EVIDENCE MATTERS

Is truth in the law just plain truth–or something *sui generis*? Is a trial a search for truth? Do adversarial procedures and exclusionary rules of evidence enable, or impede, the accurate determination of factual issues? Can degrees of proof be identified with mathematical probabilities? What role can statistical evidence properly play? How can courts best handle the scientific testimony on which cases sometimes turn? How are they to distinguish reliable scientific testimony from unreliable hokum? The dozen interdisciplinary essays collected here explore a whole nexus of such questions about science, proof, and truth in the law.

With her characteristic clarity and verve, in these essays Susan Haack brings her original and distinctive work in theory of knowledge and philosophy of science to bear on real-life legal issues. She includes detailed analyses of a wide variety of cases and lucid summaries of relevant scientific work, of the many roles of the scientific peer-review system, and of relevant legal developments.

Susan 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. She is the author of numerous highly acclaimed books, among them *Evidence and Inquiry* and *Defending Science–Within Reason*, and of many articles in legal, philosophical, and scientific journals. Haack is one of a tiny number of living philosophers included in Peter J. King, *100 Philosophers: The Life and Work of the World's Greatest Thinkers* (2004); and she appeared on the *Sunday Independent*'s list of the ten most important women philosophers of all time (2005).

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(continued after Index)

Evidence Matters

Science, Proof, and Truth in the Law

SUSAN HAACK
University of Miami

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Cambridge University Press is part of the University of Cambridge.

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www.cambridge.org
Information on this title: www.cambridge.org/9781107698345

First published 2014

Printed in the United States of America

A catalog record for this publication is available from the British Library.

Library of Congress Cataloging in Publication data
Haack, Susan, author.
Evidence matters : science, proof, and truth in the law / Susan Haack.
pages cm – (Law in context)
Includes bibliographical references and index.
ISBN 978-1-107-03996-4 (hardback) – ISBN 978-1-107-69834-5 (paperback)
1. Evidence (Law) 2. Admissible evidence. 3. Science and law. I. Title.
K2261.H32 2014
347′.06–dc23 2013048953

ISBN 978-1-107-03996-4 Hardback
ISBN 978-1-107-69834-5 Paperback

Until lately the best thing I was able to think of in favor of civilization … was that it made possible the artist, the poet, the philosopher, and the man of science.… Now I believe that … the chief worth of civilization is that it makes the means of living more complex, that it calls for great and combined intellectual efforts, instead of simple, uncoordinated ones.… Because more complex and intense intellectual efforts mean a fuller and richer life.

–Oliver Wendell Holmes (1900)

Contents

Acknowledgments

"Epistemology and the Law of Evidence: Problems and Projects"–the most recent of these essays–is based on a talk given at a 2012 workshop on legal epistemology organized by Rachel Herdy in the law faculty at the Universidade Federal do Rio de Janeiro, Brazil. It was also presented, in an abridged form and under the title "Problems and Projects in the Theory (and Practice) of Evidence Law," as an after-dinner speech at the 2013 conference of the Canadian Institute for the Administration of Justice.

"Epistemology Legalized: Or, Truth, Justice, and the American Way" was my Olin Lecture in Jurisprudence at the Notre Dame Law School, and appeared in the *American Journal of Jurisprudence* 49 (2004): 43–61.

"Legal Probabilism: An Epistemological Dissent" was first presented in 2011, at a workshop on standards of proof and scientific evidence organized by Jordi Ferrer Beltrán and his colleagues in the faculty of law at the University of Girona, Spain; and was one of the series of lectures I gave the following year in the faculty of law at the Universidad Externado de Colombia in Bogotá. It appeared, in Spanish translation by María José Viana and Carlos Bernal, in Carmen Vázquez, ed., *Estándares de prueba y preuba científica: Ensayos de epistemología jurídica* (Barcelona: Marcial Pons, 2013), 65–98.

"Irreconcilable Differences? The Troubled Marriage of Science and Law" has proved especially popular since it was first presented at a 2007 meeting in Bretton Woods, New Hampshire, organized by David Michaels and his colleagues at the Project on Scientific Knowledge and Public Policy (SKAPP). In 2008 it was presented in the faculty of law at the University of Alicante, Spain, and at Amherst College; in 2010 it was the basis of my talk at a joint conference of the School of Medicine and the College of Law at Florida International University; in 2011 it was presented in the department of philosophy at the University of Western Ontario, Canada, and in the faculty of law at the Pontifical Catholic University of Rio de Janiero (PUC-Rio); in 2012 it was

presented at the Universidad Externado de Colombia; and in 2013 a modified version was given at the VI Congreso internacional de derecho procesal in Cartagena, Colombia. This essay was first published in *Law & Contemporary Problems* 72, no.1 (2009): 1–24; and also appeared, in Spanish translation by Orión Vargas, in *Proceso judicial y cultura*, ed. Mónica Bustamente Ruaz (Medellín, Colombia: Universidad de Medellín, 2013), 109–96.

"Trial and Error: Two Confusions in *Daubert*"–the earliest of these essays–was first presented at the 2003 SKAPP meeting in Coronado, California, and later at law school colloquia at the University of Montana (2003), George Mason University, the University of Pennsylvania, University College London (2004), and the University of Florida (2005). It was first published (under the title "Trial and Error: The Supreme Court's Philosophy of Science") in the *American Journal of Public Health* 95 (2005): S66–73; was reprinted in the *International Barristers Quarterly* 41, no.2 (2006): 376–91; and also appeared, in Italian translation by Giovanni Tuzet, in *Ars Interpretandi: Annuario I ermaneutica guiridica* 11 (2006): 303–25.

"Federal Philosophy of Science: A Deconstruction–and a Reconstruction," written at the invitation of the then editor-in-chief, appeared in the *NYU Journal of Law & Liberty* 5, no.2 (2010): 394–435. This paper is based on a shorter talk entitled "Popper on Trial: A Brief History of a Big Muddle," given at a 2009 conference on Popper organized by the Max Weber Program at the European University Institute in Fiesole, Italy. It was first presented in its full form, in the same year, at the China University of Politics and Law in Beijing; then in 2010 in the faculty of law at the University of Girona and at a colloquium of the Center for Conceptual and Historical Foundations of Science at the University of Chicago; and in 2011 at PUC-Rio.

"Peer Review and Publication: Lessons for Lawyers" grew out of a shorter talk I gave at a 2006 meeting of the National Institute of Justice in Tampa, Florida; and appeared in the *Stetson Law Review* 36, no.3 (2007): 789–819–after which it served as the basis for an invited presentation on the same subject at the Committee on Science, Technology, and Law at the National Academies of Science (2008).

"What's Wrong with Litigation-Driven Science?" was first presented at the 2006 SKAPP meeting in New York; and was published (with the subtitle "An Essay in Legal Epistemology") in the *Seton Hall Law Review* 38, no.3 (2008): 1053–83.

"Proving Causation: The Weight of Combined Evidence," written at the invitation of the editors, first appeared (under the title "Proving Causation: The Holism of Warrant and the Atomism of *Daubert*") in the *Journal of Biomedical and Health Law* IV, no.2 (2008): 253–89. It was presented in the

School of Law at the University of Minnesota and at a conference organized by the department of mathematics at Iowa State University (2009); in the faculties of law at the University of Girona and at Universidad de Caldas, Colombia, and as the keynote lecture at the annual meeting of the Society of University Neurosurgeons (2010); and at PUC-Rio (2011).

"Correlation and Causation: The 'Bradford Hill Criteria' in Epidemiological, Legal, and Epistemological Perspective" was first presented at a 2012 workshop on causation in mass torts organized by Diego Papayannis in the faculty of law at the University of Girona and, later the same year, was aired to an audience of public-health professionals at the Fundação Oswaldo Cruz in Rio de Janeiro. It appears here for the first time.

"Risky Business: Statistical Proof of Specific Causation," written in 2010 at the invitation of Jordi Ferrer Beltrán, will appear, in Spanish translation by Nicola Muffato, in Diego Papayannis, ed., *Causalidad y atribución de responsibilidad* (Barcelona: Marcial Pons, forthcoming). It has been presented in legal colloquia at Uppsala University (Sweden), the University of Girona, the University of British Columbia, and Wayne State University (2011); in the law faculties at the Universidad Externado de Colombia and the University of Medellín (2012); and in the law faculty at the Jagiellonian University in Kraków, Poland (2013). It appears here for the first time in English.

"Nothing Fancy: Some Simple Truths about Truth in the Law" was written in 2010 at the invitation of Jorge Cerdio and Germán Sucar in the department of law at the Instituto Tecnológico Autónoma de México (ITAM), for inclusion in a three-volume series entitled *Verdad y Derecho* to be published by Marcial Pons, where it will appear in Spanish translation by Ramón Ortega García. In 2012 it was presented in the School of Law at the University of San Francisco and in the law faculties at the University of Girona and the University of Medellín; and in 2013 it was presented at the Osgoode Law School at York University in Toronto and in the philosophy department at Universität Paderborn, Germany. It appears here for the first time in English.

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This book has been a decade in the making; and a great many people have helped me in a great variety of ways. I am grateful to all those who invited me to participate in relevant events and publications, especially to David Michaels and the other organizers of SKAPP and to Jordi Ferrer Beltrán and his colleagues in legal philosophy at the University of Girona; to the audiences at the many fora and in the many countries where I have presented these papers over the years, and to the army of correspondents who, as always, also offered helpful comments and suggestions; to the students in my Law School class on scientific evidence, for their good questions; to my faculty assistant,

Beth Hanson, for capable and good-natured help of many kinds; to Pamela Lucken and all the other skilled and patient staff in the University of Miami Law Library who helped me find relevant material, check references, and get citations in the proper form; to Mark Migotti, who made ever-helpful comments on innumerable drafts; and to Howard Burdick, but for whom … well, but for whom.

Introduction: A Pragmatist Perspective on Science, Proof, and Truth in the Law

> To be master of any branch of knowledge, you must master those which lie next to it ....
>
> –Oliver Wendell Holmes[1]

Is truth in the law just plain truth–or is it something *sui generis*? Is a trial a search for truth–or is it something more, or something less, than that? Do the adversarial procedures of common-law systems promote factually sound verdicts? Do legal rules excluding relevant testimony enable the accurate determination of factual issues, or impede it? What bearing, if any, does the mathematical calculus of probabilities have on the degrees and standards of proof invoked in the law? What role can statistical evidence appropriately play in legal proof? How do the argument and counter-argument of adversarial proceedings differ from what scientists do as they seek out, sift, and weigh evidence? How can courts best handle the scientific testimony on which they now so often rely, and how are they to distinguish genuine science from pretenders–or reliable scientific testimony from unreliable hokum?

The dozen interdisciplinary essays collected here take up a whole nexus of such questions about science, proof, and truth in the law, bringing my work in epistemology and philosophy of science (and, from time to time, my work in philosophy of logic and language, metaphysics, etc.) to bear both on general questions about legal standards of proof and the relative merits of common-law and civil-law approaches to the handling of evidence, and on specific questions about the role of scientific testimony in legal proceedings. A key theme of my epistemology is that the structure of evidence can be understood by analogy with a crossword puzzle; and, just as this would lead you to expect, the

[1] Oliver Wendell Holmes, "The Profession of the Law" (1886), in Sheldon Novick, ed., *Collected Works of Justice Holmes* (Chicago: University of Chicago Press, 1995), vol. 3, 471–73, 472.

arguments of these essays ramify, interlock, and loop up and back. The first three essays focus on evidence, evidentiary procedures, proof, and probability; the next five turn to the role of scientific testimony and legal efforts to domesticate it; then in the next three essays I look specifically at causation evidence in toxic tort litigation; and in the last piece I explore questions about truth in the law and its relation to truth in the sciences.

All of these essays are imbued with the spirit of the classical pragmatist tradition–influenced, that is, not only by Oliver Wendell Holmes's writings on the law, but also by the classical pragmatists' thinking about inquiry generally, and about scientific inquiry in particular. My understanding of the evolution of legal concepts and legal systems, for example, and my stress on the limits of formalism, align with Holmes's. My objective conception of truth is in the spirit of C. S. Peirce's observation that "truth is SO, whether you or I or anybody thinks it is so or not"; my distinction between genuine inquiry and advocacy research runs parallel to his distinction between real inquiry and sham reasoning; and my crossword analogy is inspired in part by his critique of Descartes's metaphor of a chain of reasons. In my conception of scientific inquiry as a human enterprise, thoroughly fallible but nevertheless capable of real advance, there are echoes not only of Peirce, but also of the other classical pragmatists. And my conceptions of law, morality, and the relations between them are shaped, in part, by William James's and John Dewey's ethical writings.

Unlike the usual fare of analytic legal philosophy–often preoccupied with its own internecine disputes, and operating at a sometimes dizzyingly high level of generality and abstraction–this work of mine is prompted by real-life legal issues: by disputes that have arisen in court, by debates over the desirability of this or that rule or procedure, and so on. The rules, procedures, cases, etc., come largely from US law; but most of the issues they raise are of much more than parochial interest, and so too, I believe, are the benefits of a sound philosophical approach to understanding and resolving them.

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The first essay included here, "Epistemology and the Law of Evidence: Problems and Projects," sets the stage. I begin by explaining what I take epistemology to be, how I see it bearing on questions about evidence and evidentiary procedures in the law, and what pitfalls we need to avoid when we apply epistemological theory to legal practice. Next, I lay out my understanding of the differences between pseudo-inquiry and the real thing; of the nature and structure of evidence; and of the multiple determinants of evidential quality, and hence of degree of warrant–or, in legal terms, of proof. Then I can signal some of the ways this theoretical work can be applied in legal contexts:

to shed light on Peirce's critique of adversarialism, for example, and on Judge Kozinski's animadversions against "litigation-driven" science; to distinguish degrees of proof from mathematical probabilities, and at the same time explain what role statistical evidence can properly play; to understand the reasoning behind Jeremy Bentham's critique of exclusionary rules of evidence; to see how, when, and why a congeries of pieces of evidence may have greater weight than any of its components alone; and so on. Finally, as the title of this essay promises, I conclude with a list of "projects": i.e., of significant outstanding problems in legal epistemology; and with an argument that two-way traffic between legal practice and epistemological theory could greatly benefit not only legal thinking about evidence, but also the increasingly self-referential and narrowly-focused "niche" epistemology that, sadly, predominates today.

The second essay, "Epistemology Legalized: Or, Truth, Justice, and the American Way"–the earliest of the papers included here–focuses first on the adversarial character of US evidence law (evidence prepared and presented by the parties to a case, the witnesses for each side cross-examined by the attorneys for the other); and then on its reliance on exclusionary rules (rules limiting what evidence may be presented to a finder of fact). This essay explores two powerful epistemological criticisms of such an evidentiary régime: Peirce's, that the "hot and partisan debate" encouraged by adversarialism fosters a focus on victory rather than truth; and Bentham's, that rules limiting the admissibility of various kinds of testimony run contrary to the epistemological desideratum of comprehensiveness, the desirability of taking all the relevant evidence into account.

It can hardly be denied that the drawbacks Peirce and Bentham identified are real; nevertheless, I argue, neither Peirce's nor Bentham's critique is fatal to the idea that adversarialism and exclusionary rules *can* be a reasonable way to determine verdicts–given, that is, the inevitable limitations of time and resources. The real problem is that these common-law procedures can be defended only on certain assumptions, among them that resources are roughly equal on both sides; and that these assumptions are rarely true in practice–as I illustrate with some examples from the law governing scientific testimony, where prosecutors' resources are almost always greater than defenders', and manufacturer defendants' resources almost always greater than individual tort plaintiffs'.

In the introduction to "Epistemology Legalized" I note that epistemology should also help us make headway with some contested issues about degrees and standards of proof; and in the essay that follows, "Legal Probabilism: An Epistemological Dissent," I tackle some of these directly. The core argument is that probabilistic conceptions of degrees of proof and, in particular, the

subjective Bayesianism still dismayingly prevalent among evidence scholars, are fatally flawed. The first step is to show that degrees and standards of proof are best construed epistemologically, as degrees to which a conclusion must be warranted by the evidence presented for the party with the burden of proof to prevail–as the reasons for having standards of proof at all, as well as jury instructions on how to interpret such standards, reveal. The next step, calling on my foundherentist epistemology,[2] is to show that degrees of epistemic warrant simply don't conform to the axioms of the standard mathematical calculus of probabilities; from which it follows that degrees of proof cannot plausibly be construed probabilistically.

Still, this doesn't yet show how, in the particular, probabilistic approaches fail, or how, specifically, my approach succeeds; this is the purpose of the second half of the essay. I first show that Kadane and Schum's well-known subjective-Bayesian account of the evidence in the notorious trial of Sacco and Vanzetti (two Italian immigrants convicted of a 1920 robbery and murder) is seriously flawed; and that my foundherentist account can do significantly better. Then–to make clear that, though it isn't probabilistic, my account is perfectly capable of accommodating statistical and probabilistic evidence appropriately–I show that Finkelstein and Fairley's well-known subjective-Bayesian analysis of the case of Janet and Malcolm Collins (convicted of robbery largely on the basis of purely statistical evidence) is also seriously flawed; and that here too my approach does significantly better. I note in passing that my analysis also sheds some light on the role of DNA identification evidence–and so, like the previous essay, raises some issues specifically about scientific testimony.

Ever since scientific witnesses began to appear in court on a regular basis, there have been complaints about them; as early as 1858 we find the US Supreme Court writing that "experience has shown that opposite opinions of persons professing to be experts may be obtained to any amount."[3] Still, even in Holmes's day, scientific testimony played a much smaller role than it does now. Nonetheless, the Holmesian insight that legal systems are local, social institutions needing constantly to adapt to new circumstances is very relevant to the papers that follow, which explore the ongoing efforts of the US legal system to devise better ways of handling the scientific testimony on which, in this technologically advanced age, it more and more relies.

[2] Susan Haack, *Evidence and Inquiry: Towards Reconstruction in Epistemology* (Oxford: Blackwell, 1993); expanded 2nd ed., *Evidence and Inquiry: A Pragmatist Reconstruction of Epistemology* (Amherst, NY: Prometheus Books, 2009).

[3] Winans v. N.Y. & Erie R.R. Co., 62 U.S. 88, 101 (1858).

The fourth piece here, "Irreconcilable Differences? The Troubled Marriage of Science and Law," opens a clutch of essays on scientific testimony. It begins with a sketch of the many and various interactions of the law with science–legal regulation of hazardous scientific work, lawmakers' and regulators' reliance on scientific advice, the prosecution of scientists accused of fraudulently using federal research funds, constitutional cases involving the teaching of evolution in public high schools, "cultural heritage" cases involving ancient human remains, and courts' increasing reliance on scientific evidence–and continues with a summary history of US law on expert testimony.

Because expert witnesses present scientific, technical, or other specialized knowledge not available to the average juror, they aren't confined to testifying as to what they witnessed, but are allowed to give opinions. This special class of witnesses includes experts of every kind, including, e.g., specialists in automotive or even tire design, in accident reconstruction, in construction practices and standards, in computing, in the valuation of real estate or antiques or art, in forensic accounting, etc., etc., as well as practitioners of just about every scientific (and quasi-scientific) specialty imaginable. For a long time US law required only that an expert be qualified in his field. But in 1923 the very brief ruling in *Frye v. United States*–excluding proffered expert testimony as to the results of a primitive lie-detector test that Mr. Frye had taken–added a new requirement on the content of such testimony: that novel scientific testimony is admissible only if "the principle or discovery" on which it is based is "sufficiently established to have gained general acceptance in the particular field in which it belongs."[4] Gradually, over many decades, courts around the country began to rely on *Frye*, until eventually the "*Frye* Rule" was accepted in the majority of jurisdictions. (It remains the law today in a number of states, among them New York, California, and Pennsylvania.)

But in 1975 the Federal Rules of Evidence (FRE) were enacted; and FRE 702, providing that expert testimony was admissible provided that it was relevant and not otherwise excluded by law, made no mention either of *Frye* or of "general acceptance." Had *Frye* been superseded, or not? The situation wasn't clarified until 1993, when the US Supreme Court made its ruling in *Daubert v. Merrell Dow Pharmaceuticals*[5]–the first time in the history of the Court that it had ruled on the standard of admissibility of expert testimony.

The core argument of "Irreconcilable Differences" is that the difficulties in handling scientific testimony arise in part from tensions between the practices and values of science and the culture of the US legal system: e.g., between

[4] Frye v. United States, 293 F. 1013, 1014 (D.C. Cir. 1923).
[5] Daubert v. Merrell Dow Pharm., Inc., 509 U.S. 579 (1993) ("*Daubert* III").

the investigative character of science and the adversarial culture of the law; between the open-endedness of scientific investigation and the legal concern for finality; between the atomistic tendencies of evidence law and the quasi-holism of warrant; between the informal, pragmatic character of scientific inquiry and the formal procedures of the law; and so on. These tensions reveal themselves both in the history of legal efforts to domesticate scientific testimony by rules of admissibility, and in recent small compromises of finality and modifications of adversarialism in dealing with such testimony–the latter representing a modest move in the direction of civil-law evidentiary procedures.

The next essay, "Trial and Error: Two Confusions in *Daubert*," turns specifically to the remarkable foray into philosophy of science in the Supreme Court's *Daubert* ruling. As I said, the question before the *Daubert* Court was whether the old *Frye* Rule had been superseded, in federal courts, by FRE 702. It had, the Court ruled; nevertheless, courts still have an obligation to screen proffered expert testimony not only for relevance, *but also for reliability*. This, Justice Blackmun's ruling continued, requires that they satisfy themselves that such evidence qualifies as *bona fide* "scientific … knowledge."[6] Calling on the philosophy of Karl Popper, and throwing in a quotation from Carl Hempel for good measure, Justice Blackmun suggests that the mark of the genuinely scientific is falsifiability or testability; and, in line with this, the first of the indicia of reliability on the "flexible list" he offers by way of guidance to federal judges–now known as the "*Daubert* factors"–is whether the work on which supposedly scientific testimony is based "can be (and has been) tested."[7]

As the title of the essay suggests, these philosophical dicta of Justice Blackmun's are confused, in more ways than one. Casting around for a criterion of genuinely scientific, and hence reliable, expert testimony, he runs together two incompatible philosophies of science: Popper's falsificationism, and Hempel's confirmationism. He apparently doesn't realize that Popper's philosophy of science is singularly ill-suited for the purpose to which he put it, since–emphatically denying that scientific theories can ever be shown to be true or even probable–Popper deliberately eschews the notion of reliability; nor does he seem aware that Hempel's work provides neither a criterion of demarcation, nor any substantive help in assessing the reliability of complex scientific evidence. Moreover, when you think about it, it's clear that Justice Blackmun's approach was seriously misconceived from the get-go. He runs together "reliable" and "scientific"; but these are different not only in

6 *Id.*, 590.
7 *Id.*, 593.

meaning, but also in extension. Not all, and not only, scientific testimony is reliable.

Not surprisingly, in its subsequent rulings on expert testimony–in 1997, in *General Electric Co. v. Joiner*,[8] reaffirming that the standard of appellate review for such evidentiary rulings is abuse of discretion, and in 1999, in *Kumho Tire Co. v. Carmichael*,[9] holding that *Daubert* (but not necessarily those *Daubert* factors) applies to all expert testimony, not only to the scientific–the Supreme Court has quietly backed away from its earlier enthusiasm for philosophy of science. The result, however, has been to leave courts with wide discretion in screening expert testimony, but very limited guidance about how to do this.

Still, you have to wonder: where did the Supreme Court's allusions to Popper's philosophy of science come from, and what did federal courts make of the first *Daubert* factor, whether the proffered evidence "can be (and has been) tested"? The purpose of the next essay, "Federal Philosophy of Science: A Deconstruction–And a Reconstruction," is not only to answer these questions, but also to show exactly why Popper's falsificationism is so radically unsuited for the purpose to which Justice Blackmun put it; and to suggest a better understanding of science in its place.

I begin by presenting Popper's falsificationist philosophy of science in sufficient detail to show that, his rhetoric about "objective scientific knowledge" notwithstanding, his approach is so profoundly and so pervasively negative as to amount, in effect, to a covert skepticism; and so couldn't possibly provide a criterion of the reliability of scientific testimony. Next, I explain how Justice Blackmun misconstrues Popper's ideas, and identify some sources of his misunderstandings in the amicus briefs in *Daubert*, in the then-recent literature in the law reviews, and in Popper's own (very ambiguous) writings. Then I look in some detail at what federal courts have made of the Supreme Court's allusions to Popper. And finally, in the "reconstructive" part of this essay, I argue that, ironically enough, the interpretation most federal courts have given the first *Daubert* factor gestures towards a better epistemology of science than the flawed Popperian philosophy of science from which it ostensibly derives, but from which, in fact, it deviates quite radically; and that the account of the structure and quality of the evidence with respect to scientific claims developed in my *Defending Science–Within Reason*[10] provides the framework for understanding why.

[8] Gen. Elec. Co. v. Joiner, 522 U.S. 136 (1997) ("*Joiner* III").
[9] Kumho Tire Co. v. Carmichael, 526 U.S. 137 (1999).
[10] Susan Haack, *Defending Science–Within Reason: Between Scientism and Cynicism* (Amherst, NY: Prometheus Books, 2003).

Among the indicia of reliability on the *Daubert* Court's "flexible list," besides "falsifiability," was "peer review and publication."[11] But, as I point out in the next essay, "Peer Review and Publication: Lessons for Lawyers," "peer review" may refer either to the process of *pre-publication* peer review, or to the long-run scrutiny of the relevant scientific community *after* publication. Looking at the evolution of the pre-publication peer-review process that became standard practice at scientific journals after World War II–and at how the system presently operates–it becomes very clear that there is no guarantee *either* that all work that survives such review is sound, *or* that all sound work survives such review. So if this *Daubert* factor is understood as suggesting that courts screening for reliability should focus on whether proffered scientific testimony is based on work that has survived pre-publication peer review, though this will be relatively easy for a judge to determine, it is a very poor indicator of reliability. And if, on the other hand, it is understood as suggesting that courts should focus on whether the work on which proffered scientific testimony is based will survive the long-run scrutiny of the scientific community–which would certainly be a better indication of reliability–the problem is that it is impossible even for scientists expert in the field concerned, let alone for judges, to predict what work will survive and what will in due course be discarded as untenable.

In 1995, making the final ruling in *Daubert* (on remand from the Supreme Court), Judge Kozinski introduced a new *Daubert* factor of his own, suggesting that if the work on which proffered testimony is based is "litigation-driven," this raises a red flag about its reliability.[12] Thinking about the merits of this idea soon has us facing some subtle issues about the differences between real investigation and "advocacy research," i.e., seeking out plausible-sounding evidence supporting a predetermined conclusion. These are tackled, with Peirce's help, in the next essay, "What's Wrong with Litigation-Driven Science?" It's true, as Judge Kozinski suggests, that research undertaken for the purposes of litigation may be less reliable than research undertaken independently–but so too may research undertaken for marketing purposes; and, contrary to the exception Judge Kozinski makes in a footnote,[13] the same is true of forensic science, almost always conducted for the police or the prosecution. Moreover, as we see from Judge Bernstein's ruling in a Pennsylvania case, *Blum v. Merrell Dow Pharmaceuticals*,[14] in the kinds of toxic tort case that have shaped US law on scientific testimony, not only the expert testimony offered by plaintiffs, but

[11] *Daubert* III (note 5 above), 593.
[12] Daubert v. Merrell Dow Pharm., Inc., 43 F.3d 1311 (9th Cir. 1995) ("*Daubert* IV").
[13] *Id.*, 1317 n.5.
[14] Blum v. Merrell Dow Pharm., Inc., 33 Phila. Cnty. Rep. 193 (1996) ("*Blum* IV").

also the expert testimony offered by defendants, may be based on advocacy research.

In *Daubert*, in *Blum*, and in many such toxic tort cases–notably in *Oxendine v. Merrell Dow Pharmaceuticals*,[15] in *Joiner*,[16] and more recently in *Milward v. Acuity Specialty Products*[17]–plaintiffs argue that the expert testimony they wish to present is sufficient, considered jointly, to establish causation "by a preponderance of the evidence," even though no part of it would be sufficient by itself; and defendants sometimes argue in response that a collection of pieces of evidence can never be any stronger than any of its components individually. The next paper, "Proving Causation: The Weight of Combined Evidence"–the first of a trio on questions of causation–draws on my epistemological theory to show that, *under certain conditions*, a combination of pieces of evidence none of which is sufficient by itself really *can* warrant a causal conclusion to a higher degree than any of its components alone can do.

When my account is applied to the very complex congeries of evidence typically proffered to prove general causation in toxic tort cases, it suggests answers to some frequently-disputed questions: Is epidemiological evidence essential for proof of causation? Should such evidence be excluded unless its results are statistically significant? Should animal studies be excluded on principle? And so on. Moreover, the argument of this paper reveals that (as I suggested in "Irreconcilable Differences"), by encouraging the practice of screening each item of expert testimony individually for reliability, the evidentiary atomism implicit in *Daubert* can actually stand in the way of an accurate assessment of the worth of complex causation evidence.

In assessing questions of general causation in toxic tort cases, courts sometimes rely on the so-called "Bradford Hill criteria," which the original version of "Proving Causation" discussed only briefly. But in the next paper, "Correlation and Causation: The 'Bradford Hill Criteria' in Epidemiological, Legal, and Epistemological Perspective," I look in detail at Hill's ideas, the role they have played in litigation, and the ways in which they have been misunderstood. The first stage (the "epidemiological perspective") looks closely at the famous lecture, "The Environment and Disease,"[18] in which Hill spelled out the nine factors he believes should be taken into account in determining whether a statistical correlation between exposure to some substance and the occurrence of some disease or disorder is likely causal, and his many caveats

[15] Oxendine v. Merrell Dow Pharm., Inc., 506 A.2d 1100 (D.C. 1986) ("*Oxendine* I").
[16] *Joiner* III (note 8 above).
[17] Milward v. Acuity Specialty Prods. Grp., Inc., 69 F.3d 11 (1st Cir. 2011).
[18] Austin Bradford Hill, "The Environment and Disease: Association or Causation?" *Proceedings of the Royal Society of Medicine* 58 (1965): 295–300.

about how these factors should be used. The next stage (the "legal perspective") is to show that courts have sometimes badly misunderstood these factors, and have applied them in ways Hill never envisaged, and probably wouldn't have endorsed. Then, putting Hill's ideas in epistemological perspective, I show that what he offers is best conceived as a kind of sketch-map of the much larger territory of evidence potentially relevant to causal claims–a sketch-map that, when superimposed on the more detailed epistemological map I have provided, is seen to be helpful so far as it goes, but partial and incomplete.

Hill himself was very clear that there can be no hard-and-fast rules for determining when epidemiological evidence indicates causation, and seems to have grasped the quasi-holistic character of the determinants of evidential quality. But the legal *penchant* for convenient checklists, and the atomistic tendencies of US evidence law, have encouraged legal players to misconstrue his factors as "criteria" for the reliability of causation testimony, and many courts have misread his partial sketch-map.

The next piece, "Risky Business: Statistical Proof of Specific Causation," draws attention to its pragmatist orientation from the start with its opening quotation from Holmes about the evolution of legal concepts and rules. In line with this, the essay begins with a brief history of the evolution of the concepts of causation, responsibility, negligence, etc., deployed in the US legal system, and of some of the social, technological, and other changes that prompted these adaptations: such as the rapid growth of the railroad system in the second half of the nineteenth century, and the subsequent rise in crossing accidents; and, later, the rise of massive drug and chemical companies whose products sometimes proved harmful–in some instances, decades after exposure.

But this paper focuses primarily on one recent development in particular: the rise of the idea that evidence showing that exposure to the defendant's product more than doubles the risk of some disease or disorder is key to establishing specific causation, i.e., to showing that *this plaintiff's* injury was caused by this product. The first stage is historical: tracing how this idea arose and how it spread, distinguishing the several ways it has been construed, and exploring the reasons some courts have given for accepting it and others for rejecting it. The next stage is epistemological: showing that evidence of more than doubled risk, though relevant, is *neither necessary nor sufficient* for proof of individual causation, and providing a more defensible account of the role such evidence *can* legitimately play. And the last stage is policy-oriented: arguing, first, that to require, as some courts have done, that a plaintiff's expert must produce evidence of more than doubled risk for his testimony to be even admissible imposes an unreasonable burden; and finally, that the more

adequate understanding of the role of evidence of increased risk developed here would not only be epistemologically sounder, but also better serve the goals of tort law.

The concluding essay, "Nothing Fancy: Some Simple Truths about Truth in the Law," turns from proof to truth. I begin with the distinction between truth (the phenomenon) and truths (particular true claims); and the confusions that neglect of this distinction has fostered: e.g., that, because some truths are vague, truth itself must be a matter of degree; that, because some truths hold only at a given place, time, or jurisdiction, truth itself must be relative; and so on. Then, developing an understanding of truth along the lines of F. P. Ramsey's laconicism, I argue that, whatever the subject-matter of the proposition concerned, what it means to say that a proposition is true is the same: that it is the proposition that *p*, and *p*. Next, I look at the deceptively simple-seeming distinction between factual and legal truths, noting that there are many mixed and borderline cases–and, in passing, that the concept of legal reliability articulated in *Daubert* itself fudges the line somewhat. However, I continue, mixed and borderline cases aside, legal truths, i.e., truths about legal provisions, are a special sub-class of truths about social institutions; and, like many truths about a society, are socially constructed, made true by things people do–primarily by legislators' decisions, but also in part by judges' interpretations of statutes and precedents, and so forth.

And finally–anticipating the objection that, by focusing on truths *about* legal provisions to the neglect of the more vital issue of the truth *of* legal provisions, I have ducked the really hard questions–I turn specifically to the normative character of law. Legal systems, legal provisions, and legal decisions, I argue, may be morally better or worse, and the law *can* be an engine of moral progress; but legal norms cannot be assimilated to moral norms, and are not appropriately conceived as true or false representations of moral principles. And this, as I show, suggests a new and nuanced approach to an old but still daunting question: why the law should be obeyed.

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These essays were written for publication in a wide variety of journals and books–some for US law reviews, one for the *American Journal of Public Health*, one for the *American Journal of Jurisprudence*, and others for publication in Spain, Mexico, and Brazil. So I have edited them to unify the style of references and trimmed them here and there to avoid annoying repetition. Inevitably, though, given their interlocking structure, certain themes recur: the multiple determinants of evidential quality, for example, the quasi-holistic character of warrant, the material character of relevance, the difference between genuine inquiry and advocacy research, the misguided search for the

"scientific method," the constant evolution of the law and of legal concepts, and of US law on scientific testimony in particular.

Inevitably, also, while this editorial work was in process, there were various legal and other developments relevant to my arguments: for example, Bendectin, the drug at issue in *Daubert*, *Blum*, *Oxendine*, etc., returned to the US market (now made by a Canadian manufacturer and with a new name, "Diclegis"); and Florida, which had long been, at least officially, a *Frye* state, amended its Rule of Evidence 702 to correspond to the federal Rule 702 as modified in 2000 in light of the rulings in *Daubert*, *Joiner*, and *Kumho Tire*. I have given details of these changes in new footnotes. I have also included a glossary that will, I hope, be helpful to legal readers unfamiliar with the language of epistemology, to philosophical readers unfamiliar with the language of the law, and to any readers curious about the specifics of the diseases and disorders they read about in toxic tort cases; a table of cases cited, giving their histories; a list of the statutes, rules, etc., to which I refer; and, of course, a full bibliography.

July 2013