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Introduction

The concept of *risk* has traditionally given rise to such questions as: What is the nature of risk? Why is it practically useful to posit the notion of risk? Is risk a fundamentally irreducible and unexplainable concept? If it is not, how should we define risk? How should we think about risk in the context of human experience? These are all questions relating to what can collectively be referred to as the *cognitive status* of risk.[1]

In a recent essay,[2] Paul Thompson provides some interesting and thought-provoking perspectives on many of these questions. Central to his analysis is the question, "When are risks *real?*" In answering this question, Professor Thompson challenges two basic tenets of contemporary risk analysis. The first of these tenets is the philosophical view that risk is fundamentally an epistemic category that is grounded in empiricism — a view that Thompson labels *probabilistic*. The second

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1 For the purposes of this discussion, I draw a somewhat informal distinction between the *cognitive status* of risk and the *psychology* of risk, where the latter is concerned mainly with how people react to and perceive uncertainty, and the former is concerned with the mental processes that are associated with our conceptualization and understanding of risk.

2 Thompson, *Risk Objectivism and Risk Subjectivism: When are Risks Real?*, 1 RISK: ISSUES IN HEALTH AND SAFETY 3 (1990).

tenet concerns the role of probability theory in risk analysis. With regard to each of these tenets, Thompson argues that probabilistic conceptions of risk are fundamentally inadequate for *understanding* and *reasoning* about risk. In what follows, I shall be concerned with examining the various arguments that Thompson puts forth in defense of this claim.

From the onset, it is important to note that while the notion of risk has historically taken on a number of different interpretations, it has almost always been defined in terms of *loss* and *uncertainty*.[3] In classical decision theory, e.g., the word "risk" describes the situation where both the possible states of nature and the probabilities associated with these states are known. Although this view departs somewhat from the way the word is used in most risk analysis contexts, where probabilities are often unknown, it does accurately convey the above connotation. It is, I think, reasonable to affirm that our modern conception of risk does, in fact, conjoin the common sense notion of loss with that of uncertainty. In an earlier essay,[4] Thompson accepts this general conception of risk, and proposes the following definitions for *real, observed,* and *perceived* risk:

> *Real Risk.* The combination of chance and negative consequence that exists in the real world.
> *Observed Risk.* The evaluation of the combination of chance and negative consequence as measured by a theoretical model of the physical world.
> *Perceived Risk.* The estimate of real risk made in the absence of a theoretical model of the physical world.

Using these definitions, Thompson distinguishes between *erroneous analysis* and *erroneous judgment,* where the former represents the difference between real and observed risk, and the latter represents the difference between real and perceived risk.[5]

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3 W. D. ROWE, AN ANATOMY OF RISK (1977) [hereinafter ANATOMY OF RISK].

4 Thompson, *The Philosophical Foundations of Risk,* 24 S. J. PHIL. 273 (1986) [hereinafter *Philosophical Foundations of Risk*].

What is interesting to note about Thompson's definitions is that his notion of "real risk" immediately leads us to some difficult philosophical and methodological questions, many of which are, at bottom, questions that ultimately reduce to metaphysical disputes about realism and antirealism. As I will show below, Thompson's philosophical inclination is to view risk as a formal, abstract object. This brand of risk realism, I will argue, is plagued by a number of conceptual and pragmatic difficulties, many of which are not easily cast aside. Central to my analysis is the view that by positing the notion of "real risk," Thompson is, in effect, affirming the possibility of certain knowledge. This is shown to have undesirable consequences for the theory and practice of risk analysis, the most important of which is that it promulgates the view that there exist *exact* or *certain* values to which we should strive for in our analyses.

I want to address four main issues in response to Thompson's essay. In the next section, I begin by considering the conceptual distinction that is drawn between actual and perceived risk. Here, I relate various philosophical issues concerning the interpretation of scientific theories to questions concerning the potential reality of risks. As part of this discussion, I put forth both realist and antirealist conceptions of risk, and I examine these risk paradigms in the context of Thompson's analysis. This is then followed by a discussion of the cognitive objectives of risk analysis, and a discussion of how the results of such analyses should be interpreted in light of these objectives. In the section that follows, I examine the role of the principle of causality in modern risk analysis, with particular emphasis on Thompson's use of

5 In AN ANATOMY OF RISK, William Rowe uses the term *descriptive uncertainty* in roughly the same way that Thompson uses the term *erroneous analysis*, and observes that "it is an underlying precept of scientific positivism that it should be possible to discover all natural law. This implies that the descriptive uncertainty of the universe can be reduced to a value approaching zero." ANATOMY OF RISK, *supra* note 3, at 20.

the principle in answering the question, "when are risks real?" Here, I identify various features of Thompson's causal answer, and I argue that causality is fundamentally probabilistic in character. In the next section, I take up the issue of the role of probability in risk analysis. The section begins with a brief discussion of the meaning of probability statements. As part of this discussion, I look historically at the conceptual evolution of the frequentist and subjectivist views of probability. I then consider various aspects of the nature of objectivity and subjectivity in risk analysis, with particular emphasis on the sharp distinction that Thompson draws between risk objectivism and risk subjectivism. The paper concludes with an assessment of the degree to which the traditional debate between Bayesians and frequentists is relevant to the theory and practice of risk analysis.

Real Risk and Perceived Risk

The distinction that is drawn between *actual* and *perceived* risk is a pervasive theme in risk analysis. In principle, the distinction between the two concepts lies in the assumption or belief that actual or, as I will call it, *real* risk is a measure of how things stand in the world, whereas perceived risk is judgmental in nature, and does not necessarily coincide with reality.

At first glance, the distinction between real and perceived risk seems reasonable, in that reality and perception are not always conjoined. Thompson argues that the conceptual dichotomy between these two views of risk is founded on the philosophical premise that risk is a reality that is somehow discernible from empirical inquiry. Real risk, Thompson says, "is the object of natural science inquiry," whereas perceived risk "is the object of social science inquiry."[6,7] In this

6 Thompson, *supra* note 2, at 7.

7 The role that natural science inquiry plays in the analysis of risk needs, here, to be distinguished from views that construe "real" risk as the privileged concern of

way, risk is seen as a matter of how things stand in the world.

Insofar as science is taken to be a process by which man attempts to explain or describe how things actually stand in the world, the degree to which risk statements can be taken to be statements about reality is a question of both philosophical and methodological importance. A central question we face in this regard is to what extent is it reasonable to enquire as to the "reality" of risk(s), and, moreover, how do our interpretations or views of science and scientific theories influence the way we approach the question, "when are risks real?" These are two questions to which I now turn.

The Interpretation of Scientific Theories

The interpretation of scientific theories has long been the subject of considerable debate among philosophers and scientists, alike. Central to much of this debate is the question of whether scientific theories should, or in fact can, be interpreted as true or false statements.[8] While it is beyond the scope of this essay to provide a thorough discussion of the body of work that addresses this question, it is worth considering how some of these ideas relate to the cognitive status of risk.

Realism and antirealism

In its simplest form, scientific realism assumes that physical reality is independent of man's existence. In this way, science not only produces predictions, but also describes the *true* nature of things. Antirealism, on the other hand, denies that science describes anything "real" at all, but instead serves as a logical instrument for prediction and

science. Such views are markedly *positivistic* in that they affirm that science is the only valid source of knowledge about risk(s). It is, I think, fair to say that such modes of thought promote an intellectual arrogance that not only belies the fallibility of human knowledge, but also obfuscates the fact that science is *not* the only source of reliable knowledge. At most, positivistic conceptions of risk succeed only in impeding meaningful dialogue between the lay public and the scientific community in matters of risk and human safety.

8 E. NAGEL, THE STRUCTURE OF SCIENCE (2d ed. 1979).

control. In a particularly lucid analysis of the concept of truth, Paul Horwich summarizes the philosophical bases for these two opposing views as follows:[9]

> [T]here can seem to be a tension in ordinary thinking between the metaphysical autonomy of the world (its independence of us) and its epistemological accessibility (our capacity to find out about it). The difference between a realist and an antirealist ... is that the realist decides ... that there is actually no difficulty here ... whereas the antirealist decides, on the contrary, that the alleged conflict is genuine and that it has certain ramifications for what we can take ourselves to know.

Given these two views, we can, in a similar fashion, outline both realist and antirealist conceptions of risk. For the purposes of this discussion, we will denote these two views of risk by $risk_r$ and $risk_a$, respectively. A realist conception of risk affirms that risk is something that is intrinsically independent of human knowledge. In this way, $risk_r$ describes the view that *in the world there are relatively isolated activities, objects, or phenomena that pose a potential threat to human health and safety; as an endeavor that is, insofar as possible, grounded in science, risk analysis is a process that is capable of identifying and describing them.* Alternatively, $risk_a$ describes the view that risk is not independent of human knowledge, and is *fundamentally an epistemic category that serves as an organizing principle whereby empirical statements concerning matters of human health and safety are analyzed with respect to a conceptual framework of interpretation.*

The notion that "reality" and "truth" are semantically linked is a pervasive theme in the history of philosophy and philosophy of science.[10] Our inquiry into the potential reality of risks can easily be thought of in these terms. With regard to the two opposing views of

9 P. HORWICH, TRUTH 57 (1990).

10 *Id.*

risk, $risk_r$ describes the view that risk statements are, in fact, statements about reality, and are therefore appropriately characterized as being either true or false. Alternatively, $risk_a$ describes the view that risk statements are not premises from which factual conclusions are deduced, and are therefore not appropriately characterized as being either true or false.[11]

It is possible to retreat somewhat from the realist and antirealist conceptions of risk that $risk_r$ and $risk_a$ represent. If we suppose, as Thompson does, that risks are either real or they aren't, then, as he puts it, "we are obligated to act upon the *true facts,* so far as we can tell what they are."[12] From this vantage point, we can assert that our *belief* in the truth or reality of a particular risk statement is enhanced when the statement is supported by empirical evidence. In this way, risk statements are viewed as means of representing relations of dependence between observable events and properties. Moreover, according to this view, questions as to the truth or falsity of risk statements are characterized as such only insofar as the assertability of these statements is translatable into matters of observation and testability.[13]

Thompson's risk realism

Thompson's inclination towards a realist conception of risk is evident in the following quote:[14]

> To the extent that risk analysis is committed to the general concept of nature as a sphere of things and events, *existing independently of and largely unaffected by our attempts to know it,* the characterization of risk in terms of chance and negativity ... appears to be appropriate.

The idea that risk is independent of human knowledge is also one that Thompson holds out as being important in answering the question,

11 NAGEL, *supra* note 8.

12 Thompson, *supra* note 2, at 8 (emphasis added).

13 NAGEL, *supra* note 8.

14 *Philosophical Foundations of Risk, supra* note 4, at 285 (emphasis added).

"When are risks real?"[15] As he puts it:[16]

> 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.

From the two passages above, it is readily apparent that Thompson's philosophical inclination is to view risk as a formal, abstract object. The position he takes is markedly *platonistic,* in that it holds that risk is fundamentally independent of human knowledge. Platonism, as a distinct and discernible mode of thought, occupies an important position in the history of Western thought, and has, in many respects, influenced the development and advancement of science. An extreme platonist view of risk, however, construes "real risk" or, $risk_r$, as an abstract entity that is neither spatio-temporal nor causally interacting with the physical world.[17] Such an extreme view seems to run counter to almost every common sense conception of risk, and, moreover, leaves open the fundamental question of how we learn and attain knowledge about risks.

While I do not believe that Thompson is necessarily advocating an extreme platonist view of risk, there are some philosophical and methodological problems associated with the notion of risk realism that merit further consideration. First, by positing the notion of real risk, Thompson is essentially affirming that risk statements have truth values that are somehow independent of the theories that such statements are

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15 This point has been made before in the literature. N. RESCHER, (RISK: A PHILOSOPHICAL INTRODUCTION TO THE THEORY OF RISK EVALUATION AND MANAGEMENT (1983)), for example, makes an interesting distinction between *taking a risk* and *running a risk,* where risk taking describes the situation where an agent mindfully selects a particular course of action, and running a risk describes the situation where a risk exists, but is not necessarily known by the agent.

16 Thompson, *supra* note 2, at 20 (emphasis added).

17 A. FLEW, A DICTIONARY OF PHILOSOPHY (2d ed. 1979); H. Putnam, *Models and Reality,* in PHILOSOPHY OF MATHEMATICS: SELECTED READINGS 421-444 (P. Benacerraf & H. Putnam eds. 2d ed. 1983) [hereinafter *Models and Reality*].

invariably made relative to. Among the more problematic features of this view is that, in principle, it ascribes what Hilary Putnam[18] has called "nonnatural mental powers" to our capacity as human beings to grasp and understand abstract entities.

Putting aside, for the moment, Thompson's implicit appeal to mysterious mental faculties, let us briefly consider some of the consequences of adopting a sharp philosophical distinction between real and observed risk. Suppose, for the purposes of this discussion, that real risk and perceived risk lie at opposite ends of a continuum, and that observed risk lies somewhere in between these two extremes. In assessing or estimating observed risk, an obvious desideratum is to lie as close on this continuum to real risk as possible. Naturally, each incremental step we make towards real risk is, in effect, a better and better representation of reality. A reasonable cognitive objective in assessing observed risk, then, is to minimize the absolute difference between real and observed risk.

The *prima facie* plausibility of this view notwithstanding, there are, of course, a number of methodological problems associated with it. The most difficult of these problems was mentioned above, namely, the implicit appeal to nonnatural mental powers for learning and acquiring knowledge about real risks. In the absence of such powers, how are we to ascertain where we lie on the continuum? Moreover, how are we to know if, or when, real risk and observed risk coincide? Naturally, these are difficult questions for which there are no hard and fast answers.[19]

Considerations such as these suggest that the notion of real risk is

18 *Models and Reality, supra* note 17.

19 Putnam summarizes the problem in the following way: "It may well be the case that the idea that statements have their truth values *independent* of embedding theory is so deeply built into our ways of talking that there is simply no 'ordinary language' word or short phrase which refers to the theory-dependence of meaning and truth." *Models and Reality, supra* note 17, at 430 (emphasis in original).

plagued by a number of conceptual and pragmatic difficulties, many of which are not easily overcome. By positing $risk_r$, we are, in effect, affirming the possibility of certain knowledge. In particular, we promulgate the view that there exist *exact* or *certain* values of risk to which we should strive for in our analyses. This position is, I think, fundamentally misguided, and has been seriously challenged by modern philosophers on a number of fronts.[20]

We are forced to conclude that the merits of adopting a strict sense of the notion of real risk must be judged in terms of the cognitive objectives of risk analysis. One can argue, e.g., that risk realism can serve as a maxim for risk analysis inquiry. Viewed in this way, $risk_r$ is construed as an ideal to which we should strive for (but perhaps never achieve) in our analyses. Accordingly, the risk realism paradigm can have a potentially positive influence upon our efforts to achieve the cognitive objectives of risk analysis, and may even give rise to strategies for research and understanding. As I have argued, however, these potential benefits do not come without certain costs. Most important in this regard is the view that it is fundamentally unreasonable philosophy to posit the existence of exact values to which we should strive for in our analyses of risk. Such a view of risk, I will argue below, is rigidly linked to that of determinism, and fails to place in perspective the fact that our search for complete certainty in matters of risk and human safety is, both in principle and in practice, fundamentally unachievable.

Cognitive Objectives

Having considered how the interpretation of scientific theories can influence the way we approach the question "When are risks real?", we now turn our attention to the question of what relevance these ideas have to the analysis of risk. In particular, we address two basic questions: (1) What are the cognitive objectives of risk analysis? and (2) How should

20 *See, e.g.*, P. SUPPES, PROBABILISTIC METAPHYSICS (1984).

risk statements be interpreted in light of these objectives?

In the introduction of this essay I posed the following question: Why is it practically useful to posit the notion of risk? The principal motivation for positing the notion of risk is, I think, self-evident: to help man cope with an ever-changing and uncertain environment. Long before the rise of modern civilization, man sought to obtain reliable knowledge and information about his environment. This desire for control was, of course, motivated by a primal desire for survival. Obviously, this desire for survival and increased quality of life has, in turn, given rise to the notion of risk. In this way, the notion of risk is derived from common sense reasoning, and arose from the pragmatic concerns of everyday life.

Naturally, the pragmatic concerns of daily living require that we go beyond just abstract, generalized conceptions of risk. Accordingly, it is reasonable to assert that risk is best viewed as a fundamental concept whose *raison d'être* is to motivate certain lines of inquiry whose purpose is *prediction* and *control*. In this way, risk analysis can be construed as *a structured process that seeks to arrive at predictively informative assertions about possible events which, should they transpire, could pose a potential threat to human health and safety.*

If we accept the basic premise that prediction and control are the fundamental objectives of risk analysis, then how should risk statements be interpreted and used in light of these objectives? While it is beyond the scope of this essay to provide a detailed response to this question, it is useful to consider two views that seem particularly relevant to the present discussion, namely, the philosophical concepts of *fallibilism* and *falsifiability*.

Fallibilism

Fallibilism describes the philosophical view that the conclusions of scientific inquiry are always subject to question. Such a view as applied to risk analysis is appealing in that it does not seek to make the answers,

explanations, or predictions of such analyses final. As a matter of reasonable philosophy and practice, the results of risk analyses should be presented in such a way as to leave open the possibility that new evidence could arise that would call into question the validity or legitimacy of prior analyses. This view has long been advocated by social scientists and policy makers, alike.[21]

The reasonableness of a fallibilistic view of risk becomes readily apparent when we consider a simple model of the dynamics of how we learn and acquire knowledge about risks. At any one time, our knowledge and information about a particular risk statement, call it R, can be characterized in one of the following three ways:[22]

R is *accepted;*
R is *rejected;*
R is *indetermined,* thus neither accepted nor rejected.

Our acceptance, rejection, or indetermination of R is based upon the evaluation of the evidence that is available to us. How we actually choose to evaluate R in light of this evidence is not important here. What is important to note is that a fallibilistic view of risk does not view any one of these three possible states as being *absorbing* in character. By this I mean, e.g., that R's acceptance does not preclude the possibility that new evidence could arise that ultimately leads us to reject or suspend judgment on R. This position essentially amounts to a belief that no amount of evidence is ever viewed as providing an infallible argument for the acceptance, rejection, or indetermination of a particular risk statement. Naturally, a fallibilistic view of risk allows us to leave open the possibility that some risks may not even be known to us.

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21 In discussing the value-laden nature of risk analysis, Rowe, for example, notes that the results of risk analyses "are often expressed in explicit, objective terms that belie the subjectivity of the value judgements employed in arriving at them." ANATOMY OF RISK, *supra* note 3, at 4.

22 I. LEVI, GAMBLING WITH TRUTH (1967).

Falsifiability

The concept of falsifiability, which is due to Karl Popper,[23] is closely related to that of fallibilism. This view affirms that scientific knowledge is always open to question, and that no theory is ever conclusively verified. In the context of risk analysis, falsifiable risk statements are desirable ends in themselves, in that critical analysis is always in force, and, because of this, there is never any one claim to ultimate truth or reality.

The Causal Answer

The principle of causality plays an important role in Thompson's approach to the question, "When are risks real?" Risks are real, says Professor Thompson, "when there is sufficient reason to suspect the presence of a causal sequence that would produce [an] unwanted event." Thompson calls this the *causal answer*. A salient feature of the causal answer, Thompson says, is that it is "nontechnical" and "can be understood by any competent adult."[24] What is it that makes the causal answer, as stated, nontechnical? As I see it, what makes it so is that it fails to address two fundamental questions. First, it says nothing about the semantic interpretation of the linguistic entities *cause* and *effect*, i.e., how do these linguistic entities come together to form what, in ordinary language, we call a *causal relation?* Second, the causal answer does not provide an account of what kind of logical relation or operator is associated with, or attributed to, these causal entities.[25] Consequently, Thompson's causal answer addresses neither the *nature* nor the *structure* of causality.

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23 K. POPPER, THE LOGIC OF SCIENTIFIC DISCOVERY (1959).

24 Thompson, *supra* note 2, at 12.

25 Domotor, *Causal Models and Space-Time Geometries*, in SPACE, TIME, AND GEOMETRY (P. Suppes ed. 1973).

In principle, causal theories are instruments of *explanation* and *prediction.* As such, they play a central role in achieving the cognitive objectives of risk analysis.[26] Thompson's causal answer, however, lacks a relational structure between ordinary experience and what it says constitutes a "real" risk. As such, Thompson's causal answer is *predictively uninformative,* and therefore plays little more than a minimal role in achieving the cognitive objectives of risk analysis.

In what follows I argue that causality is fundamentally probabilistic in character, and that, because of this, Thompson's causal answer is more appropriately characterized as a *probabilistic* causal answer.

Causality and the Humeian Tradition

The principle of causality is an essential element of both common sense and scientific reasoning. In discussing the causal answer, Thompson rightly notes that the conceptual evolution of causality has been both difficult and problematic. If we examine the combined histories of philosophy and science, starting with the classical views of Aristotle, and ending with the empiricist philosophy of Hume, it becomes readily apparent that, in a particular epoch, the meaning ascribed to the principle of causality has closely paralleled that of the prevailing physical paradigm. Naturally, our view of the world influences, to a large extent, the way we approach the question of causality. It is not surprising, then, that much of the history of philosophical thinking about causality is deeply rooted in the determinism of classical Newtonian physics. And despite the fact that in this century the rise of quantum mechanics has lead to dramatic (in fact, revolutionary) transformations in the way we think and reason about the physical world, the determinism of classical physics is still very much a part of our prevailing world view.

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26 Valverde A., Jr., *Probabilistic Causality and its Applications to Risk Analysis,* in THE ANALYSIS, COMMUNICATION, AND PERCEPTION OF RISK, 9 ADVANCES IN RISK ANALYSIS (1991).

The determinism of the Newtonian paradigm dominates much of David Hume's[27] famous analysis of causality. In his analysis, the relation between cause and effect is said to possess three distinct characteristics: (i) continuity; (ii) succession in time; and (iii) constant conjunction. This view has dominated much of the history of philosophical thinking about causality.[28]

In modern accounts of causation,[29] a major point of contention with the Humeian analysis lies with requirement (iii). Specifically, the supposition that effects follow their causes in a constant fashion does not always agree with experience, and furthermore, presupposes that every event must have a fully determinant cause.[30] Many modern philosophers have also questioned the appropriateness of deterministic accounts of causality on the basis that they depart sharply with how causal notions are utilized in ordinary language.

Given these objections, what philosophers have sought to arrive at in recent decades is a conception or definition of causality that serves the scientific purposes to which the everyday notion is put. To this end, modern analyses have extended the Humeian position by viewing the connection between cause and effect as being fundamentally *probabilistic* in nature.

In discussing the possible role of probability in the causal answer, Thompson notes that if "causality is nothing more than 'constant conjunction,' as Hume thought, perhaps we would do better to replace

27 D. HUME, A TREATISE ON HUMAN NATURE (1888).

28 P. SUPPES, *supra* note 20.

29 *See, e.g.*, PROBABILITY AND CAUSALITY (J. Fetzer ed. 1988); I. J. Good, GOOD THINKING: THE FOUNDATIONS OF PROBABILITY AND ITS APPLICATIONS (1983) [hereinafter GOOD THINKING]; Suppes, *A Probabilistic Theory of Causality*, 24 ACTA PHILOSOPHICA FENNICA (1970); and P. SUPPES, *supra* note 20.

30 Patrick Suppes, for example, notes that "if we replaced *constant conjunction* by *frequent conjunction*, we would get something that would be more faithful to the facts and that would do little violence to Hume's analysis." *Supra* note 20, at 39.

the concept with a notion of statistical correlation."[31] As I have already said, the prevailing conception of causality is that it is, in fact, more than just constant conjunction. Moreover, it is, I think, well understood that perfect statistical correlation does not necessarily imply a valid causal relationship; correlation is only one means by which to *identify* valid causal relationships.[32]

Conceptually, it is important to recognize that a probabilistic view of causality is robust in that, in the limiting case, it is possible to define *sufficient* or *determinate* causes that produce their effects *with probability one.* Probabilistic causality, therefore, encompasses the deterministic view of causality as a special case. For this reason, when Thompson speaks of "replacing" the causal answer with a probabilistic one, he seems to lose sight of an important philosophical and conceptual distinction. Unless he is suggesting that the underlying metaphysical foundation for the causal answer is the assumption or belief that *every* event has a sufficient determinant cause, then the issue is *not* whether we should *replace* the causal answer with one grounded in probabilistic terms, but rather that, in the absence of such a deterministic view, the causal answer is, in fact, *probabilistic* in character.[33]

In consequence, Thompson's causal answer is, it seems to me, more appropriately characterized as a *probabilistic* causal answer, which can be summarized as follows:

Probabilistic Causal Answer. Risks are real when there is

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31 Thompson, *supra* note 2, at 13.

32 Ian Hacking, in discussing causalism, rightly notes that "good Humeians know there must be more than mere correlation." I. HACKING, REPRESENTING AND INTERVENING 35 (1983).

33 I. J. Good takes a somewhat different route in arriving at basically the same conclusion: "If the world is deterministic then probabilistic causality does not exist, but we'll never know with certainty whether determinism or indeterminism is true. So it is legitimate to assume indeterminism even if it is only a convenient fiction, somewhat like using the axiom of choice in a mathematical proof." GOOD THINKING, *supra* note 29, at 393.

> sufficient reason to believe that there exists a causal sequence that, with high probability, could produce an unwanted event.

The probabilistic causal answer allows us to responsibly represent those cases where the risks in question can not be explained in certain and unambiguous terms. Naturally, the higher the probability of a delineated causal sequence, the more real and present the risk to human health and safety becomes. Thompson seems to acknowledge the inevitability of such an approach to the causal answer when he distinguishes between (1) causal sequences that are *possible* but *unlikely,* and (2) causal sequences that suggest a real and present danger.[34]

The Role of Probability in Risk Analysis

Probability theory plays a central role in our modern conception of risk. It is interesting to note, however, that the use of probability for the analysis and management of risk has had a curious and somewhat varied history. This is due, in part, to a lack of agreement among risk analysts as to the nature of the relation between probability and risk. In addition, the interpretation of probability has often been viewed as an important issue in the foundations of risk theory. In the latter half of his essay, Thompson discusses the role of probability in risk analysis, with particular emphasis on the two well-known interpretations of probability: the *frequentist* interpretation and the *subjectivist,* or *Bayesian,* interpretation. Inasmuch as these two competing views are applied in risk analysis, Thompson draws a distinction between *risk objectivism* and *risk subjectivism,* which construe risk as a function of relative frequency and confidence, respectively. Thompson considers each of these views from both philosophical and pragmatic vantage points, and concludes that probability theory is fundamentally inadequate for understanding and reasoning about risk.

34 Thompson, *supra* note 2, at 12-13.

In this section, I consider some of Thompson's critical remarks on these issues. My remarks are organized as follows. I begin by considering the meaning of probability statements. As part of this discussion, I look historically at the conceptual evolution of the frequentist and subjectivist views of probability. I then consider various aspects of the nature of objectivity and subjectivity in risk analysis, with particular emphasis on the sharp distinction that Thompson draws between risk objectivism and risk subjectivism. Lastly, I consider the degree to which the traditional debate between frequentists and subjectivists is relevant to modern risk analysis, and I argue for a pluralistic view of the role of probability in risk analysis.

The Meaning of Probability

Probability theory dates as far back as the seventeenth century, where it had its inception in the analysis of games of chance. Historically, the conceptual evolution of probability has been anything but unproblematic. The largely philosophical debates as to the "correct" interpretation of probability are traditionally divided between two schools of thought: the *frequentist* interpretation and the *subjectivist,* or *Bayesian,* interpretation. The frequentist interpretation corresponds to our notion of probability as a relative frequency.[35] Alternatively, the Bayesian interpretation views the probability of an event as a subjective *degree of belief,* which takes into account all relevant knowledge and information.

For the better part of the twentieth century, the conceptual differences separating the Bayesian and frequentist views have been the

35 Let S denote a sample space, and let C be some subset of S. If we perform an experiment and the outcome is an element of C, then we say that the *event* C has occurred. Suppose that we perform the random experiment N times, and the event C occurs a total of f times throughout the N performances. The ratio f/N is called the *relative frequency* of the event C in these N experiments. R. V. HOGG & A. T. CRAIG, INTRODUCTION TO MATHEMATICAL STATISTICS (4th ed. 1978).

subject of long and (mostly) inconclusive debate. Early in this century, the frequentist view prevailed as the dominant paradigm, with R. A. Fisher[36] and J. Neyman[37] as central proponents. However, as statistics began to play a more prominent role in scientific inquiry, the limitations of the frequentist approach to probability became increasingly apparent, especially in contexts where scarce data was available. In the 1950's, the seminal work of Leonard Savage[38] and his colleagues laid the necessary foundations for the subjectivist viewpoint, and provided the conceptual means by which to use probabilities in the absence of data. By the 1970's, the frequentist conception of probability was no longer viewed as the only reasonable interpretation of probability.

Historically, the traditional debate between Bayesians and frequentists has taken a number of different guises. Not surprisingly, Bayesians disagree variedly among themselves,[39] as do frequentists. For this reason, it is not entirely correct to speak of Bayesians, *qua* Bayesians, and frequentists, *qua* frequentists. For the purposes of this discussion, however, it is useful to consider two notions that, on the surface, seem to separate these views, namely, *objectivity* and *subjectivity*.

Objectivity and Subjectivity in Risk Analysis

The interpretation of probability statements has traditionally been viewed as an important issue in the definition of risk. One reason for this is that the notions of objectivity and subjectivity play different roles in our usual conceptions of risk. For example, on the one hand, we

36 R. A. FISHER, CONTRIBUTIONS TO MATHEMATICAL STATISTICS (1950); Fisher, *Statistical Methods and Scientific Induction*, 17 J. STATISTICAL SOC'Y, SERIES B 69 (1955).

37 J. NEYMAN, A SELECTION OF EARLY STATISTICAL PAPERS OF J. NEYMAN (1967).

38 Savage, *The Foundations of Statistics Reconsidered (1961)*, in READINGS IN UNCERTAIN REASONING (G. Shafer & J. Pearl eds. 1990).

39 Good has determined (albeit somewhat wittily) that there are 46,656 varieties of Bayesians; GOOD THINKING, *supra* note 20, at 20–21.

want to be as "objective" as possible in assessing or estimating risk. On the other hand, we recognize that what one person considers "risky" may not be considered so by another. In some respects, then, the notions of objectivity and subjectivity are intermeshed in our everyday conceptions of risk. This duality of sorts has had important implications for the ways in which probability theory has been applied in risk analysis.

Risk objectivism

Risk objectivism construes risk as solely a function of relative frequency. The objections that Thompson raises against this view are essentially the same as those that have traditionally been raised against the frequentist view of probability. For example, a common objection concerns the problems that are associated with the direct measurement of repeated trials. In some cases, it may be difficult, costly, or even impossible to perform repeated measurements, thereby putting in question the meaningfulness of relative frequency probabilities so derived. Moreover, in those cases where frequency data is available, it may not be representative. These methodological problems, Thompson argues, make it difficult to adopt a conception of risk that is based solely on a frequentist view of probability. An important issue that Thompson fails to address in this particular discussion is the manner in which new evidence is incorporated into our probability assignments. As Savage[40] and others have argued, once a strong frequentist position is adopted, it is not possible to introduce new evidence into our probability assessment. I will not dwell here on the technical details surrounding this specific issue; I only point out, as Thompson does, that philosophical and methodological difficulties such as these have led many risk analysts to abandon a (strong) frequentist conception of risk.

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40 *Supra* note 38.

Risk subjectivism

Risk subjectivism construes risk as solely a function of confidence or subjective belief. In his discussion, Thompson rightly notes that this view has been widely adopted by the risk community in favor of risk objectivism. This is due, in part, to the fact that it seems to eliminate some of the difficulties associated with the frequentist view. As Thompson points out, "an important proportion of our concern about risk arises from the possibility of events to which we cannot assign meaningful [frequentist] probabilities."[41]

In his analysis of risk subjectivism, Thompson puts forth a number of criticisms against the use of subjective probability in risk analysis. A major problem with risk subjectivism, he says, is that the "Bayesian decision maker's estimate of risk can never be wrong." And while probabilities should be revised in light of new evidence or information, such information does not in any way "invalidate" the prior probability assignment. For these reasons, Thompson concludes that "there can be no possibility of error in the subjectivist account of risk."[42]

I have two observations to make with regard to this series of comments. First, Thompson's comments presuppose that there are such things as "right" or "wrong" risk estimates. As I mentioned earlier, this view is difficult to defend on a number of counts, not the least of which is that it presupposes a deterministic view of the world.

Second, Thompson seems to fundamentally misstate the Bayesian position. At the heart of Bayesian theory is the concept of *induction,* i.e., how beliefs change in light of new evidence or observations. *Bayesian conditionalization,* in general, and Bayes' rule, in particular, are *normative* rules for updating probabilities in light of new information. As such, the claim that Bayesian updating somehow

41 Thompson, *supra* note 2, at 25.

42 *Id.* at 20-21.

"invalidates" prior probability assignments is fundamentally at odds with the concept of belief revision, which, I think it fair to say, is not only the cornerstone of Bayesian theory, but is also the basis of most accepted precepts of rational thinking.

As Thompson's remarks demonstrate, the *dynamics* of belief revision is often a source of misconception. In many real-world applications, probabilities change or evolve over time; they are neither constant nor fixed. When viewed in this way, new or updated probabilities are not necessarily "better" than the old probabilities. How we ultimately define "better" is an entirely *subjective* matter. In this respect, it is possible to define *rationality criteria,* e.g., logical consistency and probabilistic coherence, relative to which we can make value judgments of this sort. If, e.g., we adopt the concept of coherency as a rationality criteria, then we can make the following sorts of statements: "A coherent set of beliefs is preferable to an incoherent set of beliefs." Only in the context of such criteria can we reasonably assert that one set of beliefs is "better" than another.

Thompson's motivation, it seems, in putting forth this view is to leave open the possibility of error in probabilistic risk estimation; this, in and of itself, is not an all together unreasonable desideratum. Unfortunately, his criticisms of the Bayesian view end up having the exact opposite effect. The reason for this is simple: If we do not use Bayesian conditionalization to introduce new evidence, then, by Thompson's account, we fail to "invalidate" our prior estimate. In this way, we can *never* be "wrong." The inadequacy of such a view is, of course, self evident.

The hinterland between objectivism and subjectivism

The notions of objectivity and subjectivity have been essential elements of the traditional debate between Bayesians and frequentists. The reasons for this are many. First, the notion of objectivity has historically occupied an honorific position in modern science. The lure

of "scientific objectivity" has therefore been something that most statisticians have not found easy to cast aside. In seeming opposition to this view, however, most statisticians agree that largely subjective intuition plays an equally important role in most aspects of scientific inquiry.

In the context of the traditional debate, it is important to recognize, as Efron[43] and others have argued, that there is no *a priori* reason to suppose that objectivity must be thought of in frequentist terms. Observations such as these naturally lead us to conclude that there are good reasons to suppose that the hinterland separating objectivity and subjectivity is not as clearly defined as is sometimes thought. This, in turn, leads us to question the relevance of the traditional debate between Bayesians and frequentists.

To What Extent is the Traditional Debate Relevant?

Writing three decades ago, Savage had the following to say about the traditional debate between Bayesians and frequentists:[44]

> [A] problem which after so many years still resists solution is suspect of being ill formulated, especially since this is a problem of conceptualization, not a technical mathematical problem....

The fact that Savage's remark still rings true today leads us to the following basic question: To what extent is the traditional debate relevant to the foundations and practice of risk analysis?

In general terms, there are at least three reasons to believe that the traditional debate between Bayesians and frequentists is not as relevant to risk analysis as is sometimes thought. The first reason is that, on the whole, there is generally more agreement among Bayesians and frequentists than is usually acknowledged. Most Bayesians, e.g., agree

43. Efron, *Why Isn't Everyone a Bayesian*, in READINGS IN UNCERTAIN REASONING (G. Shafer & J. Pearl, eds. 1990).

44 *Supra* note 38, at 15.

that their goal is to estimate *objective* probabilities from frequency data. Frequentists, naturally, rely on frequency data, while Bayesians, on the other hand, advocate using subjective prior probabilities to improve the estimates. In a similar fashion, objective Bayesians attribute nonfrequentist, but objective, interpretations to prior probabilities. We see, then, that in some respects it is reasonable to assert that the rift that is said to divide these two camps is not as large as is sometimes thought.

The second reason is that, given the individual successes that the Bayesian and frequentist paradigms have been able to claim, it is patently unreasonable to suppose that one of these paradigms will someday be abandoned at the expense of the other. In the past decade, e.g., Bayesian probability theory has gained increased acceptance and prominence, due, in part, to its successful application in fields such as artificial intelligence and decision analysis. Similarly, frequentist statistics occupies an important position in many aspects of scientific inquiry. In the analysis of large and messy data, e.g., frequentist methods have proved to be invaluable tools for practicing scientists and engineers. Given this state of affairs, what seems to be the most likely result of the traditional debate is some sort of Bayes/non-Bayes compromise. Such a compromise would, in principle, serve as the basis for a unified view of probability.[45]

The third reason for de-emphasizing the relevance of the traditional debate between Bayesians and frequentists is that reasonable arguments can be made for adopting an *instrumentalist* view of both paradigms, thereby conceding that for certain problems, one paradigm may be preferable to another. From this vantage point, the Bayesian and frequentist paradigms can be seen as providing a powerful set of

[45] Good is perhaps the most vocal proponent of a Bayes/non-Bayes compromise. *See, e.g.*, GOOD THINKING, *supra* note 20 and Good, *The Interface Between Statistics and Philosophy of Science*, 3 STATISTICAL SCI. 386 (1988).

analytical tools for a wide range of problems and applications. This view is not novel, and has been put forth before in the literature. C. N. Morris captures the essence of this view in the following passage:[46]

> [P]ractical statisticians encounter a variety of problems, and frequency, objective Bayes, subjective Bayes, and empirical Bayes methods provide a range of possible responses. There can be no clear victory for *any* approach for *all* applications; rather, we should train statisticians for a frequency-Bayes compromise so that they can more flexibly respond to new situations.

Adrian Smith, in advocating a pluralistic view of probability, has voiced a similar opinion. He writes:[47]

> Any approach to scientific inference which seeks to legitimize *an* answer in response to complex uncertainty is... a totalitarian parody of a would-be rational human learning process.

In general, I applaud Professor Thompson's criticism of the positivism that has seemed to pervade much of the professional talk about risk in recent decades. My views differ most notably from his, however, in the sharp distinction that he draws between risk objectivism and risk subjectivism. By creating this distinction, Thompson puts entirely too much emphasis on the probability *numbers* that risk analyses ultimately give rise to. As an alternative view, I propose a conceptual reconsideration of the role of probability theory in the analysis and management of risk: *Instead of focusing on probability numbers per se, probability should be viewed more as a general framework for structuring our reasoning about risks.*

If we accept the view that the *structure* that probability theory lends to the risk analysis process is, on the whole, much more important than

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46 Morris, *Comment* [on Efron's 'Why Isn't Everyone a Bayesian?'], in READINGS IN UNCERTAIN REASONING 28 (G. Shafer & J. Pearl, eds. 1990) (emphasis added).

47 Smith, *Comment* [on Efron's 'Why Isn't Everyone a Bayesian?'], in READINGS IN UNCERTAIN REASONING 30 (G. Shafer & J. Pearl, eds. 1990) (emphasis in original).

the *numbers* themselves, then our *understanding* of the various meanings of probability is paramount to its responsible use and application. In most situations of interest, our interpretation of probability can influence how we interpret and approach specific problems. To aid in this understanding, the risk analysis community needs to focus more of its attention on simple problems that make the conceptual and pragmatic distinctions between Bayesian and frequentist approaches clear and understandable. Only in this way will we be able to arrive at a unified view of the role of probability in risk analysis.

Conclusion

In this paper I have considered various issues relating to the cognitive status of risk. Throughout the discussion, I have sought to integrate ideas and concepts from a number of subjects, including philosophy of science, risk analysis, and statistics. A good portion of the discussion has centered around the various ways of approaching the question, "When are risks real?" My own approach to this question has been threefold in nature. First, I began by examining the various ways in which our views of science and scientific theories influence the way we think and reason about risk. I also discussed how these views must ultimately be squared with the cognitive objectives of risk analysis. Second, I argued that the principle of causality plays an essential role in achieving the cognitive objectives of risk analysis, and, moreover, that causality is best construed in probabilistic terms. Finally, I argued that probability theory plays an inevitable structural role in the analysis and management of risk. As part of this discussion, I tried to put in perspective the traditional debate between Bayesians and frequentists, and I offered reasons for why the debate is not as relevant to risk analysis as is sometimes thought.

What, then, are we left with as an answer to the question, "When are risks real?" From the discussion above, it should be apparent that there is no simple, unified, or all-encompassing answer to this question. On a fundamental level, I have tried to show that there are, in fact, a plurality of approaches to the question, and that each approach has associated with it various philosophical and methodological ramifications. Perhaps, in the final analysis, both political and ethical consideration lead us to the realization that "real" risks are those risks that are somehow worthy of consideration or action — a view that Thompson calls *normative,* and one that Paul Feyerabend succinctly captures the essence of when he says that "We decide to regard those things as real which play an important role in the kind of life that we prefer."[48]

[48] P. K. FEYERABEND, 1 REALISM, RATIONALISM AND SCIENTIFIC METHOD xiii (1981).

Editor's note: Professor Thompson's reply will appear in the next issue.