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INTERVIEW

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Interview with Susan Haack, Professor at the University of Miami, for „New Books in Philosophy"

Hosted by Robert Talisse, Professor of Philosophy at Vanderbilt University

September 19, 2014[1]

Welcome to the New Books in Philosophy Channel of the New Books Network. My name is Robert Talisse. I am Professor of Philosophy at Vanderbilt University. I co-host the channel with Carrie Figdor, Associate Professor of Philosophy at the University of Iowa. Today my guest is Professor Susan Haack. Her new book is titled *Evidence Matters: Science, Proof, and Truth in the Law*. It has just been published by Cambridge University Press. Haack is Distinguished Professor in the Humanities, Cooper Senior Scholar in Arts and Sciences, Professor of Philosophy, and Professor of Law at the University of Miami.

Our legal systems are rooted in rules and procedures concerning the burden of proof, the weighing of evidence, the reliability and admissibility of testimony, and much else. It seems obvious, then, that the law is in large part an epistemological enterprise; yet when one looks at the ways in which judges have wielded epistemological concepts, there is plenty of room for concern. In *Evidence Matters* Susan Haack brings her skill as an epistemologist to bear on a series of tangles concerning the legal concepts of proof, evidence, and reliability, especially as they apply in a series of notorious toxic tort cases. Along the way she exposes several philosophical confusions in the law's current understandings of the epistemological concepts that it must wield, and she shows how her own distinctive epistemological theory, foundherentism, can be useful to the law. *Evidence Matters* is an engaging read and a truly impressive interdisciplinary accomplishment. So let's turn to the interview.

Robert Talisse (RT): Hello, Susan Haack.

Susan Haack (SH): Hi, Bob.

RT: How are you doing today?

SH: I'm fine.

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1 Transcription by Alina Hernandez.

RT: Well great, thank you for joining us on New Books in Philosophy.

SH: You're very welcome.

RT: And thank you listeners, for tuning into our podcast. Today I'll be talking with Susan about her new book *Evidence Matters: Science, Proof, and Truth in the Law*. It's a fascinating examination of a series of interlocking philosophical confusions, as we might call them, in the law regarding matters straightforwardly epistemological: evidence, expert testimony, the admissibility of scientific data into evidence. As Susan puts it, "the law is up to its neck in epistemology"; and the book shows this quite well. However, as Susan also argues, the law is in an epistemological mess. Her positive project, naturally, is to clear things up; and those of you who know her work will probably agree that there is almost nobody better suited to clearing things up than Susan Haack! So there's a lot to talk about—and this is a book that I highly recommend to philosophers who are interested in epistemology, social philosophy, philosophy of law, or political theory.

But why don't we begin with the author, as we usually do. Susan, why don't we have you tell us a bit about yourself and how you came to this project.

SH: OK. Well, your listeners can hear this, so I don't have to tell them I was born, brought up, and educated in Britain. I think my first encounter with philosophy was when I picked up Richard Robinson's book, *An Atheist's Values*, from the library shelf while I was still a schoolgirl. I found it fascinating; but I think if someone had told me it was philosophy, I would have been surprised—I didn't know what philosophy was! Well, eventually I went to Oxford—at the instigation, I might say, of an inspiring history teacher whom a few years ago I saw again for the first time in many, many years—and was delighted to discover he had had a very successful career, and shared my enthusiasm for Dorothy Sayers. That was wonderful.

I was the first person in my family to go to university, so Oxford was kind of shocking, but also very exciting. I read politics, philosophy, and economics, thinking that politics is really interesting, and soon discovered that philosophy was even *more* interesting. So, that's how I got hooked.

My earliest work was in philosophy of logic, with *Deviant Logic*[2] and *Philosophy of Logics*.[3] I have a suspicion that part of the reason I was attracted to this was that, at that point in time, a woman who did philosophy was supposed to do ethics—that was supposed to be what we were good at. And the fact of the matter was that I

2 *Deviant Logic* (Cambridge: Cambridge University Press, 1974; second, expanded edition, Chicago: University of Chicago Press, 1996).

3 *Philosophy of Logics* (Cambridge: Cambridge University Press, 1978).

thought, and still think, ethics is really *hard*. By contrast, I thought, logic is nice and clean; I can do *that*!

Well, in due course I started teaching at the University of Warwick; and one of the first things I had to teach was a year-long class in epistemology and metaphysics, which was how my work in those areas began. Then, after I had been working in those areas for a while, I realized that there were applications of this stuff in philosophy of science; which was how I wrote the next large chunk of work, *Defending Science—Within Reason*.

And then, by a process that shows you the role of opportunism in academic life, I got drawn into the law. How? Well, I went to a Law School party at the University of Miami, for no other reason than that it was in honor of William Twining, who had been Chair of the Law Department at the University of Warwick when I was in the Philosophy Department there—we had been friends, and had actually taught a course on pragmatism together. (Well, he gave the class on Holmes and I gave the rest of it.) So I went to this party, and fell into conversation with somebody else who, it turned out, taught a class on the Analysis of Evidence. I was intrigued, and asked him: what materials do you use? And he said, "Well, Wigmore, blah, blah, blah, Twining, sure—oh, and we use your book, *Evidence and Inquiry*."[4] So I asked him: where's your office? I'll come talk to you. A few days later I showed up in his office, and he gave me an enormous heap of cases and law review articles—neither of which I knew how to read, of course; I had to learn how to read these things.

RT: Oh, sure.

SH: I think he thought he'd never see me again; but six weeks later I was back, saying, "OK: that was fascinating. I have theories; you have cases I couldn't imagine—they're so complicated, so tangled, my fingers are itching. Can I talk to your students?" And that's how it all began.

RT: Wow.

SH: Good story no?

RT: That's wonderful.

SH: Yes; my thanks to Terry Anderson and William Twining, who got me into this!

4 *Evidence and Inquiry* (Oxford: Blackwell, 1993; second, expanded edition, Amherst, NY: Prometheus Books, 2009).

RT: By throwing a party.

SH: By throwing a party, yes.

RT: Well, that's excellent. Why don't we pick up there because it does make a nice segue into talking about what's sort of a backdrop of the book. So, the law is up to its neck in epistemology, and you are the author of a very influential epistemology book and a theory with an unforgettable name, "foundherentism." Can you give us the background to the broader project in epistemology that your work is tracking? And then we'll get to talk about how, in *Evidence Matters*, the ongoing attempt is to show that your epistemic theory enables us to say sensible things about the legal concepts that seem epistemic in nature.

SH: OK, I think I should probably start by saying that I saw it the other way round: these cases, with their enormously complex evidence, seemed to me like a really good test of my theory. I was prepared for the upshot: "uh-oh, I've got something horribly wrong"; and I did in fact have to make some refinements and some modifications as I went along.

OK, that said: I'd say my epistemological work has two strands. The first concerns the theory of what makes evidence better or worse, beliefs more or less justified, claims more or less warranted; that's where the word "foundherentism" applies. (It has proven, like Peirce's "pragmaticism," ugly enough to be safe from kidnappers, which is probably a good thing—sometimes people ask, "Couldn't you have thought of a better word?"; and I say, "Well, can you?" and the best they can come up with is "codationalism," which is just as bad!) Anyway, let me say that the initial thought there was that epistemology, as it was then being conducted, was awash in false dichotomies. As I'm sure you're are aware, this is a very pragmatist thought; my sensitivity to false dichotomies is distinctly pragmatist in orientation. Among those false dichotomies, I came to believe, was the traditional rivalry between foundationalism and coherentism. It seemed, and it still seems to me that both families of theories had something right and something wrong, and that it should be possible to combine the two—to combine the strengths of the two, while losing the weaknesses of both. That's what the foundherentist theory tries to do. It allows a real, legitimate role to people's experiences of the world, as some forms of foundationalism do, but coherentism, I believe, can't. But it also allows a serious role to what I see as the pervasive mutual support among our beliefs, which coherentism does, but foundationalism, I believe, can't, not without losing its foundationalist character.

There's an analogy I used as a tool in developing the details of this theory, the analogy between the structure of evidence and a crossword puzzle. I want to emphasize that this is *only* an analogy; there are, of course, disanalogies: unfortunately, the right answers aren't in tomorrow's paper, and I don't believe that there is someone who designed this crossword puzzle—not to mention we're dealing with something a good deal larger than the "largest crossword puzzle in the world" that they sell in airline magazines! So it's only a tool: the theory has to stand on its own feet (people have sometimes misunderstood this); but I have found it a very useful tool.

I should also stress that this theory is, from the beginning and all the way through, a gradational theory—I might say "synechistic," if there are any Peirce scholars listening. That again is the pragmatist influence, I guess. I'm very sensitive to the fact that evidence can be better or worse, and that justification comes in degrees—you aren't either justified or not. That is by the way, the root of what I have to say about the Gettier paradoxes, which I really wish would just go away; but that's enough.

RT: You and, I think, a lot of other people, including a lot of epistemologists at this point I should hope.

SH: One would hope, but it seems those paradoxes never die. Now, the other strand has to do to with the conduct of inquiry, and especially with epistemological character, about which I have written a good deal. What I have written about epistemological virtues and vices doesn't have a great deal to do with *Evidence Matters*; but what I've written about the distinction between genuine inquiry and pseudo-inquiry does have a significant role to play in this new book. And then, of course, also influential in this book is the work that I have done in philosophy of science, in the theory I call in *Defending Science*[5] "Crticial Common-sensism." I think we can talk about this as we go along, probably.

RT: Sure. Before we go on to introduce some of the things distinctive to this book, can you just give us a quick run-through—I'm sure a lot of the readers understand what kind of tool the crossword puzzle analogy is and how it plays out, but maybe not all of them do.

SH: OK.

5 *Defending Science—Within Reason: Between Scientism and Cynicism* (Amherst, NY: Prometheus Books, 2003).

RT: Could you just tell us how you use the analogy to talk about the familiar epistemic issues about justification and belief and the rest.

SH: Yes, sure, OK. I can tell you how it all started.

RT: Oh, that would be wonderful.

SH: It started when I was on sabbatical in Canberra. We had no television, so in the evenings what were we going to do? We read the airmail edition of the *Manchester Guardian* and *Le Monde*, which had an absolutely ferocious crossword puzzle. So for the first time in my life I was regularly doing crossword puzzles, and at the same time I was writing some of the early stuff about epistemology. And I remember waking up in the middle of the night one time and saying, "Oh my goodness, *of course* the foundationalists are wrong; there *can be* legitimate mutual support— just look at a crossword puzzle!" That was the first thought, that there's legitimate mutual support; it's everywhere in a crossword puzzle. And then the second thought was: hey, the clues are the analogue of sensory evidence, and the already-completed entries are the analogues of the background beliefs that serve as reasons for other beliefs. And then I began asking myself, well, what makes a crossword puzzle entry more or less reasonable? And the answer was: it depends on how well it fits with the clue and any other entries that are already filled in (that's the analogue of what I call supportiveness of evidence); it depends on how reasonable those other entries are, independently of the one you're asking about (that's the basis of the independent security requirement); and it depends how much of the crossword puzzle you've done (which is the basis of the comprehensiveness requirement). So it sent me to a multidimensional account of the determinants of the quality of evidence. Clearly enough, because it's multi-dimensional, this account won't necessarily give you a linear ordering, which turned out to be important when we get to some legal issues.

RT: Right, and it's that last condition, how much of the evidence is taken into account, at this point that seems a real insight and kind of distinctive, I take it; it's still common to see epistemologists not give that consideration its due, it seems to me.

SH: I also think they get somewhat hung up on supportiveness.

RT: Right.

SH: They're not always as conscious as I think they should be about independent security; and on the whole as far as I know, have very little interest in comprehensiveness, which I think is a very difficult concept. Spelling it out is a nightmare.

RT: Right.

SH: To this day I haven't completely succeeded; but it's a very important concept, I believe. I think it also explains why, when Donald Rumsfeld was doing his thing about the unknown unknowns—do you remember that?

RT: Yes.

SH: Everybody else in the country was laughing derisively; but I was saying: wait a minute, I think he actually has an epistemological point. We don't always know what's relevant; and that means that we don't always know what we don't know that we ought to know.

RT: That's right. So let me just say for the record you're not the only one who thought that this was actually a smart thing to come out of Rumsfeld's mouth.

SH: OK, good.

RT: I report to you that right after it was reported that he had said that, I had a long conversation with my epistemology colleague, Scott Aikin, about how important that three part distinction was and then we looked in horror as uh ….

SH: … Everybody was laughing.

RT: Yeah, everybody thought it was some kind of confused joke, but it actually seemed to me to be one of the places where a politician got epistemology right.

ST: That's right. I've actually been so provocative as to call it the "Rumsfeld Insight."

RT: That's right. Well great, let me move on, that's very helpful. Before we get into the thicket of the legal stuff, let's me move on then to ask about just one sort of question in one important early chapter in the book, that I think epistemologists, as such, even if they might not care about the law at all, might be interested in hearing you address. The target in the second chapter or the third chapter is "legal probabilism," which is a particular view about how to understand what you were talking about a moment ago, which is the degree of warrant for a claim. Apparently the idea is rampant in the law that degrees of warrant are analyzable in terms of mathematical probabilities; and you make I think some really interesting and compelling arguments to show that no, degrees of warrant, or epistemic degrees of warrant are not the same thing as mathematical probabilities. Can you talk to us a little bit about some of those arguments? I thought they were very, very good.

SH: OK: I think the first thing to say is that the issue is really about degrees of proof. When you are talking about the law, the confusion is about how to understand degrees of proof and, of course, standards of proof: "beyond a reasonable doubt" in criminal cases, "by a preponderance of the evidence" in civil cases. And, as you know, sometimes the preponderance standard is actually expressed in terms of the word "probable," as "more probable than not." Now, my first thought is this. The word "probable" in English has more than one meaning. I like to joke that I am bilingual in British and American English (but my American accent is still terrible)! But whenever an issue like this arises, I always go *both* to the OED *and* to Merriam-Webster's Dictionary, because there *are* differences between the two languages. But both of them say very clearly: the word "probable" has these two quite different uses, one of which is to refer to the degree of warrant of a claim by evidence, and the other of which is to refer to this mathematical concept which is used in games of chance, in statistics, and so on and so on. Now, by looking at two things—first, jury instructions, that's to say the instructions that judges give to explain to jurors about how to understand degrees of proof (because it is the jury that has to decide whether the evidence presented meets the standard of proof), and second, the reasons for having the standards of proof at all—both clearly indicate that what we are talking about here is degrees of warrant, and not mathematical probabilities.

That degrees of warrant aren't mathematical probabilities, that you can't equate the two, of course requires a theory about degrees of warrant; and the one I use is, naturally, mine. If I thought it was wrong, I would dump it and start again—I'd probably groan so loudly you could hear me from where you are without Skype, but if I had to, I would! Still, when I started to think about this really carefully, I found myself arriving at the conclusion that degrees of warrant have a very different logical profile from mathematical probabilities; and in the process of working this out, I think I rediscovered some arguments that were already to be found in Jonathan Cohen's work, though I'm afraid I read that only later. One was simply this: the mathematical probability of p and of not-p must add up to 1; but if you have no evidence, or only really feeble evidence, either way, then the degree of warrant of p and the degree of warrant of not-*p* doesn't amount to anything. Neither of them is warranted to any degree. You understand?

RT: Yeah, yeah.

SH: OK: the second thought was that, for independent *p* and *q*, the mathematical probability of *p* & *q* is the product of the probability of *p* and the probability of *q*; which is always, unless they are both 1, less than either. But, as I argue in a different part of the book, and as I think is very clear from a lot of common-sense examples, the weight of the warrant given to a claim by several independent pieces of evidence may well be higher than the warrant given to it by any piece individually.

And then the third thought was that, given that there are, if I'm right, three determinants of evidential quality and therefore three determinants of degree of warrant, there is no guarantee of a linear ordering of degrees of warrant; whereas, mathematical probabilities, of course, *do* come in a linear ordering. It's the story of my life: I subsequently discovered—but not until after I sweated several nights working it out from my theory!—that John Maynard Keynes already suspected there was no linear ordering. Anyway, they [degrees of warrant and mathematical probabilities] simply have different shapes.

And now I want to add two things to this, because sometimes I'm misunderstood on both. (i) This is not in the least to say that statistical evidence of one kind of another doesn't play a very important role in the law; of course it does. It's only to say that its role is as one piece of evidence among others; it doesn't simply swallow the whole idea of degree of proof. And now, of course, I forgot what the other thing was. Why don't you keep going, and it will come to me!

RT: Sure. So, that's helpful. A large chunk of the book is focused on various ways in which a series of toxic tort cases have wielded the concept of evidence and the admissibility of evidence in the case of science and scientific data and testimony, and one of the key themes that keeps returning throughout the discussion in *Evidence Matters* is the series of rulings with respect to a case called *Daubert* and the ways in which *Daubert* has revised our understanding of rules of evidence. So, before we get into what I want to talk about, which is two or three of the real peculiar epistemological confusions that turn up in the law as a result of the *Daubert* ruling, maybe we can ask you to just talk a little bit about the case and how the *Frye* Rule was superseded by *Daubert* and then how this was all impacted at the level of the Federal Rules of Evidence.

SH: Oh, sure, OK. First there was *Frye*. *Frye* is a 1923 criminal case; it was a murder case involving a defendant who at one point confessed—part of the folklore about Mr. Frye is that he thought that if he confessed he would get half the reward for identifying the culprit; I don't know if that's true, but it's a great story! Subsequently, he withdrew the confession; and his attorney had him take what was then a very new and very primitive lie-detector test. And then the question before the Federal Appeals Court in Washington, D.C. was whether this very new scientific testimony should be admitted; and the very short ruling was no, it was too new, and such completely novel scientific testimony should be admissible only if it was "sufficiently established to have gained general acceptance in the field to which it belongs." Courts mostly forgot the "sufficiently established to be"; but in due course the *Frye* Rule, the rest of it, sort of spread across the country—a bit like mildew in my bathroom!—until at one point it was probably the rule in the majority of jurisdictions.

And then in 1975 the Federal Rules of Evidence were ratified; and Federal Rule 702, which was about expert testimony generally (that's to say, broader in scope than *Frye*, but including what *Frye* includes) said that such testimony was admissible provided that it was relevant and not otherwise legally excluded. So for a considerable period nobody really knew whether or not *Frye* had been superseded.

And then the Supreme Court took an opportunity to answer this question in *Daubert*, which was one of a whole raft of cases involving a morning-sickness drug, Bendectin, alleged to be teratogenic (i.e., to cause birth defects). Why did they pick *Daubert*, of all these cases? Well, there was a very clear reason. It was a very, very rare civil case in which the lower court had appealed to the *Frye* Rule in excluding the plaintiff's expert testimony. So the core of the Supreme Court's ruling in *Daubert* is simply that the Federal Rules of Evidence, specifically Federal Rule of Evidence 702, *does* supersede *Frye*. Federally, *Frye* is gone. But Justice Blackman, who writes the ruling for the majority, goes on to say a lot more. So everybody agrees, the court's unanimous, that Federal Rule 702 supersedes *Frye*. Moreover, they're also unanimous that this doesn't mean that judges don't still have a responsibility to screen proffered expert testimony; indeed, under *Daubert* they have a responsibility to screen it not only for relevance, but also for reliability.

But Justice Blackman then goes on to write at length about what that screening for reliability might involve; and it's in those dicta about what might be involved in determining whether or not proffered expert testimony is reliable that all the philosophically exciting muddles come up. I might say I was amused to see that Justice Rehnquist, in a partial dissent, says: Well, of course I agree about *Frye*, and I agree with you about judicial gatekeeping; but I don't understand much of the rest of this—and I suspect federal judges won't, either. That's why I say at the head of one of the pieces in this book that you might think of this as an articulation and a partial defense of some ideas in Justice Rehnquist's dissent.

RT: That's right, so why don't we pick up on that—because one of the things that in the clarification that we get in the majority ruling in the *Daubert* case is this very strange appeal to, of all people, Karl Popper, as the source of a conception of the reliability of data and testimony. Now I take it most of our listeners will already have heard what's peculiar about that, but why don't you tell us a little bit about that.

SH: I think there are two large confusions in the *Daubert* ruling which are probably of interest to your listeners. Justice Blackman starts by saying: "We want this testimony to be reliable. Well, what does that mean? Well, it should be genuinely scientific." Well, that's already a muddle. It's seems to me perfectly obvious when you think about it that *not all scientific testimony is reliable* and *not all reliable testimony is scientific*. So there's a confusion of "scientific" and "reliable." In fact, here you can see the influence of that honorific use of "science," as in, "Do you have any scientific evidence for that?" meaning, "Do you have any good evidence for that?" It's sort of an advertising use of the word, which I would abolish if I had the power, but I don't. OK, that's the first confusion. Well, having made that confusion Justice Blackman casts around for someone who offers a criterion of the demarcation of science—well, yes, Karl Popper. And this is how Popper, who by the way is also pretty thoroughly confused with Carl Hempel...

RT: Well, their first names do sound the same.

ST: Yes, their first names sound the same! And if you don't understand what's distinctive about Popper's philosophy of science, which I don't believe Justice Blackman did entirely, but you know they both say that scientific claims should be testable, and you don't realize that this doesn't mean the same in Hempel's mouth as it does in Popper's, then this is not so difficult a muddle to understand; but of course it *is* a muddle. (I'm afraid I once had terrible trouble with a copy-editor because I joked in one piece that the Supreme Court got its Hoppers and its Pempels all mixed up; the copy-editor kept "correcting" this, and I had to keep kept un-"correcting" it! It took about six tries to get it right.)

Where did the allusion to Popper come from? I know a certain amount about this. There were a lot of amicus briefs in *Daubert* from various people and institutions. The Chemical Manufacturers' Association, the New England Journal of Medicine, etc., etc., all wrote in with ideas about how this decision should be made; and I was quite stunned to discover what terrible, terrible misunderstandings of what Popper actually said there are in many of those amicus briefs. And there were also a couple of articles in the law reviews around the time of *Daubert* which also presented misunderstandings of what Popper actually said. So if you look at the history, if you look at the things that Justice Blackman and the clerks working for him would have been reading, it's not entirely surprising.

There's also another really interesting little historical twiddle here. Popper, as you know, uses the word "corroboration" for the condition of a theory which has been tested, but not yet falsified. In the early days, when Popper's English wasn't as good at it eventually became, Rudolf Carnap, who had had translated some things of Popper's, translated the word Popper had used and would later translate by "corroboration," as "confirmation"—disastrous, of course! Eventually, Popper realized that it was disastrous; and there's a footnote in English edition of *The Logic of Scientific Discovery* which explains that it's disastrous. But, unfortunately, he has an early paper which was reprinted in *Conjectures and Refutations* which contains this mistaken of translation of "corroboration"; and, wouldn't you know, *that*'s the paper the Supreme Court picks up on—the one with this mistranslation of "Bewährung," which was Popper's word, as "confirmation." So there are all sorts of interesting things feeding into this confusion.

I feel kind of ambivalent about it, but it was a very useful exercise, when I wrote about what Popper actually said, and why, if you were looking for a philosophy of science less suitable to explain reliability you couldn't do better than pick on Popper, because I was writing this for a legal journal—where the convention is you say absolutely nothing without a footnote to nail it down—I had to read Popper more carefully than I had ever done in my life; and in this work all the claims he makes which lead me to the conclusion that his philosophy of science is, in the end, a disguised form of skepticism, had to be nailed down by a footnote, and given both a quotation and a very specific reference. And now I feel a certain satisfaction: he can't escape. He really *did* say all those things; and here's where and here's how. It was no fun to do, but I get a lot of satisfaction out of having done it!

RT: Right; and what makes Popper such an implausible sort of anchor for Blackman's sort of criterial interest is it winds up in the legal case not really being a matter of demarcation at all, but a conception of what makes something reliable, and the Popper view is that nothing is reliable.

SH: Nothing in science is reliable, that's right. I once went all the way through *The Logic of Scientific Discovery* looking for the word "reliable." It occurs a few times, but always in scare quotes, as in: I don't care whether a scientific theory is "reliable"—waving his fingers in the air; I care about whether it's falsifiable, how much content it has.

RT: That's right.

SH: Yes, so it's a big muddle. It was interestingly followed up in the Federal Courts. I only found one case where I was absolutely confident the judge had actually read at least one page of Popper. And, interestingly enough, many of the courts who tried to apply this actually misinterpreted Popper in a way that was a good deal more plausible, I think, than Popper's own philosophy of science.

RT: So one of the sort of epistemological confusions that comes out of the series of rulings is that there is a conflation between reliability and properly scientific, and there's also confusion about, you know, how reliability is to be understood. But, later, in the ruling on *Daubert* on appeal, there's this reference to peer review, that that's a marker of the scientific or a marker of its reliability; and you've got a really nice chapter, sort of a note to lawyers, about the peer review system. Can you tell us a little bit about that aspect of the confusion as well?

SH: Sure. This was actually in the *Daubert* ruling itself; and it came from earlier stages of the *Daubert* case. It was sort of built into the Supreme Court ruling from earlier versions, and it comes up again when the case goes back to the Ninth Circuit on remand. The idea was that if scientific evidence is based on work which has survived the process of peer review and publication, then that's an indication of its reliability. The trouble with this thought, I believe, is that "peer review" is ambiguous in a key way. It might refer to the process by which a scientific paper gets accepted for publication, in which case it's relatively easy for a judge to determine whether or not the paper is peer reviewed—*relatively* easy. Or, it might mean that the paper has been published, it's been out there, people have tried to build on the results it claims to have got, and it has survived this process of long-run scrutiny in the relevant scientific community. That's impossible for a judge to determine. There's no way a judge can know whether this work will survive. If he could, scientists wouldn't need to do the work!

And I think that the *Daubert* ruling sort of admits this; but in the process perhaps conveys to some courts the unfortunate idea that you just have to ask whether or not this paper was peer reviewed before publication, and that will give you a sense of whether or not it's reliable. And I wrote that paper in part because, having done a bit of digging around about the history of the peer-review process and how it actually works in the sciences at this point in time, I had reached the conclusion that at least many legal players are very naïve about this process. I suspect some of them may think a peer reviewer actually repeats the experiments reported in the article they're peer reviewing—perhaps having no notion of how much time and how much money that would require; and I think by no means all of them understand how busy the people are who do this peer reviewing, or how much of what they say is a matter of "Look, you've really got to clean up the format, and this table is unintelligible, and, etc." But also, I think, it was important to say: you have to understand not everything that's published in a peer review journal is peer reviewed. Some of the things published in so called "peer review journals" have been invited; and sometimes with a scientific journal an author may asked, "Who would you like to peer review this?" This happened to me when I published in the *American Journal of Public Health*. Well, who would suggest someone who is going to say, "No, don't publish it"?

And I think not many people, at least at the time I was writing this stuff, seemed to have much sense of how the peer review system may be corrupted. There is one very, very interesting ruling in a Bendectin case, a 1986 ruling by Judge Bernstein in *Blum v. Merrell Dow*. I think the Blums must have had really, really good attorneys, because somehow or other they brought to the judge's attention that, for example, one of the journals where a good deal of the stuff was published apparently showing that the drug was harmless was actually edited by someone who'd been on retainer with Merrell Dow for 18 years at the time of this case. And the author of another study that the defendants were citing had written to Merrell Dow asking for support, because he thought this might be useful in court; and so on and so on. So, you get some sense from that case of how, in some instances, a peer-reviewed literature might be created by one of the parties that has an interest in on-going litigation, which is very disturbing.

I might say that, when I taught Scientific Evidence in the Law School last year, I gave the students a little assignment: look at the *Daubert* factors, explain what they are, and tell me which one you think is most helpful; and several of them said, "Oh, I think the one about peer review is the most helpful—that makes sense." And then one of them, when he came to write his terms paper said, "I think I'm going to write about peer review; I think this is a great idea." But a couple of weeks later he showed up at my office saying, "Oh my God, I was so naïve! I've just looked at what really happens. It's not what I thought at all!" And it was he who found a very recent article by a medical scientist, John Bohannon, I believe his name is, in *Science* last year. He submitted 307 versions, I think it was, of a spoof article, manifestly bullshit to anybody with a knowledge of high school chemistry, to 300-odd journals, half of which promptly accepted it. So, I would have to say, it's even worse than I thought it was. The system is, I fear, under immense strain because there is so much pressure to publish and so many people with so little time to do this work, that it's not really the kind of safeguard that we hoped it was. I'm afraid I used to think it was a lot, lot better in the sciences than it is in philosophy. But now I think that was naïve on *my* part.

RT: Right.

SH: Probably better, but not a lot better.

RT: Right, right. One thing that you mention a couple of times in the book is there have actually been cases where because of the comments about testability and peer review, judges have actually let in testimony that's been refuted.

SH: OK. There is one really very funny case—I can't even tell my students about it without cracking up. But, to be fair, it's one of the very, very early post-*Daubert* cases, one of the first cases where a federal court has to apply *Daubert*. It's the same year as *Daubert*; they're still reeling from this big shift in the federal landscape about scientific testimony. It's is a criminal case, *U.S. v. Bonds*, where the testimony the admissibility of which is at issue is DNA identification testimony. This was relatively early in the history of DNA identification testimony: the first time such testimony was admitted in court in this country was, I believe, 1987, in a Florida case. So this court's dealing with two quite new things, *Daubert* and DNA identification testimony. Well, the defendant had argued that there was reason to believe that at this point in time the FBI laboratory, which made the DNA identifications in this instance, made a lot of mistakes. There *were* a lot of things wrong with the laboratory; that was in fact true. But I'm afraid the judge argued: Well, let's see; what *Daubert* says is that we have to look to see whether this testimony can be and has been tested. Well yes; it can be and it has been tested. So OK, it's admissible—ignoring the fact that it had been tested and found to be horribly unreliable! So yes, this is wonderfully ironic. I don't know another case like it, I'm glad to say. I don't think this kind of reasoning was ubiquitous across the legal system.

RT: Well, that is some good news! Well, let's move to some of the later chapters where you're interested in one of the other sort of implications of at least the way in which *Daubert* gets interpreted, that some of the language of *Daubert*, you claim, encourages a kind of atomism about reliability of evidence and precludes what we might say sounds like a more sensible idea, about how different pieces of evidence can combine and the combination of different strands of evidence can result in a greater degree of warrant for a claim than any of the individual elements alone. So you've got a couple of the later chapters which are about this idea of combining evidence, the force of combining evidence particularly with respect to evidence about cause or causation evidence. Could you tell us a little bit about both the sort of atomism of reliability that is encouraged by some of the language in *Daubert* and then some of your own views about this?

SH: OK. I would say it's more a matter of precedents, in *Daubert* and since *Daubert*, than exactly its language. There's nothing in *Daubert* overtly obliging judges to look at proffered scientific testimony piece by piece. However, when the case goes back to the Ninth Circuit on remand, Judge Kozinski does exactly that. He looks at each piece of proffered plaintiffs' expert testimony one by one, saying: "OK, well, this one's irrelevant, this one's irrelevant, this one's irrelevant; this one's relevant, but unfortunately it has no methodology to speak of, so *a fortiori* it doesn't have a reliable methodology—so they're all excluded; summary judgment for the defendants again."

And you see the same thing in the next case in line from the Supreme Court in what's now called the *Daubert* trilogy, which is *General Electric v. Joiner*. In that case the issue about what in those days was called "weight of evidence methodology," otherwise known as "WOE," comes to the surface explicitly. Mr. Joiner worked as an electrician for a city in Georgia. One of his jobs was to take apart those big transformers, clean them, and put them back together again. They were insulated with oil, which stops them from overheating; but the oil turned out to be contaminated with PCBs (polychlorinated biphenyls), which are notoriously carcinogenic—they had actually been banned since 1978, so that wasn't really the issue. Mr. Joiner got lung cancer at the age of 37, which is very young; and believed that this was the result of his occupational exposure to PCBs.

Naturally, there weren't any direct epidemiological studies of the toxic effect of PCBs, because they had been banned for decades at the time of the case; you couldn't ethically conduct such a study—you can't expose some people to this and see what happens, because we already *know* it's toxic. So he produced a congeries of evidence about other studies of things chemically similar to this, a whole big heap of stuff; and the defendants, GE, who had seized on *Follies and Fallacies in Medicine*, a then recently-published book about fallacies in epidemiology, claimed that what Joiner was giving was simply a great big pile of weak evidence; and that if you had a great big pile of weak evidence, that was *all* you had—a great big pile of weak evidence, and it didn't matter how big the pile was, it wouldn't get any stronger. They called this, because this was what the book in question called it (most unfortunately), the “faggot fallacy,” meaning: if you have a whole big heap of twigs, that’s all you’ve got, a whole big heap of twigs; it doesn’t magically turn into something stronger. I think the thought that informed this idea in this book was that if I have one bad epidemiological study, it’s a bad epidemiological study; if I have two bad epidemiological studies, neither of them get any better just because I’ve got two. Right?

RT: Right.

ST: And if I had 37, ditto. That’s true; but it misses the point, which is whether or not combined evidence may, in certain circumstances, give greater warrant to a conclusion than any of the component pieces by itself would do; and, if so, in what circumstances, and why. You understand?

RT: Um uh.

SH: That’s an epistemological question of just the kind to make my fingers itch. I’ll say by way of preliminary that it strikes me, if you think about this independently of cases like *Joiner*, the answer to the first part of the question is “obviously, yes—*of course* combined evidence can sometimes have more weight than any of the components by itself; and, indeed, the law itself acknowledges this in other circumstances. Did the defendant have means and motive, and opportunity? Well, that’s a lot better than if you just have only the one. So, we get acknowledgment of this increased weight of combined evidence elsewhere in the law, but somehow or other the legal system has difficulty with it in these cases where the key issue is about causation.

So all I do is put my epistemological theory to work to answer this question, having intuitively arrived at the common-sense conclusion—that the answer is “yes.” Well, if my theory’s right, what would have to be the case for the answer to be “yes?” Well, if the three dimensions that determine evidentiary quality are supportiveness, independent security, and comprehensiveness, then combined evidence can warrant a claim to a higher degree than any of the components individually only if it increases supportiveness and/or increases independent security, and/or increases comprehensiveness. So far, so good?

RT: Yep.

SH: It turns out to be not quite that simple. This is one of the places where the theory needed tweaking, where I’m grateful to the legal system for making me tweak it. It turns out, as I was sort of vaguely aware before, but hadn’t been fully articulate about, that the independent security condition of course is asymmetrical, because a conclusion is more warranted the more independently secure the *favorable* evidence is, but less warranted the more independently secure the *negative* evidence is. And, moreover, comprehensiveness isn't going to increase warrant if you add more evidence but it's less favorable than what you had already. OK: so all of this had to be worked out in very considerable, tedious detail. (I shouldn't have said "tedious," I don't want to put anybody off!). I found the part about independent security extremely challenging. It took me a long time, and the first time I tentatively presented something on this matter I got it wrong, but I think I got it right eventually. You really can improve independent security, even though you can't make that epidemiological study any better.

RT: Right. Let me ask, then, about the business in the law about doubling the risk in these causation cases where there's a sort of a rule or understanding to the effect that, if the risk is more than doubled, then we have greater warrant for a causal conclusion than we otherwise would. You resist this.

SH: Not quite. Let's back up a little bit. First of all, we're talking now about *specific* causation. If you're trying a toxic tort case, you're the plaintiff, you have to establish both general causation—this stuff, to which my client was exposed, can cause this disorder, which my client has now got—and specific causation: that's to say, you have to be able to give evidence that this is what caused *this* plaintiff's disorder. Still with me?

RT: Yep.

SH: Proving specific causation is very hard; if you think about it, it's really difficult. You can't straightforwardly do it by epidemiology, because that's about a population, not about an individual. So it's a very tricky question, specific causation. And in the aftermath of the swine flu epidemic of 1976—it's amazing what you learn about when you go digging into legal history!—one court got the idea, well, look, if the plaintiff can show (and you can do *this* epidemiologically) that persons who were exposed to substance S are more than twice as likely to get disorder D than those who were not so exposed, then maybe that's necessary, and maybe it's sufficient, to show that, in this instance, *this* is what caused the plaintiff's disorder. Are you following me?

RT: Um, uh.

SH: The idea being, "well, more than half of the cases will be like that." It was that simple. This thought spread gradually, not as fast as the mildew in my bathroom, but gradually. It's kind of a convenient idea, when you think about it. Proving specific causation is very hard; and this apparently makes it significantly easier. And then Judge Kozinski used this idea in his ruling on remand in *Daubert*, arguing that, since most of the plaintiff's experts didn't even claim that there was a more than doubled risk of birth defects in the baby if the mother had taken Bendectin while she was pregnant, their evidence wasn't even relevant, and therefore wasn't admissible. Well, that case was, of course, enormously influential because it was the final ruling in *Daubert* which is the landmark case on scientific testimony. And in consequence this idea that a showing of more than doubled risk might be a good requirement on admissibility then spread like wildfire through the Federal Courts.

I might say there have always been courts that have resisted this—some of them on policy grounds, very reasonable policy grounds, I might add. Judge Jack Weinstein, who is a very influential and important federal judge, saw early on that if you take this as your test, first of all you're going to be compensating a whole lot of people who weren't really injured by the stuff; and that's crazy. He's right.

OK. Now, I think this [more than doubled risk idea] is a particular instance of the confusion of the two uses of "probable" that we encountered earlier in the book.

RT: Right.

SH: Right: an identification of the statistical idea of more than doubled risk with the idea of proof by a preponderance of the evidence, which is of course the standard in these cases. So I first of all argue at some length, using some of the apparatus from earlier, that this test is neither necessary nor sufficient for proof of specific causation. And that it is not necessary requires the following thought, which I believe the toxic tort system ought to keep firmly in mind: that some people are much more susceptible than others to certain drugs or some pollutants, etc.—they don't necessarily affect everybody equally; and we ought to allow for the possibility that even though there isn't a more than a doubled risk in the general population, this plaintiff might be able to show that he or she has a specific weakness of some kind, which makes him or her more susceptible. The other moral I would like to get across is that *any* increase of risk is some indication that there may be a causal connection. It's perfectly true that the greater the increased risk, the more plausible that conclusion. But we ought to take seriously any increased risk if there is some possibility that the plaintiff can produce other evidence showing their susceptibility. You understand what I'm saying here?

RT: Yeah.

SH: Of course, if you can show that the risk if you're exposed to this stuff is 200 times the risk if you're not, then that's pretty strong evidence of causation by itself. I'm not making this up. That actually is the relative risk of scrotal cancer if you work as a chimney sweep. (I always advise my male students on no account, however bad the market in legal jobs, to take such a job, because it's deadly!) But still, we should take *all* increased risk potentially to mean something, and we should look for specific susceptibilities. As medicine becomes more sophisticated, I think we're learning more and more that some medications work for some people and not for others. Presumably some of them are more dangerous to some people than to others also. So, I'm inclined to say—though I guess it's a little brave for "a person of mere theory" like myself—do you remember that lovely quotation from Bradley at the outset of one of the chapters …?

RT: Yes.

SH: "In a sphere of practical matter a person of mere theory is an useless and dangerous pedant." (I particularly like that '*an* useless,' for some reason.) Anyway, I'm tempted to say I think this shows that better epistemology would also be better policy.

RT: Right. Well you've been very generous with your time, but I want to ask one more question about the book. I wanted to make sure we got to the specific causation stuff. But there is a wonderful chapter earlier in the book about so called "litigation-driven science," and I think you make some very helpful distinctions between inquiry, sham inquiry, and advocacy in your discussion of the dangers of science driven by litigation, either as protecting against litigation or defending companies once litigation is underway. Can you tell us a little bit about that, because that does sort of reconnect with that second prong of your epistemological work, which is about the conduct of inquiry.

SH: Yes, that's right; and that work on the conduct of inquiry is itself influenced by Peirce who, as I'm sure you know, has a distinction between genuine inquiry and sham inquiry, which in *Manifesto of a Passionate Moderate*[6] I developed into a threefold distinction between genuine inquiry, sham inquiry (that's Peirce's word for inquiry while you've already decided on the conclusion before you start), and what I

6 *Manifesto of a Passionate Moderate: Unfashionable Essays* (Chicago: University of Chicago Press, 1998).

call “fake” inquiry, which is inquiry where you’ve already picked on a conclusion and you really don’t give a damn whether it’s true or false, you just think that defending it will make you famous.

Well, in this context, we’re talking about sham inquiry. I think the distinction is, like so many, a distinction of degree. That there’s probably no absolute pure inquiry, and there’s probably no absolutely impure sham inquiry. But, there *is* inquiry which is more and less affected by the desire to come up with this result rather than that. And I reserve the phrase “advocacy research” for the kind of investigation which is undertaken with the goal of finding what evidence we can in support of this policy, or this conclusion.

This idea achieves its importance in our legal system, I think, again in significant part through Judge Kozinski’s ruling on remand in *Daubert*—a very influential piece of work, written with tremendous energy and skill but, I also think, at bottom quite confused; but that’s another issue. Judge Kozinski notices, and it’s perfectly true, that all the scientists who were willing to testify on the Dauberts’ behalf have undertaken their work, as it were, after the event, after the Bendectin litigation has begun; and would not have done this work had there not been such litigation. So it is in a clear sense “litigation-driven.” And he suggests, and I think not without justice, that science which is so driven may be inherently likely to be of lower quality, less reliable than science which is conducted in the ordinary way of scientific business.

I think that’s true, and I think, with Peirce’s help, I can show *why* it’s true, why somebody undertaking research in that spirit will probably be less thorough, less critical, and so on than someone not operating in that spirit. But I think there are some problems with the use that Judge Kozinski makes of this idea. The first is that, of course, plaintiffs’ science in these toxic tort litigations is in the nature of the case likely to be litigation driven. I mean, why would they have done it until they got injured? And it’s rather easy to forget that it’s also possible that a drug manufacturer’s or chemical manufacturer’s work, before they market a drug or chemical, or indeed the post-market research, may also be done with an eye to litigation. If you look at the dates of the studies being cited about Bendectin, you’ll soon find that some of them were conducted *after* Bendectin was withdrawn from the market in 1984; and these are clearly intended, at least in part, to produce more evidence so that the manufacturer can defend itself in court. It’s a reasonable thing to do; but what it means is that science’s being litigation-driven is not a test which uniformly works against plaintiffs as opposed to defendants in these cases. That’s a simple point.

That said, I also have considerable reservations about a footnote in Judge Kozinski’s ruling. He recognizes, of course, that forensic science in criminal cases is all litigation-driven. If there weren’t crimes, we wouldn’t need forensic science. But he makes an exception: somehow or the other, the fact that the forensic sciences are litigation-driven doesn't matter. The argument he offers is: "well, these guys keep on testifying in court; they don't testify just once, they testify over and over and over again"—as if that made them more likely to be reliable; which makes me think: "yes, but look: the defendants often say in these toxic tort cases of the few scientists who are willing to testify for the plaintiffs, 'Aw, they're just professional expert witnesses, they testify over and over," as if this were a reason for *dis*trusting them, not for trusting them. So I think that footnote of Judge Kozinski opens a can of worms, and then he slaps down the lid again rather faster than I would have done. But then, I sort of have a taste for cans of worms, and perhaps he doesn't!

RT: Well, Susan, you've been very generous with your time and I know that there's a lot more really, really excellent material going on in the book *Evidence Matters*, but maybe we should cut it off here and allow me to just ask my usual parting question: What's on the horizon, what's your next project?

SH: OK. If you asked me, "What are you doing *now this minute*?" I would say: finishing a paper on credulity, an epistemic vice I'm very interested in, which I am presenting at the conference of the American Catholic and Philosophical Association. (The conference is about virtues, disposition, and habits. When they invited me, I said, "Please, can I pick a vice?" and they said OK!) I'm also busy preparing classes for a new experimental course in the College of Arts & Sciences on Religion and Evolution, and the U.S. Constitution, which I am teaching jointly with a Professor of Religion who is also Senior Vice-Provost. It's tremendous fun, but of course it's also a lot of work. And I'm reading a 450 page Spanish Ph.D. dissertation—wow! I'm glad to say I can, but, oh boy, is this a lot of work! And in the longer run? Of course, you know that one of the problems when you write a book like this is that people want you to do more of the same and yes, there's more of the same that I want to do. But I also have a significant body of work exploring what classical legal pragmatism could teach us in legal philosophy more generally. I have a long paper about Holmes' "The Path of the Law" for example, and another piece presenting what I call a neo-classical legal pragmatism—an account of what I call the "pluralistic universe" of law. I don't need to explain the allusion to you.

RT: William James.

SH: Yes, William James.

RT: And the “neo-classical” qualifier in front of “legal pragmatism” is to distinguish Holmes from someone like Richard Posner? Is that right?

SH: It’s to distinguish what I’m doing—which is calling not only on Holmes, but also on Peirce, James, Dewey—from Posner, yes, from the things that Richard Rorty wrote about the law, *and* from some of the use that’s being made of Robert Brandom’s work by legal scholars. There are a lot of misunderstandings of pragmatism out there. But my primary purpose isn’t really to clean those up. There will have to be a chunk that does that; but if I might say so, I spent ten years chasing after Rorty, and I’m not spending *another* ten years chasing after Posner!

RT: Good for you.

SH: There are other things to do. I want to be constructive. I also have material about the role of logic in the law, which is much in sympathy with Holmes’ view, but informed by knowledge of recent developments in logic of which he couldn’t possibly have known. And I have some stuff about what is probably Holmes’s most notorious ruling, *Buck v. Bell*, holding that it’s perfectly constitutional for the State of Virginia to sterilize the (allegedly retarded) Carrie Buck. I want to understand how this fits in with what he says about the law, and to get people looking at the relation between what he writes about the law and what he writes about his rulings in a way which I think is not quite a usual one. I think what I’m saying is there might be a book on Legal Pragmatism more generally in my future.

RT: Wow, that sounds very exciting, and I’ll keep an eye out for it; and when it’s out maybe we’ll get to talk to you again about it.

SH: That would be good. This has been fun.

RT: Well, thank you so much, Susan, for the time.

SH: Thank you for asking me.

RT: Sure, take care now.

SH: OK, bye.

RT: You’ve been listening to my interview with Professor Susan Haack of the University of Miami. We were talking about her new book *Evidence Matters: Science, Proof, and Truth in the Law* which is newly published by Cambridge University Press. I’m Robert Talisse, your host. This is New Books in Philosophy, thank you for listening.