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Paul B. Thompson

RISK OBJECTIVISM AND RISK SUBJECTIVISM: WHEN ARE RISKS *REAL?*

Paul B.Thompson*

Introduction

In introducing a session on "The Risks We Run and the Risks We Accept," at a 1979 conference of risk analysts, Chauncey Starr characterized the risk judgments of the great mass of citizens in the following way:[1]

> Their perceptions may be so far from reality that you and I know that they're absurd, but that's how they feel about it and that's the way they perceive things. So, in discussing the subject [of risk], we really have to distinguish between the reality of what may or may not occur, the analysis of it,

* Professor Thompson received his B.A. from Emory University (1974) and his M.A. (1979) and Ph.D. (1980) both from State University of New York at Stony Brook. He is Associate Professor of Philosophy and Agricultural Economics at Texas A&M University.

1 C. Starr, *Introductory Remarks in* SOCIETAL RISK ASSESSMENT: HOW SAFE IS SAFE ENOUGH? 4 (R. C. Schwing and W. A. Albers eds. 1980). In the same volume, W. W. Lowrance essentially rejects Starr's formulation of the problem by characterizing analysis as a form of perception that differs from the laypersons primarily in being "subjectively endorsed by the scientific community" in *The Nature of Risk*, at 6-7. Lowrance's approach converts Starr's concern for getting clear about real risk into a public policy issue regarding the political authority of scientists, rather than a philosophical and methodological problem about the nature of risk. This strategy for thinking about the policy problem is pursued in W. W. LOWRANCE, SCIENCE, TECHNOLOGY AND PUBLIC POLICY (1986) and by Raynor and Cantor, *How Fair Is Safe Enough?: The Cultural Approach to Technology Choice*, 7 RISK ANALYSIS 3 (1987). A more cynical statement of the view is found at Tiemann, *Risk, Technology and Society*, 7 RISK ANALYSIS 11 (1987). This contrasting view cannot be profitably pursued in the present context without rather elaborately complicating matters that are already complex.

and our perception of it.

Starr's opinion was (and is, I think) widely shared by risk analysts, and others who hope that risk assessment can fulfill its initial promise to provide objective, scientific information on a wide range of social and technological risks. There is a bit more to the view than "providing information," however, for in these casually worded comments, Starr presumes a concept of risk that challenges traditional approaches in law and public policy.

A recent article[2] documents the shift from traditional risk concepts to probabilistic concepts in the law. The traditional approach has been to use the judgment of the reasonable person as the paradigm for identifying risk. If a reasonable person would have judged an act to be risky, it was risky, even if harm did not materialize as a result. Drunken driving and reckless endangerment, for example, are violations of law, even in the absence of actual harm. Conversely, there are unforeseeable harms that can be caused by even the most unexceptional acts — walking down stairs or boiling peas on the stove. If these activities are to be regarded as part and parcel of the normal activities of a reasonable person, then there will be no basis for a finding of responsibility (exceptions abound, I am aware) for the oddball, unfortunate outcomes that do occur.[3] The probabilistic approach to risk takes it that risk is an omnipresent, mathematically measurable possibility of harm that applies to natural events as well as human actions. Because the probabilistic concept identifies risk by measuring the probability and value of events, rather than by examining analogies to paradigm cases, risk can be seen as reality that yields its secrets to scientific enquiry.[4]

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2 Schroeder, *Rights Against Risk*, 86 COLUM. L. REV. 495 (1986)

3 J. J. THOMSON, RIGHTS, RESTITUTION AND RISK, 225-250 (1986); P. Thompson, *Collective Action and the Analysis of Risk*, 1(2) PUBLIC AFFAIRS Q. 23 (1987).

4 *See* Starr, Rudman and Whipple, *Philosophical Basis for Risk Analysis*,

A key application of this view is in law and regulatory policy, where the new probabilistic concept can be used to supplant more traditional concepts.[5]

Starr's distinctions between reality and perception have implications for law and policy because the probabilistic concept of risk provides a new basis for identifying and assessing risk. Traditionally, the principal criterion for the riskiness of an action has been the judgment of a archetypal "reasonable person," but it is precisely the judgment of the typical person that Starr wants to set aside as mere perception. Real risk is analyzed by science; perceived risk is the opinion and judgment of the common man. The new view substitutes the analytic of science for the fallible judgment of the ordinary mortal; but the ordinary mortal and the reasonable person are one and the same. The new view makes the explicit claim that scientific research provides a superior basis for reliable prediction than does the uninformed judgment of the reasonable person, but it also makes the implicit claim that rigorous interpretation of probability theory yields a superior *conceptual framework* for understanding risk issues. While the first of these claims is surely true, the second is not. Rejecting this second claim does *not,* however, entail that probabilistic data should never be used in the regulation of technological hazards, nor that people who have a good grasp of probabilities and statistics are never equipped to make better decisions on risks than people of comparable intelligence who lack such knowledge. It *is* the case, on the other hand, that the way such knowledge is to be applied to the analysis and management of risk is a

1976 ANN. REV. ENERGY 1; E. P. O'DONNELL, A PROPOSED APPROACH TO THE ESTABLISHMENT AND USE OF QUANTITATIVE SAFETY GOALS IN THE NUCLEAR REGULATORY PROCESS (The Atomic Industrial Forum, Comm. Reactor Licensing and Safety 1981).

5 See Schroeder, *supra* note 2; Mayo, *Increasing Public Participation in Controversies Involving Hazards: The Value of Metastatistical Rules,* 10(4) Science Tech. Hum. Values 54 (1985).

matter of more subtly than is often thought. This conclusion is related to work that takes risk to socially constructed rather than "real" in any robust sense,[6] but that is far ahead of the subject at hand.

The Reality of What May or May Not Occur

The parlance of "real" or "actual" risk is often employed in one of three problem contexts. First, it is useful in explaining how the physical and biological sciences are brought to bear upon problems of risk.[7] These sciences study the world as it is. If they are to study risk, the focus will be upon "the reality of what may or may not occur." Second, once the role of the natural and engineering sciences has been established, it may be useful to make a methodological distinction between physical and biological sciences, on the one hand, and the

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6 Risk analysis as Starr and others have defined it, takes events to be the object of inquiry; one has an analysis of risk when one identifies a class of potentially harmful events and makes an assessment of their probability, relative to some item of interest. In taking risk to be an act sortal, risk is less a discoverable property of events than a concept for sorting acts into categories that may be of cultural origin and have oblique purposes (when, indeed, they have any purpose at all). Mary Douglas has, at times, seemed to advocate such a view; M. DOUGLAS AND A. WILDAVSKY, RISK AND CULTURE (1982). I have discussed it in *Collective Action and the Analysis of Risk*, *supra* note 3. See also Lowrance, 1980; and Raynor and Cantor, 1987, both *supra* note 1.

7 Risk analysis emerged in the decade of the 1970's as a multidisciplinary enquiry that employed engineering and natural science methodologies in the service of measuring, predicting, and managing a large class of events that were presumed to have physical and biological causes as their basis. The natural science basis of these techniques distinguished them from actuarial, economic or financial risk analysis. The positivist bent of the physical, biological, and engineering scientists who were pioneering this new field led them to think of themselves as studying reality, and to think of the old actuarial risk analysts as studying some nonnatural phenomenon. Starr, Rudman and Whipple, 1976 *supra* note 4, at 640-642, use the term "real risk" in just this way. More typical is W. D. ROWE, AN ANATOMY OF RISK (1977), who lays out the framework for natural science enquiry into the probability of unwanted consequences as the main task of risk analysis but does not use the terms "real" or "actual." Rowe's general framework is carried into more recent discussions by Wilson and Crouch, *Risk Assessment and Comparison: An Introduction*, 236 SCIENCE 267 (1987).

social sciences (including law and philosophy), on the other. In Starr's quote the word "analysis" refers back to "the reality of may or may not occur." Risk analysis is the analysis of a reality. Social science will be useful, of course, because we also need to understand the perception of risk. We need to understand perception, however, not because it gives us any insight into the reality of risk, but because that reality may be, as Starr intimates, rather unrelated to how people behave. *Real risk* is the object of natural science inquiry; *perceived risk* is the object of social science inquiry.

There is a third use of the expression "real risk" that, at first glance, appears to be unrelated to the previous two. We use the word "real" to express norms. Real risks are the risks we ought to pay attention to, as opposed to imaginary or hysterical risks. We want the real thing, and we want it as real as it gets. So, often to identify something as "real" is to make a prescription: this is the item on which judgment should be based, on which action should be taken. In fact, this normative sense of "real risk" is generally related to the previous two, and it is clearly implied by Starr's remark. The view he has expressed is that the natural scientist's analysis of risk ought to be the authoritative basis for regulatory decision making.[8] It ought to be authoritative because it is the one that addresses reality, rather than perception. Let reality be our

8 The Starr quote is typical of this view in that perceived risk is roughly equated with unscientific, hence poorly informed, attitudes held by members of the general public. Risk analysts recognize that these perceptions are likely to be widely shared, and that they influence policy in a wide range of cases, but many would also insist that such policy choices be made in full cognizance of the "unreality" of public beliefs. *See* Starr, Rudman, and Whipple *supra* note 4; Starr and Whipple, *The Risks of Risk Decision*, 208 SCIENCE 1114 (1980); and Ruckleshaus *Science, Risk and Public Policy*, 221 SCIENCE 1026 (1983). Ruckleshaus modified his views somewhat in *Risk in a Free Society*, 4 RISK ANALYSIS 157 (1984). The issue leads naturally to the discussion of "risk communication," conceived as a dialogue between perception and reality. *See* Otway, *Experts, Risk Communication and Democracy*, 7 RISK ANALYSIS 125 (1987).

guide. Now, one might be suspicious of the way that Starr has so concisely and unpretentiously collapsed descriptive and normative senses of the word "real," but perhaps it comes down to little more than saying that we are obligated to act upon the true facts, so far as we can tell what they are. This is surely an unexceptionable assumption, as far as it goes, and I will not question it. It is a somewhat different presumption that creates the mischief, one having to do with the way that the true facts are distinguished from other factors. For our purposes, this distinction appears in the further assumption that risk management can be divided into an assessment phase, in which real or actual risk is the object of analysis, and an acceptance phase in which perceived risks are accommodated through the political process. On at least one version of this view, assessment is entirely a matter of assembling and quantifying facts, while acceptance involves the application of values and decision rules to the technically derived factual picture of what the real risk is. Studies of perceived risk are thought to provide insight into the values and preferences that might be employed in making a judgment of acceptability, but neither studies nor the perceptions themselves figure in identifying the real risk itself.[9] In this classically positivist account of the philosophy of natural science, real risk is a matter of how things stand in the world, while perceptions are reactions to the world, affecting reality only indirectly in virtue of their influence on human action. If this view is to be of any help in regulatory policy, the difference between the scientific risk analysts' view of "how things stand in the world" and the reasonable person's view must consist simply in the scientist having a more accurate account

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9 *See, e.g.*, Rowe, *supra* note 7 at 28-43. *See also* Otway, Linnerooth, and Niehaus, *On the Social Aspects of Risk Assessment, in* RELIABILITY ENGINEERING AND RISK ASSESSMENT (J. B. Fussell and G. R. Burdick, eds. 1977); and Firebaugh, *Public Attitudes and Information on the Nuclear Option,* 22(2) NUCLEAR SAFETY, 147 (1981). Former Environmental Protection Agency Administrator William Ruckleshaus committed himself explicitly to this view in 1983; *see supra* note 8.

of the facts. If there are conceptual differences in their view as well, it is not at all clear that the "values" of the reasonable person can be unambiguously applied to the "facts" of the scientific risk analyst.

Characterizing the Problem

The philosophical problem might be pursued from one of two starting points: we might begin by asking whether a metaphysical theory of risk is possible,[10] or we might try to get clear about some prototypical cases for calling a risk "real," and then see whether there is a philosophically defensible concept that matches them in a satisfactory way. It is the second approach that promises to get at the main problem most directly, and it is this virtue that recommends it here. We begin by considering some test cases.

Consider two stories, two fictional cases, if you will, about the possibility of a rattlesnake residing in your mailbox. The first story is recounted to you by the seven year old boy who lives in the house next door. He tells you that a voodoo priest has put a spell on your house and that it may have caused the letters put in your box to be transformed into an angry rattlesnake. The second story is recounted to you by the rational adult who lives in the house next door. She tells you that vandals have been up to a particularly odious form of mischief, stuffing live rattlers into neighborhood mailboxes, two having been found so far. Now consider the question: Is it risky to open the mailbox in your usual fashion? The first story about the voodoo priest is entirely incredible, and if that is prelude to the question, the answer would seem to be "No." The second story, however, is quite credible, and if it is this one that precedes the question, the answer would appear to be "Yes." There is, thus, something about the relationship between the

10 N. RESCHER, RISK: A PHILOSOPHICAL INTRODUCTION TO RISK ANALYSIS (1983); *cf.* P. Thompson, *The Philosophical Foundations of Risk*, 24 SOUTHERN J. PHILOSOPHY 273 (1986).

credibility of our information and our willingness to judge an otherwise unexceptional act "risky."

One fairly common sense way to put the problem is to say that in the credible case there really is a risk of being bitten by rattlesnake, while in the incredible case there is not. Accordingly, the problem of separating these two stories can be understood as instance of the general question, "When are risks real?" The general question, of course, is the one we want to answer; so we want to set aside anything peculiar to our cases. Our seven year old neighbor probably does not expect you to believe his story (indeed, probably doesn't believe it himself), so there is a sense in which no one is expected to think that there really is a risk in the incredible case. Replace the seven year old with a deadly serious paranoid schizophrenic and that fact changes; we think there very will *might* be a snake in the mailbox. We are no more willing to credit the story about a voodoo priest, however. The judgment that the alleged risk in the incredible case is not a real one does not, therefore, depend only upon the sincerity of the informant. If we take the "deluded person" case as a third one, we might tend to think that there is a real risk in opening the mailbox because we have general beliefs about the behavior of such people, and clear and present evidence that the person before us is likely to be providing a deceitful account of a genuine danger.

Do these simple, nontechnical cases capture the main philosophical elements? The sheer simplicity of the cases should not be a problem, for they could be trumped up so that it is not rattlesnakes and mailboxes that concern us, but events inside the core of a nuclear reactor. Our neighbors could be experts, activists, or power company executives. Sorting out stories about voodoo priests and ascertaining the motives of our strange neighbors may be easier than sorting out the testimony of experts and divining the motives of scientists and executives who have

great personal stake in the technology at hand. In many important respects these issues seem to be quite the same.

A Baseline for the Reality of Risk

Our question, then is, "When are risks real?" Although we do not as yet have philosophical criteria for answering this question in a general way, our three rattlesnake cases can serve as paradigms in which common sense would have little difficulty establishing the reality of the risk in each particular instance. As noted above, the growth of risk analysis capability corresponds to a growing willingness to give a probabilistic answer to the general question, but before examining how probabilistic answers measure up to the paradigm cases, it will be useful to propose nonprobabilistic criteria that will serve as a baseline for deciding when risks are real.

Each of the three rattlesnake cases purports to provide some sort of story that explains how a rattlesnake might have come to be in a mailbox. Each of the three cases either describes or suggests a sufficient cause for there being a rattlesnake in the box. What is missing from the first case, the seven year old who reports a voodoo spell, that is present in the other two? The alleged causal mechanism, a magical spell, is at odds with the standard repertoire of causal forces that rational, informed adults take to be capable of producing events in the world. The second case indicates the cause, vandals placing the snake in the box, directly; the third case indicates a cause, a schizophrenic acting irrationally and placing a snake in the box, indirectly. Both of the indicated causal sequences are consistent with patterns of cause and effect that rational adults recognize, and, indeed, *are* the causal sequences that would be regarded as explaining the presence of the snake, should it subsequently be discovered in the box. Even if a snake were found in the box following the seven year old's testimony, the idea

of a magic spell would not be accepted as the cause of the snake's being found. Since the alleged cause would be rejected as a true cause, even when the snake *is* found, it is not taken as grounds for thinking that there is a risk of finding the snake in the *ex ante* case. The general answer to the question, "When are risks real?" is that risks are real when there is sufficient reason to suspect the presence of a causal sequence that would produce the unwanted event. I shall call this answer the *causal answer,* and shall contrast it to two *probabilistic answers* below.

The causal answer to the question of when risks are real has a number of advantages. First, it has *prima facie* plausibility, and is highly adaptable to a large variety of cases. It provides a test that is capable of eliminating a large number of hysterical or capricious allegations of risk. When legal persons commit acts that initiate a causal sequence indicating risk, the causal answer provides grounds for identifying parties that may be held responsible for damages when unwanted events materialize. The causal answer is nontechnical and can be understood, at least on the face of it, by any competent adult. The causal answer is also open ended in that initial causal judgments may suggest why additional information concerning the likelihood of specific sequences resulting in harmful events is desirable on a case by case basis.

At the same time, it must be admitted that there are enormous problems with the causal answer. Relying as it does upon a common sense repertoire of causal mechanisms, it is subject to multiple interpretations. There are, of course, people in the world who *do* recognize black magic as a legitimate form of causation, and people who *do not* recognize the efficacy of mental illness. The causal answer in itself does very little to help us distinguish between causal sequences that are possible but unlikely and casual sequences that suggest a real

and present danger. The causal answer is hardly the last word in identifying a risk, but it is not intended as such. The causal answer is a first order sorting device, at best: it supports the judgment that a risk is *real,* but does little to indicate the relative seriousness of equally real risks.

What is more, the concept of causality is itself notoriously difficult.[11] The law has standards for determining causality that are quite different from those of science. The criterion given by the causal answer is more consistent with a legal tradition that establishes cause by reference to the judgment of a reasonable person. On the other hand, if causality is nothing more than "constant conjunction," as Hume thought, perhaps we would do better to replace the concept with a notion of statistical correlation.[12] Analysis of this question opens immediately into some of the most difficult issues in the interface between science and the law. The gist of Chauncey Starr's suggestion, as it applies to law and public policy, is that we should *replace* the causal answer with a probabilistic one, just as science has (perhaps) replaced the notion of causality with more precise concepts derived from logic and mathematics.

Risk Objectivism and Risk Subjectivism

In keeping with the spirit of Starr's remarks, then, a more precise answer to the question, "When are risks real?" would say that they are real when there is a measurable probability of harm. This answer divides immediately into two, because there are two schools of thought on what probability means.[13] *Objectivists* use the idea of relative

11 Davis, *Probabilistic Theories of Causation,* in PROBABILITY AND CAUSALITY (J. Fetzer, ed. 1988); W. SALMON, SCIENTIFIC EXPLANATION AND THE CAUSAL STRUCTURE OF THE WORLD (1984); D. DAVIDSON, ESSAYS ON ACTIONS AND EVENTS (1980); J. L. MACKIE, THE CEMENT OF THE UNIVERSE (1974); and D. HUME, A TREATISE OF HUMAN NATURE (1739 — reprinted 1980).

12 W. SALMON, STATISTICAL EXPLANATION AND STATISTICAL RELEVANCE (1971).

frequency to define probability, while *subjectivists* use the idea of confidence.[14] Ian Hacking has recently called the debate between these schools a sterile one, ignored by those who actually are in the business of measuring probabilities,[15] so the entire debate may be regarded with skepticism. Nevertheless, the two rival accounts of probability provide

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[13] P. Hurley offers a simple textbook account of these views in A CONCISE INTRODUCTION TO LOGIC 405-418 (2nd Ed., 1985). More extended theoretical discussions include W. Kneale, Probability and Induction (1949); R. Carnap, Logical Foundations of Probability (1950); W. Salmon, The Foundations of Scientific Inference (1967); and I. Hacking, The Emergence of Probability (1975). The following authors defend objectivist views: R. von Mises, WAHRSHEINLICHKEIT, STATISTIK UND WAHRHEIT (1928 — translated by H. Geiringer as PROBABILITY, STATISTICS AND TRUTH 1951); Popper, *The Propensity Interpretation of Probability*, 10 BRITISH J. PHILOSOPHY SCIENCE 25 (1959); C. HEMPEL, ASPECTS OF SCIENTIFIC EXPLANATION 376-411 (1965); and C. GLYMOUR, THEORY AND EVIDENCE 85-93 (1980). Many of the important papers on subjectivism are collected in STUDIES IN SUBJECTIVE PROBABILITY (H. Kyburg and H. Smokler eds. 1964), *but see also* D. V. LINDLEY, INTRODUCTION TO PROBABILITY AND STATISTICS FROM A BAYESIAN VIEWPOINT (1965); B. de FINETTI, THEORY OF PROBABILITY (1974); P. HORWICH, PROBABILITY AND EVIDENCE (1982); R. JEFFREY, THE LOGIC OF DECISION (1983); and Papineau, *Probabilities and Causes*, 82 J. PHILOSOPHY 57 (1985).

[14] This characterization oversimplifies the distinction, which is really a three way distinction, at least, and perhaps a four way one. The classical theory does not derive probabilities by observing relative frequencies, but is usually counted "objective." Bayesian probability is usually equated with subjectivism, but R. Rozenkrantz, argues for objective Bayesianism in INFERENCE, METHOD AND DECISION (1977). *See, e.g.*, R. MILLER, FACT AND METHOD 267-345 (1986). Empirical risk estimation is far more sensitive to constraints on data collection than to the philosophical role of probability in confirming scientific theories, making it somewhat difficult to assess the relevance of philosophical sources cited here and *supra* at note 11. Quantitative risk analysts would probably be satisfied with the oversimplified alternatives given here: *see, e.g.*, Parry and Winter, *Characterization and Evaluation of Uncertainty in Probabilistic Risk Analysis*, 22 NUCLEAR SAFETY 28 (1981); Abramson, *Some Misconceptions About the Foundations of Risk Analysis*, 1 RISK ANALYSIS 229-230 (1981); and Kaplan and Garrick, *Some Misconceptions About Misconceptions: A Response to Abramson*, 1 RISK ANALYSIS 230-233 (1981).

[15] Hacking, *Culpable Ignorance of Interference Effects, in* VALUES AT RISK 141 (D. MacLean, ed. 1986).

the risk analyst with two different ways to redefine the causal answer, each of which goes to the foundations of scientific method. I shall, accordingly, discuss *risk objectivism* as the view that risk is a function of relative frequency (as well as other variables, of course), and *risk subjectivism* as the view that risk is a function of confidence (same qualification).

Risk objectivism

In many respects, the chance of pulling a rattlesnake out of a mailbox looks a lot like the chance of pulling a particular lottery ticket out of a vat. My odds of winning the lottery can be specified objectively as the ratio of the number of tickets I am holding to the total number of tickets sold. If I am holding ten tickets and only one hundred have been sold, my odds are one in ten. Why not say, by analogy, that the risk of pulling a snake from my mailbox is a ratio of the number of snake filled mailboxes to the total number of mailboxes? If there are fifty million mailboxes in the country, and five of them have snakes inside, my odds of pulling a snake from this one would be one in ten million.[16] There are some data collection problems both in knowing how many mailboxes there really are, and in knowing how many house snakes short of actually opening all fifty million of them to see.[17] These

16 Many statements of risk analysis methods have assumed without argument that risk identification will proceed by collecting relative frequency data. In addition to sources cited *supra* at note 3, *see* NUCLEAR REGULATORY COMMISSION, REACTOR SAFETY STUDY — AN ASSESSMENT OF ACCIDENT RISKS IN U. S. COMMERCIAL NUCLEAR POWER PLANTS, REPORT WASH-1400 (1975); NUCLEAR SYSTEMS RELIABILITY ENGINEERING AND RISK ASSESSMENT (J. B. Fussell and G. R. Burdick, eds. 1977); H. W. LEWIS *ET. AL.*, RISK ASSESSMENT REVIEW GROUP REPORT TO THE U. S. NUCLEAR REGULATORY COMMISSION, NRC REPORT NUREG/CR-0400 (1978); and Lewis, *The Safety of Fission Reactors*, 242(3) Scientific American 53-65 (1980).

17 Lewis, 1978, *supra* note 16. Many antinuclear activists seized upon this point in rejecting the rosy predictions published in the Rasmussen report (WASH-1400, *supra* note 16). Few (if any) advocated a subjectivist view in response. For anti-

problems have led some risk analysts to reject objectivism in favor the subjectivist view described below,[18] but they are not, in themselves, decisive objections to objectivism as an answer to the question, "When are risks real?" We have captured the reality of a risk when we have given the conditional probability, measured by relative frequencies, that the unwanted event occurs, relative to the given evidence. Stated more intuitively, our description of risk converges with reality when the events of concern (the snake in the mailbox) are described relative to all events of the general type (opening the mailbox). We may have practical difficulties in collecting the data needed to make our judgment converge with reality, but it is, at least, clear what the target is. The objective description of snakes in mailboxes is, thus, quite like the description of ticket stubs in a rotating vat.

The objectivist view eliminates reference to causes entirely, but does so at some cost to conceptual clarity. It seems to make the answer to our question "When are risks real?" the same in every case, even the incredible one. There is, after all *some* chance that a snake might be in the mailbox anytime you reach inside. There are, thus, no cases in which risk is not real. The risk objectivist has thus told us less than we knew (or, at least, thought we knew) in the first place. The damage may not be total, however. The relative frequency view requires three conditional probabilities to assess risk, each ranging over a different population of mailboxes. First, we want the number of actual snakes appearing in mailboxes incredibly described by unreliable seven year old

nuclear critics committed to the objectivist view, *see* Holdren, *The nuclear controversy and the limitations of decision making by experts,* 32 BULL. ATOMIC SCIENTISTS 20-22 (1976); A. LOVINS AND J. H. PRICE, NON-NUCLEAR FUTURES (1975); and Levi, *Assessing Accident Risks in U.S. Commercial Nuclear Power: Scientific Method and the Rasmussen Report,* 1980 SOCIAL RESEARCH 395-408.

18 Apostolakis, *Probability and Risk Assessment: The Subjectivist Viewpoint and Some Suggestions,* 19 NUCLEAR SAFETY 302-315 (1978); Parry and Winter, 1981 *supra* note 14.

children. Second, we want the number of actual snakes appearing in mailboxes credibly described by reliable adults. Third, we want the number of actual snakes inside mailboxes described in any fashion by suspiciously acting paranoid schizophrenics. Although there is, in principle (at least), a real risk in each case, it is reasonable to think that the conditional probability will be far higher in the credible case, and significantly higher in the third than in the first. Maybe this is all that is meant by "real risk."

Let us, again, set aside the practical problems of data collection (which have now been made even more intractable). Two problems remain. The first is that some of the important questions have been begged in determining which conditional probabilities to assess. The intuitive judgments about causes have not really been eliminated in the objectivist view; they are now just being built into the way that we define the conditions for assessing relative frequency. It is tempting to imagine that a scientist can, in the ideal case, observe and record all facts, then identify those correlations among facts having statistical significance strictly on the basis of mathematical and logical axioms. Were this ideal a tenable picture of scientific method, the reality of a risk might be given by the correlation of unwanted events to *ex ante* predictors. But such an ideal is so far from tenable as to be filled with absurdities too numerous to mention.[19] The point can be made, perhaps, by noting that if statistical studies confirmed our presumptive judgments of the relative reality of risk in our rattlesnake cases, it would almost certainly be the end of discussion. If *per impossible,* however, a statistical analysis of alleged instances of magic spells showed them to be highly correlated to the finding of snakes, the discussion would only be beginning. We would be less inclined to think that there really *are*

19 C. G. HEMPEL, PHILOSOPHY OF NATURAL SCIENCE (1966) discusses these problems at 11-18.

risks when such magic spells are reported than that some hitherto unsuspected causal mechanism (perhaps devotees of the shaman) is at work. Relative frequencies provide some *confirmation* for our initial causal judgments, but it is not at all clear that they provide a conceptual replacement for them.

The second problem is a special case of the first. Relative frequencies require repeatable test situations in order to become meaningful, but causal sequences may exist only in a single case. Indeed, the causal sequences that are of interest in legal hearings are often unique. The reasonable person has little difficulty in assigning causal efficacy to such sequences, but events such as "reports of vandals putting snakes in mailboxes," or "schizophrenics putting snakes in mailboxes," may not be repeated frequently enough to have statistical significance, even in the ideal case when "all the facts have been collected." There thus seems little warrant for *replacing* causal statements in risk descriptions with statements of conditional probabilities measured by relative frequencies. The relationship between our common sense notions of cause and risk, on the one hand, and statistically measured relative frequencies, on the other, is far more subtle than those who would have us define risk in terms of probability and consequence have recognized.

Risk subjectivism

The Bayesian or subjectivist view of probability has become attractive partly because it speaks to a host of the problems, both conceptual and practical, raised by the objectivist view.[20] In the interest of brevity, these advantages of the subjectivist view will be omitted here. A subjectivist will say many of things that have already been said about our rattlesnake cases, including the idea that risk can be

[20] Apostolakis, *supra* note 18 and Parry and Winter, *supra* note 14, remark on practical problems; Miller *supra* note 14 discusses conceptual advantages.

defined in terms of the conditional probabilities of unwanted events. For the subjectivist, our willingness to credit the evidence of our rational neighbor, and to discredit the evidence of the seven year old is a best expressed as a level of confidence in the prediction (e.g. that there is a snake in the mailbox) that each account implies. The level of confidence must be bound by time and information, for our confidence in any prediction changes over time and as we acquire more information. We may say, for example, that we think it will rain tomorrow (hence it would be risky to plan a picnic), but our confidence in this prediction will likely shrink or grow after hearing a weather report. The subjectivist will tell us that what we have (perhaps naively) described as a worry about the reality of risk is better described as a quandary about when we should have confidence in any claim indicating or alleging the existence of risk. In analyzing a simpler rattlesnake in a mailbox case, S. Kaplan and B. J. Garrick begin by noting that whether reaching into the mailbox is risky does not depend upon whether there really is a rattlesnake in it; it depends, instead, upon what we know (or believe) about the mailbox at the time we prepare to open it. For an observer who knows there is a snake in the box, opening the box is very risky; for one who does have this information, the risk may be quite low.[21]

Now, the subjectivist view appears to be vulnerable to a trivial objection, namely, that it makes risk entirely a matter personal reactions rather than scientific enquiry. It seems we can just *choose* the stories in which to place our confidence, and while you may prefer stories about neighborhood vandals, I may prefer stories about voodoo priests. The subjectivist response is that this is right as far as starting points are concerned, but that the person who fails to revise beliefs based on experience is just irrational. Bayes theorem, the heart of subjectivist theory, tells us how to revise our beliefs, and how to adjust the

21 Kaplan and Garrick, *On the Quantitative Definition of Risk*, 1 RISK ANALYSIS 11-27 (1981).

confidence we place in them. Failure to learn in a manner roughly consistent with Bayes theorem can only be attributed to unjustifiable biases or inexplicable confidence in beliefs that consistently turn out to mislead us.[22] This emphasis upon learning and the success of our beliefs, thus, turns out to explain pretty well why we should reject the story of the voodoo priest, but should be willing to accept the story about vandals. Two Bayesian decision makers given the same information will always arrive at the same level of confidence. If I am willing to credit the voodoo priest and you are not, it is simply because one of us has more (or different) information than the other.

This talk about learning requires one to state the objection to subjectivism in more precise terms. A key is in how Kaplan and Garrick themselves describe the rattlesnake case. They seem comfortable in saying that there is a risk here only if I have reason to believe that there is a snake inside. The snake is either in the box or it is not, the problem of risk arises only in conjunction with information about which state of affairs obtains.[23] Having opened the box many times and found nothing more sinister than an audit notice from the IRS, there is (for you) no significant risk of there being a snake. For me, having heard the neighbor's story, the risk is real. This, however, just seems wrong. Part of the motivation for positing a notion of real risk, distinct from perception, is to describe the situation in which we run risks of which we are unaware, even, perhaps, could not have been aware.

The problem is not that any strongly felt estimate of risk is valid, but that the Bayesian decision maker's estimate of risk can never be wrong.

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22 Research in cognitive psychology reveals that everyone fails to learn in the Bayesian way, but it is far from clear what to make of this fact. *See* D. KAHNEMAN, P. SLOVIC, AND A. TVERSKY, JUDGMENT UNDER UNCERTAINTY: HEURISTICS AND BIASES (1982).

23 Kaplan and Garrick, 1981 *supra* note 21.

Learning is just the addition of more information. New information may require revision of our beliefs, but it cannot invalidate the judgment made on the prior, more limited, information. Aside from the failure to apply Bayesian rules of inference, there can be no possibility of error in the subjectivist account of risk. There is no way to say that someone ought to have known, ought to have thought of, or was culpably ignorant. Contrary to this result, an important proportion of our concern about risk arises from the possibility of events to which we cannot assign meaningful probabilities, and of which we may be culpably ignorant.[24] The subjectivist account defines risk only in the *ex ante* perspective; it does not tell us what risks we should have been aware of, *ex post.*

What Have We Learned?

The objectivist view makes it too hard for us to be right in making a risk judgment, while the subjectivist view makes it too hard for us to be wrong. While frequencies are clearly relevant to the confirmation of a causal hypothesis, the objectivist attempt to define "cause" or "risk" in terms of relative frequencies simply does not carve nature at the joints: too many instances in which a reasonable person would make paradigmatically rational judgments about risk turn out to be undefined and meaningless when translated into probabilistic concepts. On the other hand, in making risk entirely a matter of consistent application of Bayesian inference rules, given fixed information, the subjectivist makes it impossible to misjudge risks. If risk is always just what the decision maker thinks it is, then the concept of risk provides no grounds for *ex post* judgments of culpability; but such judgments are the heart of tort law. We are, therefore, left with the causal answer: risks are real whenever its admittedly vague conditions would be judged to apply by a

24 Hacking, *supra* note 15, at 152-154.

reasonable person. These conditions require that the evidence available at the time of choice supports an inference that a proposed action is causally related to unwanted consequences, or that such causal relations exist, despite being unknown and even unknowable to the agent. This is not to say that an agent should be held responsible for unknowable consequences, only that ignorance of them does not make risk non-existent. The objectivist view of risk overshoots these rough criteria in one direction, while the subjectivist view overshoots them in the other.

This philosophical conclusion supports several implications for policy. It is, at least a further argument against the view that the assessment stage of risk analysis is a purely objective and value free domain of scientific enquiry. The dangers of sharply splitting assessment from acceptance have been noted before,[25] but this seems to be a lesson that bears repeating. A more specific policy implication here is that probabilistic notions of risk are not necessarily better than causal ones. While I would not want to be construed as rejecting the relevance of statistical risk studies, I do think that the considerations discussed above support the view that a reasonable person's concept of risk, vague as it is, is *better* suited to the regulatory requirements of risk management than are probabilistic concepts. As such, any suggestion that probabilistic concepts of risk should become the *basis* of risk management decision practice or of legal decisions regarding risk is a regressive pursuit of false Gods. Such a shift in our social practices for the management of risk might serve the political aims of big science (many of which I support), but not the philosophical commitment to truth and objectivity.

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[25] Dreyfus and Dreyfus, *Inadequacies in the Decision Analysis Model of Rationality*, in 1 FOUNDATIONS AND APPLICATIONS OF DECISION THEORY 115 (Hooker, Leach and McClennon eds. 1978); Shrader-Frechette, *Environmental Impact Analysis and the Fallacy of Unfinished Business*, 4(1) ENVIRONMENTAL ETHICS 37 (1982); M. SAGOFF, RISK-BENEFIT ANALYSIS IN DECISIONS CONCERNING PUBLIC SAFETY AND HEALTH (1985).