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Kristin S. Shrader-Frechette

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Kristin S. Shrader-Frechette*

Introduction

Until recently, the study of risk behavior — what some have called "motivated irrationality" — has suffered from the lack of attention of any single group of researchers. Brain scientists at the National Institute of Mental Health recently sought to remedy this deficiency. They gave evidence that those who voluntarily increase their individual (not societal) risk, like rock climbers and hang gliders, for example, have lower levels of the brain enzyme monoamine oxidase. They also have lower levels of a brain chemical called DBH, and higher levels of gonadal hormone.[1]

One problem with attributing risk taking simply to biochemical factors, however, is that many people delight in taking certain kinds of risks but detest others. American visitors to China, for example, are appalled at the high percentage of Chinese men who smoke, while most Chinese are surprised at Americans' chronic over-consumption of alcohol, especially when it is associated with one-half of all fatal automobile accidents and many types of violence.[2] How do we

* Professor Shrader-Frechette received her B.S. in physics from Xavier University and her Ph.D. from Notre Dame University. She is Graduate Research Professor of Philosophy, University of South Florida and Editor-in-Chief for the monograph series, ENVIRONMENTAL ETHICS AND SCIENCE POLICY.

1 Weiss, *How Dare We? Scientists Seek the Sources of Risk-Taking Behavior*, 132 SCI. NEWS 58 (1987).

2 *Id.* at 59.

explain such phenomena? Perhaps everyone employs subjective criteria for rational risk behavior. Or, perhaps the criteria are not subjective, but so complex that they are impossible to formulate.

At least one fact about responses to societal risk is certain: they are highly value laden. One highly evaluative response to societal risk is made by experts who subscribe to (what I call) the "Expert-Judgment Strategy".

The Expert-Judgment Strategy

Assessors who subscribe to the "Expert-Judgment Strategy" assume that one can always make a legitimate distinction between "actual risk" calculated by experts and so-called "perceived risk" postulated by laypersons.[3] They assume that experts grasp real, not perceived, risk, but that the public is able only to know perceived risk. This essay argues that all risk is *perceived,* even though there are criteria for showing why some risk perceptions are more objective or better than others. It argues that, although risk is not wholly *relative,* it is unavoidably "perceived." After showing what is wrong with the Expert-Judgment Strategy and the ethical consequences following from its use, the essay argues for an alternative approach to hazard evaluation and risk management. It describes a new, *negotiated* (rather than merely expert-based) account of *rational* risk management.

To understand more precisely what is meant by the Expert-Judgment Strategy, recall that one of the most fundamental sources of divergence,

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3 Many risk assessors employ this strategy, e.g., Cohen, Hafele, Okrent, Whipple, Jones-Lee, Rothschild and Morgan. *See, e.g.*, Whipple, *Nonpessimistic Risk Assessment* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, *supra* note 3, at 1112 (D. Paustenbach ed. 1989). *See also* Cohen, *Risk Analyses of Buried Wastes* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, *supra.* at 575. Some scholars, like Wildavsky and Douglas, also assume that, because risk is perceived, it is relative. *See* A. WILDAVSKY & M. DOUGLAS, RISK AND CULTURE (1982). For arguments against their view, *see* section 3 of this essay.

in technology-related risk assessments, is whether evaluation of specific hazards is in part a function of a real, probabilistically described risk, or whether hazard evaluation is wholly a product of the societal processes by which information concerning the danger is exchanged. More succinctly put, do people fear nuclear power, for example, because they assume that it is an inherently dangerous technology? Or do they fear atomic energy because they are victims of a sensational media campaign that has played on ignorance and paranoia about technology?[4]

The Expert-Judgment Strategy consists of the belief, either that risk can be reduced to some characteristic of a technology, determined only by experts, or that it is possible for experts alone to distinguish "actual risk," as a property of a technology, from so-called "perceived risk" postulated by laypersons. Once they make the distinction between perceived risk/real risk, many assessors assume that the perceived risks of laypeople are the source of most controversy over technology.[5] As a consequence, they ask how to mitigate the impact of *perceptions* about risk (perceptions they assume to be erroneous), rather than how to mitigate the impact of *risk* itself. They assume that public relations, or "risk communication," is their only problem.[6]

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4 E. LIEBOW & J. FAWCETT, SOCIOECONOMIC ASPECTS OF REPOSITORY-RELATED RISK PERCEPTIONS: A PRELIMINARY LITERATURE REVIEW 4, 6 (Battelle Human Affairs Research Center, July 16, 1987); *See also* Short, *The Social Fabric at Risk: Toward the Social Transformation of Risk-Analysis,* 49 AM. SOC. REV. 711 (1984). C. PERROW, NORMAL ACCIDENTS: LIVING WITH HIGH-RISK TECHNOLOGIES (1984).

5 *See supra* note 3.

6 *See, e.g.*, Whipple, *supra* note 3, at 1113; Liebow, *Letter to Kristin Shrader-Frechette,* (Batelle Human Affairs Research Center [hereinafter Batelle], July 17, 1987); Liebow & Fawcett, *supra* note 4, at 4, 6; E. LIEBOW & D. HERBORN, ASSESSING THE ECONOMIC AND SOCIAL EFFECTS OF PERCEIVING THE REPOSITORY AS "RISKY": A PRELIMINARY APPROACH, (Batelle, May 28, 1987). On August 6, 1987, Batelle researchers assembled experts from all over the U.S. to discuss the problems associated with mitigating the impacts of risk perceptions; *see* PACIFIC NORTHWEST LABORATORY, PNL 6515, ASSESSING SOCIAL AND ECONOMIC

The Expert-Judgment Strategy can also occur when assessors presuppose that a technological risk is defined purely in terms of *physical* impacts. For example, if researchers speak of the "social amplification of risk" as a process whereby hazards produce social impacts (e.g., fear) that exceed their health and safety effects,[7] then they appear to presuppose that the risk itself is purely physical and measurable by experts. On the contrary, however, it is arguable that social impacts (such as a decrease in civil liberties or uncertainties associated with the availability of adequate compensation, should a hazard occur) are part of the risk itself.

Some researchers who fall victim to the Expert-Judgment Strategy also speak, for example, of "perception-caused impacts" and of "*perception-induced* adverse impacts."[8] But there are no adverse impacts that are purely perception-induced unless one has completely uncontroversial measures of hazards (versus perceptions of hazards) and uncontroversial measures of risk impacts (versus impacts of risk perceptions). We have wholly exact measures for neither. What we have, instead, is a quantitative, "expert" definition of *risk,* as opposed to a qualitative, allegedly subjective notion of lay *risk perception.* Those who fall victim to the Expert-Judgment Strategy typically assume that risk can be defined purely probabilistically, as an average annual probability of fatality. They likewise assume that anyone (e.g., a layperson concerned about consent, equity, etc.) who does not subscribe to this purely probabilistic definition has an erroneous *risk*

EFFECTS OF PERCEIVED RISK (M. Nealey & E. Liebow eds. 1988).

7 *See* R. Kasperson, Emel et al., *Radioactive Wastes and the Social Amplification of Risk,* in WASTE MANAGEMENT '87: PROCEEDINGS OF THE SYMPOSIUM ON WASTE MANAGEMENT held Mar. 4, 1987 at Tucson, AZ 2 (available from Batelle); Liebow & Fawcett, *supra* note 4, at 4; Liebow & Herborn, *supra* note 6, at 3.

8 Liebow, *supra* note 6, at 1; Liebow & Fawcett, *supra* note 4, at 1. For an excellent discussion of the risk perceptions of the public, *see* B. WYNNE, RISK MANAGEMENT AND HAZARDOUS WASTE 10 (1987).

perception, rather than an accurate, alternative conception of *risk.*

It will not do to stipulatively define one type of risk (that of laypeople) merely as a misperception of another type of risk (that of experts), however, unless one has evidence for the claim that the latter is the correct definition. And the latter is a plausible candidate for a correct definition only if we can distinguish precisely and completely between the risks and perceptions of them. In subsequent paragraphs, I shall argue that, although some risk perceptions are more accurate or objective than others, it is *impossible* to distinguish between risks and risk perceptions, and hence not reasonable to subscribe to the Expert-Judgment Strategy.

All Risks are Perceived, Although they are not Wholly Relative

One can distinguish between *risks* and *risk perceptions* only if she is able to establish that a perception about a risk (and not the risk itself) caused a particular *impact.* Yet this is virtually impossible to do, for a number of reasons. For one thing, behavior has multiple causes, and sometimes neither a researcher nor the actor knows what those causes are. It is not enough to establish correlations between particular impacts, e.g., aversion to a particular danger, and specific risk perceptions. This would not show that the perceptions *caused* the alleged effects. Some other factor, perhaps a valuational one, could have caused the impacts. For example, there might be a correlation between catastrophic risks and the impact of high risk aversion. Yet this correlation might be accidental. Instead it might be the case that all catastrophic risks are also involuntarily imposed, and hence that the real cause of high risk aversion is the lack of control over it, not its catastrophic nature. If so, then it may be difficult to distinguish between impacts of risks and impacts of risk perceptions.

To understand why this distinction is problematic, consider another example. Suppose a person becomes an antinuclear activist, on the grounds that he is unwilling to take the chance that a commercial reactor accident will occur. Suppose also that he is reluctant to take this chance, because nuclear liability is limited to approximately one percent of the total possible losses, in the event of a catastrophe. The problematic question raised by this case is whether the impact, the person's activism, is caused by the real risk, e.g., the risk of not being compensated fully, or whether the impact, the activism, is caused by the *perceived risk,* e.g., the perception that, since the chances of accident are not small, the risk of noncompensation is not small.

If one argues that the *risk* causes the activism, then to mitigate this impact, the threat of noncompensation ought to be removed and the Price-Anderson Act (which limits nuclear liability) ought to be repealed. If one argues that erroneous *risk perceptions* (that noncompensation is likely) cause the activism, since a catastrophic accident is also probable, then mitigating the impact requires removing these allegedly erroneous perceptions. But one can remove the perceptions that noncompensation is likely only by guaranteeing full compensation should an accident occur. And one can guarantee full compensation only by repealing the Price-Anderson Act. Hence, whether risk or risk perception causes an impact, viz., lay aversion/activism, strategies for mitigating the impact are often the same. But this case suggests that it is often difficult to determine: (1) whether risks or risk perceptions *cause* a particular impact; (2) whether one can distinguish between *strategies* for mitigating impacts of risks, as opposed to impacts of risk perceptions; and therefore (3) whether one can distinguish *impacts* of risks from impacts of perceived risks. These difficulties raise the question of whether there may be another way to differentiate hazards from perception of them. There are at least eight reasons that suggest it may be impossible to differentiate risks from risk perceptions.

Before going into these eight reasons, it is important to emphasize that our inability to distinguish risks from risk perceptions does not force us into a position of *cultural relativism.* Even though all risks are perceived, many of them are also real. An analogy will illustrate this point. Death is real and death is certain. The *risk* of death, however, although equally real, is not certain, since it is in part a probability, and such probabilities can rarely be known with certainty. The *risk* of death is purely perceived, theoretical, or estimated *until* death becomes a certainty. Indeed the occurrence of death, in a particular case, reveals how accurate our perceptions or estimates of the risk of death were. But if this reasoning is correct, then (more generally) although all risks of some event, X, occurring are real, the exact degree and nature of these risks are not, in principle, confirmable until X actually occurs. Prior to this occurrence, risk perceptions can be judged as more or less accurate only on the basis of nonempirical and theoretical criteria like explanatory power, simplicity, internal coherence, etc. Nevertheless, risk perceptions are often *real* and *objective,* at least in the sense that empirical evidence, e.g., accident frequency, is relevant to them and is capable of providing grounds for amending them. This means that all risks (the probability *p* that some X will occur) are both *perceived* and *real.* Their exact nature and magnitude become more fully *knowable,* however, insofar as more instances of X occur. Douglas and Wildavsky erred in believing that, because all risks are *perceived,* therefore all risks are *relative.*[9] Their inference would hold, however, only if there were no ways to assess perceptions of risk. Since there are ways, perceived risks are not wholly relative. Risk perceptions can be assessed on the basis of rational criticism, in terms of their conformity with empirically observed frequencies/probabilities, and on the basis of their

9 WILDAVSKY & DOUGLAS, *supra* note 3.

predictive and explanatory power.

Now that we have sketched at least a partial basis for showing how one might argue that this account is not completely relativistic, let's examine at least eight reasons why actual risk is not typically distinguishable from perceived risk.

First, risk *probabilities* often do not reflect risk *frequencies*. This is in part because there are numerous difficulties of hazard estimation that do not admit of analytical, probabilistic resolution by experts. Often the risk problem is not well enough understood to allow accurate predictions, as the use of techniques like fault-tree analysis shows. Hence experts are forced to rely on subjective or perceived risk probabilities, instead of on actual empirical accident frequencies established over a long trial period. Even if assessors based their notions of *probability* on actual, empirical, accident *frequency,* this move would not always deliver their estimates of risk from the charge of being "perceived." Since there are reliable frequencies only for events that have had a long recorded history, use of historical accident/hazard data for new technologies likely results in an underestimating of the danger; this is because certain events may not have occurred between the inception of a technology and the end of the period for which the risk information is compiled. Moreover, low accident frequency does not prove low accident probability. Only when the period of observing accident frequency approaches infinity would the two, frequency and probability, converge.

A *second* reason why one cannot distinguish actual from perceived risk, in any wholly accurate way, is that actual risk estimates are always very *rough* and *imprecise.* The estimates typically vary from two to six orders of magnitude. Such imprecision is unavoidable, whether the estimates are based on probabilistic calculations or on actual experience. On the one hand, if they are based on probabilities, then assessors are forced to employ a number of value-laden theoretical assumptions and mathematical models. On the other hand, if the risk estimates are based on actual experience, or accident frequency, they are likewise "perceived" because probability does not equal frequency, as has already been mentioned, except for an infinite number of trials. Moreover, even actual frequencies do not provide a precise measure of a particular risk, because this number is always formulated as an *average* of a given group of frequencies, and such averages, by definition, do not take particular (and perhaps relevant, or site-specific) deviations into account.

Third, some of the most important aspects of hazards, whether real or perceived, are *not amenable to quantification.* What experts call "actual" risk estimates are based on the presupposition that risk is measured by probability and consequences, and that both can be quantified. Yet most laypeople would probably claim that what makes a thing most hazardous are factors that are not susceptible to quantification, factors such as a risk being imposed without consent, or being unknown, or posing a threat to civil liberties or to constitutional guarantees of due process.[10]

Fourth, whether it is perceived or (allegedly) actual, risk is a *theoretical concept,* not something capable of precise empirical prediction or confirmation. If it were empirically confirmed/determined, the hazard would be certain, not just a risk. In *general,* "risk" is defined in terms of expected utility theory and hence is a theoretical concept carrying with it all the baggage of this specific decision theory. In particular applications, "risk" is always defined on the basis of a whole

10 *See* VALUES AT RISK (D. MacLean ed. 1986). *See also* K. SHRADER-FRECHETTE, RISK ANALYSIS AND SCIENTIFIC METHOD 176 (1985). Finally, *see* Andrews, *Environmental Impact Assessment and Risk Assessment* in ENVIRONMENTAL IMPACT ASSESSMENT 85 (P. Wathern ed. 1988); and Cox & Ricci, *Legal and Philosophical Aspects of Risk Analysis* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, *supra* note 3, at 1017.

host of theoretical assumptions, many of which are often controversial. For example, a number of incompatible "cancer models" (dose-response models), each with attendant assumptions, has been used to estimate the incidence of tumors in populations exposed to formaldehyde. In 1987, the U.S. Environmental Protection Agency (EPA) called formaldehyde a "probable human carcinogen". The EPA said that those exposed to formaldehyde treated pressed wood could face a cancer risk, over 10 years, of 2 in 10,000. Experts at the Harvard School of Public Health, however, criticized the EPA risk assessment as premature and said the true formaldehyde risk was uncertain. Other expert groups, however, including scientists at the American Cancer Society and the Consumer Product Safety Commission, argued that the EPA models were incorrect, but in the opposite direction. They said EPA findings underestimated the cancer risk.[11]

The formaldehyde case, as well as those of EDB, dioxin, and methylene chloride, all illustrate that, even as late as the 1980's, particular accounts of risk are highly controversial and theory-laden. But if all risk is known in terms of the categories of a particular scientific or modeling *theory,* then there is no actual hazard, apart from some particular theoretical account of it. Hence there is no uncontroversial way to distinguish "actual" from "perceived" risk.

Fifth, because risk perceptions often affect risk probabilities, and vice versa, it is frequently impossible to distinguish hazards from perceptions of them. This fact is well known to social scientists as part of the account known as the "self-fulfilling prophecy." For example, if I

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[11] For discussion of the formaldehyde case, *see* Ricci & Henderson, *Fear, Fiat and Fiasco* in PHENOTYPIC VARIATION IN POPULATIONS 288 (A. Woodhead, M. Bender & R. Leonard eds. 1988). *See also* Paustenbach, *A Survey of Health Risk Assessment* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, *supra* note 3, at 38, and Gammage & Travis, *Formaldehyde Exposure and Risk* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, at 601.

perceive my chances of getting cancer to be high, then my perceptions can exacerbate stress and therefore increase the probability that actually do become a cancer victim. Hence there is often no clear distinction between actual and perceived risk.

Sixth, there are a number of reasons for arguing that the distinction between actual and perceived risk cannot be based on the alleged *objectivity* of *expert* estimates, as opposed to the alleged *subjectivity* of lay risk estimates. Admittedly, laypersons typically overestimate the severity of many technological hazards. However, even if it could be established that the public exaggerates the accident probabilities associated with some technology, e.g., liquified natural gas (LNG), this fact alone would be a necessary, but not a sufficient, condition for establishing the thesis that laypersons erroneously overestimate the severity of the LNG risk. Their risk perceptions could only be said to be erroneously high if they were *based solely on* incorrect accident probabilities. This is because, even though laypersons' perceived probabilities may be erroneous, they may not completely explain or determine their risk aversion. The public might view risks as high, not only because their accident probabilities are of a certain level, but also because their consequences are potentially catastrophic.

Moreover, apart from whether probabilities alone explain or dictate risk judgments, there is reason to believe that, at least in some areas, expert estimates of probabilities are not necessarily superior to those of laypeople. In their classic studies of the heuristic judgmental strategies that often lead to error in probability estimates, Kahneman, Tversky, and Oskamp concluded that experts were just as prone as laypeople to judgmental error regarding probabilities whenever they had merely statistical data.[12]

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[12] Tversky & Kahneman, *Belief in the Law of Small Numbers* in JUDGMENT UNDER UNCERTAINTY: HEURISTICS AND BIASES 23 (D. Kahneman, P. Slovic & A. Tversky eds. 1982); Kahneman & Tversky, *Subjective Probability* in JUDGMENT

Another common judgmental error of mathematically trained professionals is overconfidence; this occurs because experts' trust in their probability estimates is typically a function of how much information they have gathered, rather than a function of its accuracy or its predictive success. Since everyone, even those highly trained in probability and statistics, must make simplifying assumptions in estimating probabilities, and since experts are just as prone as laypeople to these judgmental errors, there is little reason to believe that experts are always able to calculate actual or *real* risk, while laypeople are merely able only to construct perceived or *subjective* risk.[13]

Seventh, those who attempt to distinguish "actual risk" and "perceived risk" are wrong to assume that the latter is merely an erroneous understanding of the former.[14] They are wrong because there is no universal definition of risk underlying the two concepts. For one thing, experts disagree as to whether (and when) to employ concepts such as "individual risk," "relative risk," "population risk," and "absolute risk".[15] Moreover, as was already suggested, the term 'risk' in "actual risk" and "perceived risk" has neither the same referent

UNDER UNCERTAINTY: HEURISTICS AND BIASES, *supra*, at 46. *See also* note 9, *supra.*

13 *See* note 11, *supra. See also* Oskamp, *Overconfidence in Case-Study Judgments* in JUDGMENT UNDER UNCERTAINTY: HEURISTICS AND BIASES, *supra*, at 287.

14 *See* Whipple, *supra* note 3, at 1112 and Cohen, *supra* note 3, at 575.

15 For various definitions of risk, *see, e.g.*, Cox, *Comparative Risk Measures* in PHENOTYPIC VARIATION IN POPULATIONS 233 (A. Woodhead, M. Bender & R. Leonard eds. 1988). For discussion of "relative risk," for example, *see* Ames, Magaw & Gold, *Ranking Possible Carcinogens* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, *supra* note 3, at 1083, Layard & Silvers, *Epidemiology in Environmental Risk Assessment* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, at 159; Harley, *Environmental Lung Cancer Risk from Radon Daughter Exposure* in THE RISK ASSESSMENT OF ENVIRONMENTAL AND HUMAN HEALTH HAZARDS, at 620; and Cohen, *supra* note 3, at 574.

nor the same meaning. What Hafele, Okrent, Jones-Lee, Morgan, and others call "actual risk" is the probability of a particular hazard occurring, times the magnitude of its consequences. Typically this is expressed as an average annual probability of fatality, where fatality is the consequence associated with a particular risk. What they call "perceived risk," alleging that it is an incorrect view of actual risk, however, is not merely (an incorrect) *perception* of probability times consequences. Rather, most laypeople would claim that what typical risk assessors call "perceived risk" includes *more* than mere probability. Hence when laypeople say that something is a "high risk," they do not necessarily mean only that it has a high probability of causing death. And if so, then "actual risk" is not mere probability times fatality, and "perceived risks" are not merely perceptions of probability times fatality.

Eighth, there is no distinction between perceived risks and actual risks because there are no risks except perceived risks. If there were hazards that were not perceived, then we would not know them. Because we know them, in some sense, proves that even real risks are perceived, even real risks must be known via categories and perceptions. This is related to the earlier point that all risks are, *in part,* theoretical constructs, not completely empirical, not wholly capable of confirmation. All risks are defined, filtered, and judged on the basis of some subjective standard, whether it is expected utility theory or benefit-cost analysis, or something else. No notion of what is hazardous is without theoretical baggage, and if not, then all risks are perceived.

Apart from these eight reasons for rejecting the distinction between actual and perceived risk, it is important to point out that acceptance of a sharp distinction between real risk, as probability times accident consequences, and perceived risk, as an incorrect view of probability times consequences, leads to at least two undesirable *consequences.* One negative effect is that the distinction misidentifies the source of divergence in risk judgments and misattributes this disagreement to accurate, versus inaccurate, knowledge of probability times accident consequences. Hence it provides policy makers with little insight on controversy over hazards, little basis for investigating the important ethical, methodological, and evaluative reasons for divergent risk judgments.

Moreover, if policy makers assume that there is an unbridgeable gap between lay/perceived and expert/real risk, then there is less reason for them to take account of lay views, since error could be said to have no rights. Hence the Expert-Judgment Strategy could lead to disenfranchising democratic decision makers.

But if accepting a sharp distinction between risk and risk perception is both politically dangerous and epistemologically confused, then what follows? It makes no sense to talk about risks versus perceived risks, as if experts had some magic window on reality. Instead we must deal with all hazards as they are perceived, even though (as was just argued) they are not purely relative. We must focus on disputes over risk perceptions and attempt to ameliorate them and the controversy surrounding them.

The U.S. Environmental Protection Agency Versus the Utilities

But if we ought to deal with all risks as risk perceptions, and not assume that experts alone somehow have access to real or unperceived risks, then we might rely on what Liebow and Herborn call an "institutional memory," in order to learn how to use past conflicts as a basis for resolving current risk controversies.[16] One case is particularly instructive in this regard. It has been the nation's most celebrated, lengthiest, and perhaps most costly environmental controversy.

The conflict began in 1964 and was between the EPA and five New

[16] Liebow & Herborn, *supra* note 6, at 4.

York utility companies. The controversy focused on the potential environmental impacts of Con Ed's proposed Cornwell Project, a pumped-storage facility to be built on a mountain overlooking the Hudson valley. At the focus of the debate was the impact of the facility's water withdrawals on the Hudson River striped bass population. As several authors in BIOSCIENCE put it:[17]

> [T]he Hudson River controversy was a unique test of the ability of biologists to use their science to aid public decision makers in achieving an equitable solution to an important environmental problem.... After more than a decade of study and the expenditure of tens of millions of dollars, it was still not possible to draw definitive conclusions about the long-term effects of entrainment and impingement on fish populations in the Hudson River. We do not believe that this failure can be blamed on lack of effort, on the incompetence of the biologists involved, or on the use of the wrong model. We believe that it occurred because of insufficient understanding of underlying biological processes [even though,]... in the Hudson River controversy, the scientific issues were more clearly defined, and the research effort greater, than for any other major environmental dispute known to us.

The settlement of the Hudson River case, negotiated between the EPA and the five companies, called for the utilities to deviate from the outage schedule, provided that the overall degree of mitigation of impacts was not reduced. The credit allowed for shutting down a specific generating unit during a given week was determined by the contribution of that unit to the conditional entrainment mortality rate for striped bass.

What scholars learned (from tens of millions of dollars of research spent on biological processes that were impossible to define in the Hudson-River case) is that sometimes science, even science regarding

[17] Barnthouse et al., *Population Biology in the Courtroom: the Hudson River Controversy*, 34 BIOSCI. 17 (1984).

risk perception and behavior, is often at an impasse. In the Hudson-River case, a resolution was reached only after all affected parties decided *not* to try to define particular impacts and ascertain their causes. Likewise, in the risk perception case, I suspect that a resolution of controversy will occur only after people stop trying to distinguish perceived risk from real risk. Aristotle astutely noted that the wise person recognizes the degree of certainty appropriate to particular types of inquiries, and that she does not seek a level of certainty inappropriate to the specific kind of investigation.

If Aristotle's insight is applicable to the question of hazard evaluation and management, then perhaps assessors ought to stop the interminable haggling over which persons are correct in their risk judgments, the experts or the public. Instead, perhaps we and they, the experts, ought to shift our focus, in part, from the *scientific* to the *ethical* dimension. We ought to attempt to formulate a *procedural* or *negotiated* solution as a means of solving the problem of defining, evaluating, and managing risks.

Negotiating Risk Solutions: Arguments for Consent and Compensation

If an appropriate risk-evaluation strategy is to have persons negotiate about their alternative definitions of risk and their different value judgments concerning hazards, rather than to have them simply assess the predictive or explanatory power of their risk evaluations, or merely follow the Expert-Judgment Strategy, then there is an obvious question. How does one negotiate among persons so as to resolve some of their conflicts about acceptable risk? E. Peelle, sociologist at Oak Ridge Laboratories, has argued that creating a citizens' task force to specify mitigation, compensation, and incentive measures for hazardous technologies could help resolve controversies over risk. In Tennessee, for example, such task forces have already succeeded. The "net local balance" of the Department of Energy's proposal to site a Monitored Retrievable Storage facility in the state changed from negative to positive after a citizens' task force was organized.[18]

What the Tennessee experience suggests is that there are both pragmatic and ethical grounds for rejecting the Expert-Judgment Strategy. Instead we ought to follow at least two mandates of risk management, both consistent with the negotiated settlement in the Hudson-River case. As an alternative to completely rejecting lay perceptions of risk, we ought to (1) explicitly recognize the necessity of free, informed *consent* to risk, and we ought to aim at obtaining free, informed consent to all risks, regardless of how they are perceived or defined and (2) negotiate so as to provide full *compensation* for all risk-bearing and for all risk imposition. That is, we ought to provide compensation consistent with potential victims' perceived hazards, not merely compensation dictated by following the Expert-Judgment Strategy.

Guaranteeing Consent

Regarding consent to risk, we ought to recognize that, apart from how experts define hazards, their imposition is ethically justifiable only if the persons affected by them have given free, informed consent. But if this is so, then the obvious analogue for hazard evaluation and management is medical ethics; a physician is ethically justified in imposing a possibly nontherapeutic risk on a patient only after she or her representative has given free, informed consent to the imposition. Even if the patient is no expert and has faulty risk perceptions, she retains the right to consent to the risk. The doctor cannot make that

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[18] E. Peelle, The MRS Task Force: Economic and Non-Economic Incentives..., (unpublished manuscript based on lecture given at Waste Management '87 Conference held Mar. 4, 1987 at the University of Arizona); *see also* note 18, *infra* and Andrews, *supra* note 10, at 85.

decision for her.[19]

The *pragmatic* justification for recognizing the ethical requirements of free, informed consent, and then negotiating regarding compensating persons for consenting to higher risks, is that such recognition defuses opposition about the level of the risk imposed and about the justification for it. It helps resolve conflict about perceived risk, controversy that is often generated by employing the Expert-Judgment Strategy. Such opposition is defused because the necessity of providing a "risk package" (including compensation and risk-mitigation agreements), to which potential victims will give consent, drives both proponents and opponents to work toward a negotiated agreement. Admittedly, opponents of taking a particular technological risk may view this negotiated consent as a way to coopt them, and proponents of taking the risk may see it as an expensive way to buy agreement. Nevertheless, there are both ethical and pragmatic grounds for attempting to negotiate consent.[20] Not to pursue an agreement is to be forced either into curtailing technological progress or into a situation in which someone, other than all those affected, makes risk evaluations and decides risk policies.

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[19] Examples of important essays on risk and consent are included in VALUES AT RISK, *supra* note 10; Cox & Ricci, *supra* note 10, at 1017; and in HAZARDS: TECHNOLOGY AND FAIRNESS (R.Kates, A. Weinberg et al. eds. 1986). Examples of classic essays on informed consent and medical experimentation include Jonas, *Philosophical Reflections on Experimenting with Human Subjects*, Curran, *The Tuskegee Syphilis Study* and Barber, *The Ethics of Experimentation*, all in MORAL CHOICES (P. Rieff & I. Finkle eds. 1977). *See also* Freund, *Ethical Problems in Human Experimentation*, Beecher, *Ethics and Clinical Research* and Havighurst, *Compensating Persons Injured in Human Experimentation*, all in READINGS ON ETHICAL AND SOCIAL ISSUES IN BIOMEDICAL ENGINEERING 16 (R. Wertz ed. 1973).

[20] *See also*, *supra* note 17; E. Peelle, Hazardous Waste Outlook (unpublished manuscript for TVA and U.S. DOE, contract No. DE-AC05-840R21400); Peelle, Carnes et al., *Incentives and Nuclear Waste Siting* 7 ENERGY SYS. AND POL'Y. 323 (1983).

Guaranteeing Compensation

Regarding compensation for risk, we ought to recognize that, apart from how experts define a particular risk, its imposition is ethically justifiable only if the persons affected by the hazard are compensated for the danger they face. Moreover, the compensation must be commensurate with the potential victims' perception of the hazard, not merely with an expert's judgment. The justification for requiring compensation includes both due process and equal protection notions from our ethical/legal traditions. Equal protection requires, at a minimum, that when society sanctions placing some persons at higher risks than others, those disadvantaged deserve compensation. Moreover, if some persons bear a risk associated with benefits received by others not bearing the risk, e.g., by living near a toxic waste dump, equity demands that those facing the greater danger have the right to compensation.[21]

The pragmatic grounds for compensation are closely related to contemporary and neoclassical economic theory. These are: (1) that contemporary benefit-cost analysis presupposes that the gainers compensate the losers, if a transaction is to be economically efficient. The compensation rule simply moves the concept of "money changing hands" from the level of economic theory to that of practical dispute resolution; (2) that compensation for those facing special technology-related hazards is consistent with current market mechanisms for compensating those who bear risks, e.g., those endangered by noise because they live near airports; and (3) that risk compensation for those living near a waste facility, for example, is also consistent with the economic theory of the compensating wage differential. On that theory, imposition of a higher workplace risk is justified, in part, if those

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21 For arguments to this effect, *see* J. THOMSON, RIGHTS, RESTITUTION AND RISK (1986). *See also* Carnes et al., *Incentives in Nuclear Waste Siting* in RESOLVING LOCAL CONFLICT 354 (R. Lake ed. 1987). *See also* O'Hare, *Not on My Block You Don't*, 25 PUBLIC POL'Y. 407 (1977).

bearing the additional risk give free, informed consent to the risk, and if they receive higher wages proportional to the hazards they face.[22]

Admittedly, it may be the case that the ethical and social costs associated with risks like those from hazardous waste facilities are not compensable.[23] If they are, then compensation is pragmatically desirable because it addresses equity problems and is likely to help eliminate opposition to the facilities and resolve conflict.[24] If the risks posed by threats like hazardous waste facilities are not compensable, then society must decide whether they are avoidable. If they are avoidable, and not "necessary risks,"[25] then well and good: society can forego the benefits obtained through such hazards. If the risks are not avoidable and not compensable, then society must develop an equitable scheme for distributing the risk; at least part of such an equitable scheme is likely to involve some form of compensation, since society sanctions the imposition of harm, and the harm is unavoidable.[26]

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22 For discussion of compensation for risks, *see* Part 2 of HAZARDS: TECHNOLOGY AND FAIRNESS (R. Kates, A. Weinberg et al. eds. 1986). *See* K. S. SHRADER-FRECHETTE, *supra* note 10, at Chapter 4, for a discussion of the compensating wage differential. *See also* K. S. SHRADER-FRECHETTE, SCIENCE POLICY, ETHICS AND ECONOMIC METHODOLOGY (1984), for discussion of benefit-cost analysis and its application to environmental policy making.

23 *See* Bacow & Milkey, *Overcoming Local Opposition* in RESOLVING LOCAL CONFLICT, *supra* note 21 at 164.

24 *Id.*

25 *See* Samuels, *The Arrogance of Intellectual Power* in PHENOTYPIC VARIATION IN POPULATIONS 118 (A. Woodhead, M. Bender & R. Leonard eds. 1988) for discussion of "necessary risks".

26 For further discussion of risk compensation, *see* Cox & Ricci, *supra* note 10, at 1017–1046; S. CARNES, ET AL., INCENTIVES AND THE SITING OF RADIOACTIVE WASTE FACILITIES, (Oak Ridge National Laboratories, ORNL/5880, 1982); Kunreuther et al., *A Decision-Process Perspective on Risk and Policy Analysis* in RESOLVING LOCAL CONFLICT, *supra* note 21, at 260, 270; R. MCMAHON ET AL., USING COMPENSATION AND INCENTIVES WHEN SITING HAZARDOUS WASTE MANAGEMENT FACILITIES (U.S. Environmental Protection Agency, SW 942, 1981);

Removing Liability Limits

Along with compensating those affected by technological risks, there are also ethical and pragmatic grounds for not limiting liability, in the event of a catastrophic technological accident. The case of nuclear power provides a good example of how the energy controversy is fueled by the Price-Anderson Act, the current nuclear liability statute. As mentioned earlier, this law limits the liability of nuclear power plant owners to $640 million, with the consequence that many victims of a possible catastrophic accident might not be compensated for all their health costs and property losses. Since the U.S. Brookhaven Report estimated that property damages alone, typically only about one-fourth of total accident costs, could go as high as $17 billion for a catastrophic fission accident, this means that nuclear utilities are liable for only about one percent of total possible losses, in the event of a catastrophe.[27] Even the $17 billion figure may be too low, when one realizes that Chernobyl, which caused few immediate fatalities, will cost about $10 billion to cleanup.[28]

If such damage figures are accurate, then (apart from due process and equal protection arguments) there are strong pragmatic grounds for risk managers to rid policy of the nuclear "insurance asymmetry." This asymmetry consists of the fact that, although laypeople can be insured against plane crashes, surgeon malpractice, and a whole host of technology-related dangers, their homeowners' policies have exclusion

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O'Hare & Sanderson, *Fair Compensation and the Boomtown Problem* in RESOLVING LOCAL CONFLICT, *supra*, at 376 (R. Lake ed. 1987) and H. Raiffa, THE ART OF SCIENCE AND NEGOTIATION (1982); G. ROCHLIN, THE ROLE OF PARTICIPATORY IMPACT ASSESSMENT IN RADIOACTIVE WASTE MANAGEMENT PROGRAM ACTIVITIES (Institute of Governmental Studies Report No. IGS/RW-002, University of California, Berkeley 1980); Sandman, *Getting to Maybe* in RESOLVING LOCAL CONFLICT, *supra*, at 333.

27 K. S. SHRADER-FRECHETTE, NUCLEAR POWER AND PUBLIC POLICY 78 (1983).

28 Silberman, *Risky Business: Congress Debates Nuclear Insurance*, Not Man Apart, May–June 1987, at 1.

clauses governing harms caused by atomic energy. All such risk asymmetries need to be removed. If public officials expect laypeople to believe them, when they say that something is safe, then they ought to act as if it were safe, and provide full liability. After all, there would be no danger in guaranteeing full coverage, if indeed the likelihood of a catastrophe were quite small. The public reasons: Only if such probabilities are not low is there a basis for the government not to require full liability for all societally imposed risks.[29]

There is another pragmatic argument for negotiating consent and compensation, as a procedural alternative to accepting the Expert-Judgment Strategy. This is that, if proponents of risky technologies are interested in winning acceptance for their point of view, then they ought to leave no obvious targets for their opponents to attack. Otherwise public controversy over safety will never cease. Some of the obvious targets in the contemporary risk debate include policy makers' tendencies to give uncritical acceptance to expert definitions of risk; to support risk imposition before negotiating consent and compensation agreements with those likely to be affected by the hazard; and to sanction government-imposed liability limits for some of the accidents most feared by the public.

If one wants to win acceptance for a risky technology, then it ought to be assessed, evaluated, implemented, and managed in as uncontroversial a way as possible. Welfare economists discovered an analogous insight several decades ago. Seeking to win support for the controversial conclusions of their benefit-cost analyses, they realized that they needed to avoid any questionable assumptions that were not essential to their method, e.g., the assumption that the value of a human life can be measured by discounted future earnings. Just as techniques like discounted future earnings jeopardize the success and acceptance of

29 Shrader-Frechette, *supra* note 27, at 73.

benefit-cost analysis, so also assumptions like the Expert-Judgment Strategy jeopardize the success and acceptance of important, but risky, technologies.

Conclusion

If any single lesson can be learned from the arguments of this essay, it is that experts do not always know best. This is particularly the case if the problem at issue, distinguishing from risk perceptions is not wholly amenable to theoretical resolution. Like successful adjudication of the Hudson-River controversy, adjudicating conflicts over hazard evaluation often requires that we supplement the *theoretical* task of *defining* the risk problem in some purely *scientific* way. Yet, we need also to focus on the *practical* task of *negotiating* its resolution in a *procedural* way.