# Epistemology in the Courtroom: A Little "Knowledge" Is a Dangerous Thing

Core epistemological questions—questions about what we know, how we know it, and when we are justified in saying we know it—have a long and deep history. The US Supreme Court broached the subject in the 1993 decision *Daubert v Merrell Dow Pharmaceuticals, Inc*, with references to Hempel, Popper, and other scholars.

We comment here on the articles of Rothman and Greenland, who are scientists, and Haack, who is a philosopher. Their views suggest that questions of causation are neither as simple nor as difficult as many scientists and philosophers have made them. (*Am J Public Health.* 2005;95:S13–S15. doi: 10.2105/AJPH.2005.061838.)

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**ROTHMAN AND GREENLAND,** who are nonphilosopher scientists, and Haack, a nonscientist philosopher, address problems at the nexus of science and philosophy broached by nonphilosopher, nonscientist jurists in the decision of *Daubert v Merrell Dow Pharmaceuticals, Inc*, and its progeny. It is no wonder there is confusion. The core epistemological questions—questions about what we know, how we know it, and when we are justified in saying we know it—have a long and deep history. But both articles suggest that perhaps the practical problem is not as difficult and abstruse as many philosophers of science have made it out to be.

Rothman and Greenland as well as Haack emphasize the experiential basis for the concept of causation. Rothman and Greenland call our attention to the common experiences of causation with which we are all familiar. Their example is turning on a light switch. Such "psychological" origins of our notion of causation have been observed many times before, including in the specific context of "legal" causation. For example, H.L.A. Hart and Anthony Honoré noted the following in 1959:

> Human beings have learnt, by making appropriate movements of their bodies, to bring about desired alterations in objects, animate or inanimate, in their environment, and to express these simple achievements by transitive verbs like push, pull, bend, twist, break, injure. The process involved here consists of an initial immediate bodily manipulation of the thing affected and often takes little time.[1]

So far, so good. Rothman and Greenland go on to point out, however, that these simplified idealizations of causation have to be modified when there are additional factors involved. For example, flipping the light switch might not turn on the lights if the bulb is burned out. We are thus led to a multi-causal picture. Interestingly, it is close in spirit and even in letter to a venerable legal definition of a "cause-in-fact" as a "but-for" criterion; for example, *"but-for"* the exposure, the plaintiff would not have contracted cancer. Rothman and Greenland's extended version also can be found in the literature of jurisprudence (the philosophy of legal concepts), where it is known as a NESS condition, that is, a Necessary Element of a Sufficient Set.[2]

Haack's account is somewhat different, although it also stresses the continuity with everyday versions of knowing. Some of the differences from Rothman and Greenland can be traced to the fact that Haack addresses the somewhat different question of reliability. Whereas Rothman, an epidemiologist, is primarily concerned with what causation means to an epidemiologist, Haack is concerned with how a belief can be properly classified as knowledge, that is, as a reliable basis for supporting an opinion. She identifies it as a key question in the admissibility of scientific evidence, although the *Daubert* court did not clearly articulate this, equating, as she observes, reliability of opinion with scientific methodology. This led the *Daubert* majority to become entangled in matters of what makes something "scientific." Her account of the confusions, misconceptions, and attempts to fuse contradictory philosophies is a cautionary tale of what happens when lay people try to opine on technical matters of another discipline, in this case jurists holding forth on the philosophy of science. According to Haack, good philosophy of science is possibly somewhat helpful to jurisprudence; bad philosophy of science definitely can be harmful. *Daubert* is a case in point.

Haack also demonstrates with wit and clarity that the ultimate goal of the *Daubert* court, that is, "to craft a crisp test to identify genuine, and hence reliable, science," was a mirage; to date, no satisfactory criteria cleanly demarcating science from nonscience have been proposed, and philosophers of science in recent years have generally eschewed the project. This may come as a surprise to many scientists, who assume that philosophers have long since settled these matters. But as Haack shows in her article, this is far from true.

Haack's own approach has the flavor of Rothman and Greenland's. She quotes with approval Einstein's comment that scientific inquiry is "nothing but

a refinement of our everyday thinking."

> Every kind of empirical inquiry, from the simplest everyday puzzling over the causes of delayed buses or spoiled food to the most complex investigations of detectives, of historians, of legal and literary scholars, and of scientists, involves making an informed guess about the explanation of some event or phenomenon, figuring out the consequences of its being true, and checking how well those consequences stand up to evidence. This is the procedure of all scientists; but it is not the procedure only of scientists.[3]

Haack emphasizes, however, that the continuity between science and nonscientific ways of thinking is not in terms of the content of knowledge but in the *ways* of knowledge, making the problem of demarcating scientific knowing from other types of knowing "a distraction," a distraction to which the *Daubert* court fell prey in a significant way. This is not to say that there aren't many kinds of nonscientific inquiry that fall far short of what most of us would call reliable inquiry, but only that the same might be true of inquiry properly called scientific. Legal reasoning is not scientific reasoning (because it doesn't inquire into matters normally called "scientific"), but this does not mean that it is unreliable. It still uses all the tools of rational inquiry and discourse and may be good or not so good.

Thus, it is at the locus of viewing everyday experience as the exemplar of important scientific and nonscientific concepts like "cause" that Rothman and Greenland and Haack come together. This is often referred to as a naturalistic as compared to a rationalist approach. Here is how the philosopher of science Fred Dretske once contrasted these views:

> There are two ways to think about knowledge. One way is to start, so to speak, at the bottom. With animals. The idea is to isolate knowledge in a pure form, where its essential nature is not obscured by irrelevant details. Cats can see. Dogs know things. Fido remembers where he buried his bone. That is why he is digging near the bush. Kitty knows where the mouse ran. That is why she waits patiently in front of the hole. . . . This . . . is one way of thinking about knowledge. Call it the *bottom-up* strategy. It appeals to those philosophers who seek some naturalistic basis for . . . integrating philosophical questions about knowledge, perception, and memory with scientific concerns about the psychological and biological mechanisms for implementing our cognitive transactions with the world. [From the evolutionary point of view, knowledge] is what animals need in order to coordinate their behavior with the environmental conditions on which their survival and well-being depend. . . . There is, however, another strategy, something I will call a *top-down* approach to understanding cognitive phenomena. It takes its point of departure from Descartes, from traditional worries about skepticism, from the normative considerations that dictate proper methods of inquiry and appropriate standards for belief. White-frocked scientists, not furry pets, are the exemplars, the models, the ideal. Patient and objective inquiry, supported by observation, testing, and experiment, leading (if one is lucky) to confirmation of one hypothesis over its rivals—*that*, and not brute perception, is the yardstick to be used in taking the measure of knowledge. Knowledge is what you get when you conduct inquiry, and fix belief, in *that* way. The rest is, at best, true opinion or an inferior grade of knowledge.[4]

Among non-naturalist philosophers, the views of Karl Popper particularly deserve mention, because they have gained much notice among epidemiologists. Popper believed that proper scientific method did not consist in trying to confirm a theory, but rather in repeated attempts to refute it. After all, the proposition that all ravens are black can never be confirmed (what about the next raven?), but it can easily be refuted by exhibiting a single nonblack raven. The following is a common version, as depicted by philosopher of science Robert Klee:

> First a theory is boldly conjectured—free-form, as it were—and then an observational prediction is deduced. An experiment is then designed with the express purpose of showing that the prediction will not be fulfilled. If that happens, the theory from which the prediction was deduced must be wrong. On the other hand, if the prediction is borne out by the experiment, then the theory from which the prediction was deduced is not thereby confirmed, but merely corroborated (that is, it has survived one attempt to falsify it).[5]

But then Klee added: "This would be a happy tale *if only nature cooperated in the right way.*" (Emphasis mine.) This is because no hypothesis tests only one proposition but rather a whole constellation of them, including the nature of the measuring instruments, observations, and experimental conditions. These are all "auxiliary hypotheses" that are tested right along with the main question. This is true even in the simple case of black ravens. As epidemiologists know only too well, the apparent refutation is more than likely to be greeted with the retort, "You call *that* a raven?"

Indeed, the late Willard van Orman Quine, one of the most distinguished philosophers of the 20th century, emphasized something all scientists, judges, and juries are much too familiar with, that is, any seemingly disconfirming observational evidence always can be accommodated to any theory. This is the Quine-Duhem thesis,[6] now known as the underdetermination of theory. The accommodations are made by altering or adjusting auxiliary or background beliefs so as to "save" the apparently disconfirmed evidence. (Quine regarded refutability as a "virtue" that was usually present "in degrees." It was one of five "virtues" identified by Quine and Ullian as desirable features of hypotheses.)

The fact that observational evidence *under*determines theory or theory-based explanation is not just an artifact of "data gaps," but rather a deep feature of science and scientific practice. Quine was clear, however, that the thesis does not imply that "anything goes." Although theories can always be "adjusted" to accommodate disconfirming evidence, the costs of such adjustments in terms of the complex of background beliefs that would be altered or lost may be too high. Not all alternative explanations or theories have equal status. As a pragmatist, Quine was guided by the extent to which alternative hypotheses maximize desirable virtues such as simplicity, fertility, conservatism, modesty, and refutability. Quine depicted the framing of "opinions" (i.e., hypotheses) as attempts to "explain some otherwise unexplained happenings by inventing a plausible story, a plausible description or history of relevant portions of the world."[5] This mirrors closely the actual practice of scientists who, although they may think of themselves as Popperian falsificationists, act as Quinean pragmatists.

Quinean pragmatism is but one of many contemporary

strands in the philosophy of science. Not at all popular today are Popperian falsificationism or inductivism, which are incompatible with each other and with real-world scientific practice, despite the *Daubert* court's reliance on just these top-down doctrines as justification, and despite Popper's popularity among some epidemiologists. As Klee remarked, "A sensitive appreciation for the epistemological impact of the underdetermination of theory is essentially what killed Popperian falsificationism as a plausible model of science."[5] No single standard has taken its place.[6] In the 1960s, in the wake of Kuhn's watershed book, *The Structure of Scientific Revolutions*, the philosophy of science took a historicist turn that has deeply influenced current thinking. The renewed interest in science as it is actually practiced, in the form of historical or even anthropological case studies, has brought a new appreciation for the complexities of the question of what makes something "scientific."

Rothman and Greenland end their article with an important coda rejecting any notion that causal inference can be performed "by the numbers" by using a checklist such as the so-called Hill criteria, so beloved of lawyers seeking a bright-line standard for causality. Scientific reasoning is no more susceptible to a mechanical approach than legal reasoning. As Rothman and Greenland say in their article:

> If a set of necessary and sufficient causal criteria could be used to distinguish causal from noncausal relations in epidemiologic studies, the job of the scientist would be eased considerably. With such criteria, all the concerns about the logic or lack thereof in causal inference could be forgotten: it would only be necessary to consult the checklist of criteria to see if a relation were causal. We know from philosophy that a set of sufficient criteria does not exist. Nevertheless, lists of causal criteria have become popular, possibly because they seem to provide a road map through complicated territory[7]

Haack, too, flirts with a top-down approach by identifying some ideal form of inquiry as the hallmark of reliable knowledge, albeit not identifying this kind of inquiry solely with science. One is led to suspect that a completely naturalistic approach to key concepts such as justified knowledge and causation will not satisfy many people, including judges.

Rationalism is still an important element in most people's notion of establishing reliable belief. The problem comes in trying to explain exactly what that means. Haack resorts to a vivid metaphor of solving a crossword puzzle, with its interlocking pieces of evidence and obscure and indirect clues. But this is a metaphor or analogy rather than an algorithm; it is certainly not a test for true inquiry. Rothman and Greenland are even vaguer, stipulating that one establishes the validity of a study by soaking it "in the brine of criticism," which, I guess, still leaves us in a pickle. ■


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