatomy of the epistemic value judgments whose presence, even in allegedly pure science, has toppled the positivist edifice of certain and wholly objective knowledge.\textsuperscript{12} What is more important, however, is to trace some of the consequences, for policy about technology, if the current wisdom about naturalized epistemology is correct.

Most obviously, if there is no specific algorithm for theory choice, then scientific experts have no completely unassailable basis on which to criticize the policy evaluations of the public. Granted, experts typically have a better grasp of the mathematics and facts relevant to policy about science and technology. The problem, however, is that even science, and assuredly technology assessment or environmental impact analysis, involve more than mathematics or facts; because they do, there is room for considerable controversy over the epistemic or methodological values in terms of which the facts and mathematics relevant to policy analysis are interpreted.\textsuperscript{13}

\textbf{Experts Are Often Wrong}

That expert interpretations in the area of science and technology are often questionable, and that there is no positivist rule to guarantee their complete reliability, is illustrated by a recent study by hazard assessors in the Netherlands. They used actual empirical frequencies obtained from a study done by Oak Ridge National Laboratories to calibrate some of the more testable subjective probabilities used in the famous


\textsuperscript{13} \textit{Id.}. For a discussion of epistemic value judgments in science, see K. SHRADER-FRECHETTE, \textit{SCIENCE POLICY, ETHICS, AND ECONOMIC METHODOLOGY}, Chs. Three and Four (1985).
Rasmussen Report, WASH-1400, probably one of the most famous and most extensive risk assessments ever accomplished. The Oak-Ridge frequencies were obtained as part of an evaluation of operating experience at nuclear installations. These frequencies were of various types of mishaps involving reactor subsystems whose failure probabilities were calculated in WASH-1400. The Oak-Ridge study used operating experience to determine the failure probability for seven such subsystems, and the Dutch researchers then compared these probabilities with the 90 percent confidence bounds for the same probabilities calculated in WASH-1400. The subsystem failures included loss-of-coolant accidents, auxiliary feedwater-system failures, high-pressure injection failures, long-term core-cooling failures, and automatic depressurization-system failures for both pressurized and boiling water reactors. Amazingly, all the values from operating experience fell outside the 90 percent confidence bands in the WASH-1400 study. However, there is only a subjective probability of ten percent that the true value should fall outside these bands. This means that, if the authors' subjective probabilities were well calibrated, we should expect that approximately ten percent of the true values should lie outside their respective bands. The fact that all the quantities fall outside them means that WASH-1400, the most famous and allegedly best risk assessment, is very poorly calibrated. Moreover, the fact that five of the seven values fell above the upper confidence bound suggests that the WASH-1400 accident probabilities, subjective probabilities, are too low. This means that, if the Oak-Ridge data are correct, then WASH-1400 exhibits a number of flaws, including an overconfidence bias.

Kahneman and Tversky have uncovered other biases of experts.

14 WASH-1400, supra, note 3. This figure is also a per-year, per-reactor probability.

They corroborated the claim that, in the absence of an algorithm completely guaranteeing scientific rationality, experts do not necessarily or always make more correct judgments about the acceptability of technological risk than do laypersons. Kahneman and Tversky showed that virtually everyone falls victim to a number of characteristic biases in the interpretation of statistical and probabilistic data. For example, people often follow an intuition called representativeness, according to which they believe samples to be very similar to one another and to the population from which they are drawn; they also erroneously believe that sampling is a self-correcting process.\textsuperscript{16} In subscribing to the representativeness bias, both experts and laypeople are insensitive: to the prior probability of outcomes; to sample size; to the inability to obtain a good prediction; to the inaccuracy of predictions based on redundant and correlated input variables; and to regression toward the mean. Nevertheless, training in elementary probability and statistics warns against all these errors.\textsuperscript{17}

Both risk assessors and statistics experts also typically fall victim to a bias called "availability," assessing the frequency of a class, or the probability of an event, by the ease with which instances or occurrences can be brought to mind. In subscribing to the availability bias, they forget that they are judging a class on the basis of the retrievability of the instances, and that imaginability is not a good criterion for probability.\textsuperscript{18}

Most people also fall victim to the "anchoring" bias, making


\textsuperscript{17} Id., at 4-11.

\textsuperscript{18} Id., at 11-14.
estimates on the basis of adjusting values of an initial variable. In so doing, they forget: that diverse initial starting points typically yield different results; that insufficient adjustments can skew results; and that probabilities of failures are typically underestimated in complex systems. Although employing each of these biases (representativeness, availability, and anchoring) is both economical and often effective, any of them can lead to systematic and predictable errors.\(^{19}\)

These systematic and predictive errors are important because:  

\[ \text{technology and:}^{20} \]

... risk assessment must be based on complex theoretical analyses such as fault trees, rather than on direct experience. Hence, despite an appearance of objectivity, these analyses include a large component of judgment. Someone, relying on educated intuition, must determine the structure of the problem, the consequences to be considered, and the importance of the various branches of the fault tree. In other words, the risk assessor must make a number of unavoidable, sometimes incorrect, epistemic value judgments.

Kahneman and Tversky warned that "the same type of systematic errors," often found in the epistemic or methodological value judgments of laypersons, "can be found in the intuitive judgments of sophisticated scientists. Apparently, acquaintance with the theory of probability does not eliminate all erroneous intuitions concerning the laws of chance."\(^{21}\) The researchers even found that psychologists themselves, who should know better, used their feelings of confidence in their understanding of cases as a basis for predicting behavior and diagnosing ailments, even though there was no correlation between their feelings of confidence and the correctness of the judgments.\(^{22}\)

\(^{19}\) Id., at 14-20.
\(^{20}\) Supra, note 7, at 463.
\(^{21}\) Supra, note 16, at 46.
\(^{22}\) S. Oskamp, Overconfidence in Case-Study Judgments, in Kahneman et al., supra, note 16, at 287-293.
Such revelations about the prevalence and causes of expert error are not totally surprising since, after all, the experts have been wrong before. They were wrong when they said that irradiating enlarged tonsils was harmless. They were wrong when they said that x-raying feet, to determine shoe size, was harmless. They were wrong when they said that irradiating women's breasts, to alleviate mastitis, was harmless. And they were wrong when they said that witnessing A-bomb tests at close range was harmless.\footnote{See K. SHRADER-FRECHETTE, NUCLEAR POWER AND PUBLIC POLICY, at 98-100 (1983).} For all these reasons it should not be surprising that psychometric analysts have found, more generally, that once experts go beyond the data and rely on value judgments, they tend to be as error-prone and overconfident as laypeople. With respect to technological risk assessment, psychometric researchers have concluded that experts systematically overlook many "pathways to disaster." These include: (1) failure to consider the way human error could cause technical systems to fail, as at Three Mile Island; (2) overconfidence in current scientific knowledge, such as that causing the 1976 collapse of the Teton Dam; and (3) failure to appreciate how technical systems, as a whole, function. For example, engineers were surprised when cargo-compartment decompression destroyed control systems in some airplanes. Experts also typically overlook: (4) slowness to detect chronic, cumulative effects, e.g., as in the case of acid rain; (5) the failure to anticipate inadequate human responses to safety measures, e.g., failure of Chernobyl officials to evacuate immediately; and (6) the inability to anticipate "common-mode" failures simultaneously afflicting systems that are designed to be independent. A simple fire at Brown's Ferry, Alabama, for example, damaged all five emergency core cooling systems for the reactor.\footnote{See K. SHRADER-FRECHETTE, NUCLEAR POWER AND PUBLIC POLICY, at 98-100 (1983).}
Lessons To Be Learned From Experts' Errors

What all these cases of expert errors indicate, and what the larger failure of the positivists' ideal of complete scientific objectivity suggests, is that we need to reform our policymaking regarding hazardous technology and environmental impacts. This restructuring needs both to protect us from the most dangerous consequences of expert error and to insure us that the laypeople most likely to be affected by a risk have a larger voice in making public policy regarding it. To accomplish this reform, we need to have minimum federal standards for risk abatement and pollution control. In addition, we need to move policymaking out of regulatory agencies and into procedures determined by citizen negotiation or, that failing, adversary assessment.25 Citizen negotiation and adversary assessment, however, presuppose that experts do not always give us the "right" or the "rational" answers about how safe is safe enough. Rather the public themselves must help decide the merits of alternative answers to questions about technological and environmental safety.

Talk about alternative answers, however, suggests that we need to reform technology assessment, environmental impact analysis, and risk management in at least three ways. First, instead of having experts perform a single study, we need to develop alternative technology assessments or environmental impact analyses, weighting them on the basis of different value systems and different epistemic or methodological value judgments. Second, we need to debate the merits of these alternative analyses, each with its own interpretational and evaluative weights. In this way citizens can decide not only what policy they want, but also what value systems they wish to guide their

25 For more information about adversary assessment, see supra, note 13, at Ch. Nine; for discussion of negotiation as related to technology/risk assessment, see K. SHRADER-FRECHETTE, RISK AND RATIONALITY, Ch. Thirteen (forthcoming).

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decisions. Third, in areas where assessors obviously have more technical knowledge, e.g., of probabilities, we need to weight expert opinion on the basis of past predictive successes. In other words, we need to calibrate the scientists and engineers who provide information relevant to policy choices. Let's examine some of the reasons for each of these moves.

Since no necessary connection exists between Pareto optimality (the central concept of benefit-cost analysis) or Bayesian rules and socially desirable policy, it would be helpful if there were some way to avoid the tendency to assume that economic methods or Bayesian rules, alone, reveal socially desirable policy. Alternatively weighted assessments would enable persons to see that sound policy is not only a matter of economic calculations but also a question of epistemological and ethical analysis, as well as citizens' negotiations.

Second, ethically weighted assessments would provide a more helpful framework for democratic decisionmaking. Weighted policy analyses could show how different measures of social risks, costs, and benefits might respond to changed value assumptions.

Third, because alternative, weighted analyses could explicitly bring epistemic and ethical value judgments into policy considerations at a very early stage of the process, citizens might be able to exercise more direct control over the values to which policy gives assent. To employ a system of alternative, ethically weighted analyses, among which policymakers and the public can decide, would be to recognize (1) that existing assessments already contain implicit ethical weights, and (2) that any proponent of a particular system of ethical and epistemological

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27 This point is emphasized by Pearce, supra., at 134.
weights ought to be required to plead her case, along with advocates of different value positions, in the public court of reason.

A fourth reason for using alternative, ethically weighted analyses is that it appears more desirable than the procedures likely to be adopted in its place, e.g., using what economists and risk assessors call "revealed preferences." Rather than assuming that past preferences (based on existing economic assumptions) are correct, using current weighting schemes allows the public to determine what its value and policy choices ought to be.

The purpose of introducing alternative, ethically weighted assessments, of course, would not be to provide a prescription for policy, but to allow the public and other decisionmakers to see how sensitive policy conclusions are to different evaluative assumptions. The work of Kneese et al. illustrates dramatically that what policy is said to be technically or economically feasible or unfeasible can change dramatically when different ethical weighting criteria are employed.

Some of the main practitioners of the method of revealed preferences include C. Starr, C. Whipple, and D. Okrent. See, e.g., Starr and Whipple, Risks, supra, note 4. See also Okrent, Comment on Societal Risk, 208 SCIENCE 374 (1980). See also C. STARR, CURRENT ISSUES IN ENERGY (1979), and D. OKRENT AND C. WHIPPLE, APPROACH TO SOCIETAL RISK ACCEPTANCE CRITERIA AND RISK MANAGEMENT ( PB-271-264 U.S. Dept. Commerce 1977). Although other means, e.g., the method of expressed preferences, of assigning measures to RCBA parameters have been discussed, I treat only the methods of market assignment of values and expressed preferences since these two dominate all current RCBA practice.

On the revealed preferences' scheme, society's current revealed preferences are determined on the basis of inductive inferences about what was allegedly preferred in the past. This means, for example, that if, in the past, society "accepted" X number of automobile fatalities per 100,000 miles driven, then (all things being equal, which they never are) society will accept the same level of fatalities now. Hence, on this theory, society "reveals" its preferences by means of past behavior which it tolerated.

For a critical perspective on the problems with the method of revealed preferences, see supra, note 6, at 34 ff.

Apart from the arguments for experimenting with different ethically weighted systems, there are a number of reasons for mandating that several alternative assessments be done for each project involving the imposition of public or societal risk. First, successful decisionmaking depends in part on knowing all the relevant facts and seeing all sides of a given "story." It is more likely that all sides to a story will be revealed if different groups, using different weights, do hazard analyses, than if only one project team performs only one study. The necessity of seeing different "sides" is borne out by wide divergences among the conclusions of different assessments of the same risk. Various studies of liquefied natural gas risks for Oxnard, California, for example, differed by three orders of magnitude.30

Second, consumers have a right to free, informed consent to the technological and environmental risks imposed on them, just as they have analogous rights in the area of medical ethics.31 If generating alternative analyses helps to insure that all sides to a policy controversy are heard, then it likely also helps to insure that public consent to risk is genuinely informed.

Third, all risks are value laden, and all hazard analyses employ judgmental strategies and epistemic or methodological value judgments. As a result, there are no wholly objective risk assessments. But if not, then technology assessment and risk analysis are in part subjective and likely to be highly politicized. And if so, then they ought to be accomplished in a political and legal arena where citizens and their representatives are able to recognize the consequences of alternative

(30 Unpublished report, to the National Science Foundation, Program in Ethics and Values in Science and Technology, August, 1979).
31 For an analysis of some problems with consent as related to technology/risk assessment, see supra, note 6, at Ch. Four.)
assessments. Citizens also need to be able to recognize which assessments are likely to give the most consideration to their interests.

Only if the naive positivists were correct in their belief (that there was a value-free algorithm guaranteeing theory choice) would it make sense to perform only one hazard analysis. Since they are not correct, and since "acceptable risk" or "reasonable policy" involves both procedural and ethical dimensions, as well as scientific ones, we need alternative assessments to capture the various ethical and political values informing public policy.32

On the new account of technology assessment and risk management that I want to defend, increasing the degree of analytical sophistication is not sufficient either for enfranchising the public or for resolving policy controversies. As environmental conflict over the proposed Cornwall facility demonstrated, expanded research efforts seldom are able to produce dispositive policy information. And if not, then decisionmakers must rely on procedural and democratic, rather than merely scientific, methods of assessing and managing risk.33

A fourth reason for performing alternative risk analyses, each with different methodological and ethical assumptions, is that there are numerous uncertainties in hazard assessment. Some of these uncertainties are evident in the wide margins of probabilistic error demonstrated in the Netherlands' study of WASH-1400, already mentioned.

In addition to preparing alternative assessments to represent different citizens' views, another methodological device for improving hazard analysis and technology assessment is to weight expert opinions. This suggestion amounts to giving more credence to experts whose risk

32 See supra, note 13, Ch. 9.
estimates have been vindicated by past predictive success. By weighting expert opinions, analysts would have a way to exercise probabilistic control over them. They could attempt to determine whether a hazard assessor (who provides a subjective probability for some accident or failure rate) is "well calibrated." (A subjective probability assessor can be said to be well calibrated if for every probability value \( r \) in the class of all events to which the assessor assigns subjective probability \( r \), the relative frequency with which these events occur is equal to \( r \).)

The primary justification for checking the "calibration" of technology and risk assessors is that use of scientific methodology requires "testing" the risk probabilities estimated by experts, especially since there is a wide divergence of opinion as to their actual values. In the famous WASH-1400 study of nuclear-reactor safety, for example, thirty experts were asked to estimate failure probabilities for sixty components, e.g., the rupture probability of a high quality steel pipe of diameter greater than three inches, per section-hour. On the WASH-1400 study, the average spread over the sixty components was 167,820. (The spread of these thirty expert opinions, for a given component, is the ratio of the largest to the smallest estimate.) In the same study, another disturbing fact was that the probability estimates of the thirty experts were not independent; if an expert was a pessimist with respect to one component, then there was a tendency for him to be a pessimist with respect to other components as well. Both the spread of expert opinions and their lack of independence suggest that it would be important to calibrate them, if we are interested in realistic technology assessment and policy analysis.

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34 Cooke, *supra*, note 15.
35 *Id.*, Ch. 2.
A Model for Methodological Improvement and Objectivity
in Technology Assessment and Risk Analysis:
Scientific Proceduralism

So far, I have argued that we ought to reform technology assessment and quantified risk analysis by using ethical and methodological weighting techniques, performing alternative assessments, and calibrating expert opinions. All of these methodological suggestions for improving technology assessment and risk management are predicated on two views: (1) that assessors ought to give up the rigid, anti-naturalistic, naive positivist assumption that expert assessments are wholly objective and value free, and (2) that contemporary technology/risk assessment needs to become more democratic, more open to control by the public, and more responsive to populist and procedural accounts of rational policy analysis and risk management.

What now remains to be established is how to safeguard scientific rationality and objectivity, even though technology assessment/risk assessment methods need to take account of democratic, procedural, ethical, and political factors, viz., populist factors allegedly not capable of being handled in purely rational and narrowly objective ways. Such a procedural account of technology assessment and hazard management presupposes that rationality and objectivity, in their final stages, require an appeal to particular cases as similar to other cases believed to be correct. This is what legal reasoning requires, just as Aristotle recognized, rather than an appeal only to scientific method.

The key to this procedural account of technology assessment, policy analysis, and risk management is Aristotle's belief that there are no explicit rules for ethical or value judgments. Instead we must use inexplicit and general rules to guide our moral reasoning. Aristotle believed that we came to know these inexplicit rules by relying on the ability of a group of people, similarly brought up, to see certain cases as
like others. This is also what Wittgensteinians are disposed to believe about all instances of human learning.

At the final level of technology assessment and risk management, Aristotle and Wittgenstein must be correct. Ultimately even rules must give way, not to further appeals to specific scientific rules, as the naive positivists and many scientists presuppose, but to a shared appreciation of similarities between cases.36

As such, this Popperian and Wittgensteinian account (scientific proceduralism) anchors objectivity to a legal, rather than a scientific, model of knowing and to a largely procedural, rather than substantive, account of rational assessment. Criticisms made by the scientific and lay community likely to be affected by a given risk, technology, or environmental impact would help to safeguard the procedural and democratic aspects of rational technology/risk assessment. Calibration of expert opinions and sensitivity analyses would help to safeguard its predictive and scientific components, viz., its rationality and objectivity.

This sketch of the scientific objectivity characteristic of a more populist notion of technology/risk assessment is premised on the Popperian assumption that open, critical, and methodologically pluralistic approaches (via citizen participation, alternative assessments, sensitivity analyses, calibration, and ethical/methodological weighting schemes) come closest to revealing the theoretical, linguistic, and cultural invariants of reality, much as a plurality of experimental perspectives helps reveal the invariants of quantum mechanical systems.

**Conclusion**

On the view that I am suggesting, what are the relevant variance principles applicable to technology assessment and hazard management?

These principles are that risk behavior or science policy is rational and objective if it survives criticism by various communities of citizens and experts, each with different transformations or evaluations of the same hazard. Arriving at rational policy, on this account, requires an epistemology in which what we ought to believe about technology assessment and risk analysis is bootstrapped onto how we ought to act. That is, we ought to act in ways that permit open criticism, that recognize due-process rights, that give equal consideration to the interests of all persons, and so on. Acting in this way, however, is a matter of realizing that the constraints or invariants in hazard analysis are in part realized through normative judgments. This is because the constraints must be implemented in institutional forms recognizing values such as equal treatment and informed consent. This means that rational assessment, on the view defended here, is irreducibly political in much the same sense that quantum mechanics is irreducibly statistical. But if so, then any account of rational technology assessment or policy analysis is as much a part of politics as science.

37 For a similar view, see D. G. Holdsworth, Objectivity, Scientific Theories and the Perception of Risk (Unpublished paper available from author at Ontario Hydroelectric, Toronto, CANADA.).

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