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“SCIENTIFIC INFERENCE” VS. “LEGAL REASONING”? —NOT SO FAST!*

*¿“INFERENCIA CIENTÍFICA” VS. “RAZONAMIENTO JURÍDICO”?
—¡NO TAN RÁPIDO!*

Susan HAACK**

Abstract:

To understand why the interactions of science with the legal system can be so problematic, it's not enough to gesture vaguely towards a supposed contrast between scientific and legal “modes of thinking”; we need to look, instead, to the consequences of the different purposes of science and the law, the different constraints under which they pursue those purposes, and the different cultures of the two enterprises. From this perspective we can see why the law sometimes asks more of science than science can give, and sometimes gets less from science than science could give; and also why a simple dichotomy of “scientific inference” vs. “legal reasoning” is more misleading than helpful.

Keywords:

Law, Legal Reasoning, Science, Scientific Inference.

Resumen:

Para entender por qué la interacción de la ciencia y el derecho puede ser tan problemática, no basta con apuntar vagamente hacia un supuesto contraste entre los “modos de pensamiento” científico y jurídico. Es necesario considerar, en cambio, las consecuencias de los distintos objetivos que tanto la ciencia como el derecho persiguen, así como las limitaciones bajo las cuales

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dichos objetivos son perseguidos, y las diferentes culturas que involucran a ambas empresas. Desde esa perspectiva, es posible observar no sólo por qué el derecho en algunas ocasiones pide más de lo que la ciencia puede dar en tanto que en otras recibe menos de lo que la ciencia podría dar, sino también por qué la simple dicotomía “inferencia científica” vs. “razonamiento jurídico” es en realidad más engañosa que útil.

Palabras clave:

Ciencia, derecho, inferencia científica, razonamiento jurídico.

I should like to know [said Mr Chichely] how a coroner is to judge of evidence if he has not had a legal training?" "In my opinion," said Lydgate, "legal training only makes a man more incompetent in questions that require knowledge of another kind... No man can judge what is good evidence on any particular subject unless he knows that subject well. People talk about evidence as if it could be weighed in scales by a blind Justice. [But a] lawyer is no better than an old woman at a post-mortem examination... You might as well say that scanning verse will teach you to scan the potato crops.¹

¹ George Eliot, *Middlemarch* (first published 1871-72, Signet Classics 1964) 155. Mr Chichely, the town attorney, and Dr Lydgate, the town doctor, are discussing who should be appointed as the new town coroner.

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Mulling over the invitation² to write 6,000 or so words on “different legal and scientific modes of thinking,” I felt like a panicked examination candidate who’s just realized that the question he has twenty minutes to answer would take at least a Ph.D. dissertation to handle adequately. There are so many issues entangled here that I hardly knew which loose end to tug first.

Eventually, I decided to begin with two simple observations: that the work of a scientist is very different from the work of an attorney or a judge; and that, when scientists give expert testimony or advise a court, a regulative body, etc., communication can be difficult and very imperfect. Lawyers, judges, and juries don’t always understand scientific evidence very well; and scientists don’t always convey clearly to lawyers, judges, and jurors what’s solidly-established and uncontroversial, what’s very likely true but still contested by a few, what’s still conjectural, and what’s as yet beyond the reach even of plausible scientific conjecture. In consequence, the law sometimes asks more of science than science can give, and sometimes gets less from science than the best that science could give.

Is this simply because the kind of person who becomes an attorney or a judge will likely have a very different temperament and very different talents from the kind of person who becomes an astrophysicist or an anthropologist, and will certainly have had very different education and training? No: while true enough, this is hardly explanation enough. We need to understand *why* the two enterprises require such different temperaments and talents and such different kinds of specialized apprenticeship. Well, might those supposed “different legal and scientific modes of thinking” provide an explanation? No; this still doesn’t go deep enough. True, no one would mistake a scientific article for a legal brief or a judicial ruling; but there’s a deeper reason for this difference: the different goals of science and the law.

The aspiration of science, I take it, is to find out how the world is; the aspiration of a legal system, to decide what to do, in these

² An invitation, sent by Professor Richard Lempert on behalf of the Academy, to participate in an American Academy of Arts and Sciences meeting on law and science.

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or those circumstances, to make our human world, in some measure, more just and more livable. If we want to understand why the law experiences such difficulties in handling scientific evidence, we need to go beyond those supposed “different modes of thinking” to consider these very different *purposes*, the very different *constraints* under which scientists and legal players pursue those purposes, and the very different *cultures* of the two enterprises.

The core business of the sciences is inquiry, investigation, finding explanations of natural, or in the case of the social sciences, social phenomena, events, etc. To be sure, scientists do many other things as well, and sometimes instead: attend seminars on “grant-writing,” apply for grants, read others’ grant applications, design apparatus, computer programs, etc., write up their results, referee others’ papers, make slides and posters to present at conferences, and perhaps apply for patents, attend to their start-up companies, and so on. And, to be sure, if a scientist feels passionately about some issue, he may find himself morphing into an advocate for action to combat global warming, to get Intelligent Design Theory out of the public-school classroom, or to bring clean water to the tribe he is studying; or, if he discovers a taste for this kind of thing, may find himself becoming a professional expert witness, biotech executive, scientific administrator, dean, provost, or university president.

Still, the essential business of science, as of history, detective work, investigative journalism, legal scholarship, etc., is inquiry: i.e., an effort to find answers to some question or questions. And what this requires is that you identify a phenomenon in need of explanation, come up with a potentially explanatory conjecture, figure out the consequences of the conjecture, look at the evidence you have and any further evidence you can find a way to lay hands on, and assess where all this evidence points and what conclusions might be drawn, with what degree of confidence.

It’s often assumed that there’s a distinctively scientific way of inquiring, the “scientific method,” and that it’s this method that explains the remarkable successes of the sciences. In *Daubert*,³ Justice Blackmun took this for granted; by now it is even enshrined in

³ *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579, 592 (1993).

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Black's Law Dictionary.⁴ Scientists themselves, put on the spot to say something about how they do what they do, often gesture at some half-understood philosophical conception of this supposed method⁵ —often, unfortunately,⁶ like Justice Blackmun's conception, something vaguely Popperian in tenor. But there *is no* method used by all scientists and only by scientists. On the one hand, there are the familiar, inescapable kinds of inference involved in all serious empirical inquiry. But these are not used *only* by scientists. On the other hand, there are the myriad specialized tools, techniques, and procedures devised by generation after generation of scientists to get more evidence of the kind they need and a subtler sense of where it points: instruments of observation, mathematical and statistical techniques, experimental, epidemiological, and other protocols, etc., *and* the internal social arrangements that have grown up in scientific communities to enable the sharing of results, encourage creativity, discourage dishonesty, and so on. But these scientific "helps"⁷ to inquiry, which are constantly evolving and often local to a specific field, are not used by *all* scientists.

This is not to deny that scientists make inferences —of course they do: about what might explain a puzzling event or phenome-

⁴ The account of "scientific method" now found in *Black's Law Dictionary* (10th edn, Thomson Reuters 2014) 1547, cobbles together a quotation from *Daubert* and a quotation from the NRC/NAS report, *Strengthening Forensic Science in the United States* (National Academies Press 2009).

⁵ See, e.g., Peter Bock, *Getting It Right: R&D Methods for Science and Engineering* (Harcourt Brace 2001) 168; Stephen S. Carey, *A Beginner's Guide to Scientific Method* (4th edn, Wadsworth 2011) 305ff. Hugh G. Grauch, *Scientific Method in Practice* (Cambridge University Press 2003) acknowledges the simple-mindedness of the kind of thing typically found in beginning college texts, but is himself quite naïve about the supposedly "incisive thinking and penetrating analysis" of Popper and Kuhn (11).

⁶ "Unfortunately" because, ironically enough, Popper's philosophy of science is really just a thinly-disguised skepticism. Susan Haack, 'Just Say "No" to Logical Negativism' (2011) in Haack, *Putting Philosophy to Work: Inquiry and Its Place in Culture* (first published 2008, expanded edn, Prometheus Books 2013) 179-94 (text) and 298-305 (notes).

⁷ The word is Francis Bacon's. Francis Bacon, *Works*, vol IV, 42 (*The New Organon* [1620] Aphorism II).

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non, about what the consequences would be if this or that conjecture were true, about the degree to which a conjecture is warranted by the evidence. But such inferences aren't unique to the sciences—everyone trying to answer an empirical question makes them; and doing science isn't *simply* a matter of inference. For one thing, it requires not only reasoning, but also appropriate connections with the world; ultimately, all scientific theories rest on experience, the evidence of the senses. For another, science can't confine itself to a language fixed in advance, but will require the development of new vocabulary or the adaptation of older vocabulary in a specialized language more closely approximating newly-identified kinds of stuff, thing, event, etc.—as witness, for example, the hundred-year history of the development of the concept of DNA.⁸ That's why, as Dr Lydgate realized a long time ago, scientific work requires a grasp of *content*, not just of *form*.

Perhaps all this sounds radical; and from the perspective of twentieth-century proponents of inductivist, deductivist, probabilistic, Bayesian, game-theoretical, etc., models of the Scientific Method, it *is* radical. But it would have been entirely familiar to Thomas Huxley, according to whom the “man of science... simply uses with scrupulous exactness, the method which we all, habitually and at every minute, use carelessly”;⁹ to Albert Einstein, who once observed that “the whole of science is nothing more than a refinement of common sense”;¹⁰ to Percy Bridgman, who commented that the supposed scientific method is “something talked about by people... on the outside,”¹¹ when the crucial thing is that a scientist “do his utmost

⁸ More detailed history can be found in Susan Haack, *Defending Science-Within Reason: Between Scientism and Cynicism* (Prometheus Books 2003) 77-78 and 224-27; and ‘The Growth of Meaning and the Limits of Formalism, in Science and Law’ (2009) XXIX(1) *Análisis Filosófico* 5.

⁹ Thomas H. Huxley, *On the Educational Value of the Natural History Sciences* (John van Voorst 1854), 13.

¹⁰ Albert Einstein, ‘Physics and Reality’ in *Ideas and Opinions of Albert Einstein* (Crown Publishers 1954) 290.

¹¹ Percy Bridgman, ‘On Scientific Method’ (1949) in Bridgman, *Reflections of a Physicist* (Philosophical Library 1955) 81.

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with his mind, *no holds barred*";¹² to James B. Conant, who wrote that "what the scientist does is simply to carry over, into another frame of reference, habits that go back to the caveman";¹³ or to Gustav Bergmann, who described science as the "long arm" of common sense.¹⁴

I'm tempted to say, with Paul Feyerabend, that in science, "*the only principle that does not inhibit progress is: anything goes*".¹⁵ This is not, however, to suggest, as he did, that there's nothing more to science than power, politics, rhetoric, and negotiation; but only to recognize the creative, open-ended, improvisational character of scientific work. And neither is it to suggest, as he did, that science isn't a rational enterprise; but only to acknowledge that the reason the sciences have been able to make their remarkable discoveries isn't that they have a unique method of inquiry, but that they have amplified and refined the familiar procedures of the most ordinary everyday inquiry with tools and techniques that enable them to get more, more exact, and more reliable evidence, and better assess where it points. As the saying goes, nothing succeeds like success: each step forward enables scientists to correct past missteps, and perhaps to glimpse new ways to go about tackling the next problem.

If this is right, there is no peculiarly scientific method a scientist is obliged to employ. And neither, obviously, is there anything in the sciences analogous to legal rules about burdens and standards of proof or the admissibility of evidence.¹⁶ There *are*, however, other kinds of constraint on scientific work. The most important, internal

¹² Percy Bridgman, 'New Vistas for Intelligence' (1947) in Bridgman, *Reflections of a Physicist* (ibid.) 553, 554.

¹³ James B. Conant, *Modern Science and Modern Man* (Columbia University: Bampton Lectures in America #5 1952) 22 <<https://archive.org/details/modern-scienceand030457mbp>> accessed 28 August 2018.

¹⁴ Gustav Bergmann, *Philosophy of Science* (University of Wisconsin Press 1957) 222.

¹⁵ Paul K. Feyerabend, *Against Method* (first published 1975, Verso 1978) 23.

¹⁶ To be sure, there are sometimes conventions about evidence adopted in specific scientific fields (such as standards of statistical significance); but these are hardly comparable to legal rules specifying that this or that kind of evidence may not be heard by the trier of fact or, if heard, must be disregarded.

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to the enterprise, is that —like a historian or a legal scholar or any other empirical inquirer— a scientist has an obligation to respect the evidence, the facts. But there are also external constraints, such as rules about the ethical treatment of human or animal subjects; and professional constraints, such as the need to find funding for your work, the risk that you won't be able to publish anywhere respectable if your work is too far from mainstream ideas, or the fear that you will be written off as a crank if your ideas are too radically at odds with what is taken to be already well-established.

Again, scientific inquiry isn't bound by anything like the statutes of limitations or rules limiting the introduction of new evidence to which legal proceedings are subject; it takes the time it takes. Depending on the difficulty of the problem in the existing state of knowledge, the availability of resources, the abilities, backgrounds, and motivation of the people involved, and how hospitable the environment in which the research is conducted happens to be, progress may be painfully slow or exhilaratingly fast, steady or uneven. There are, however, external time constraints, such as the need to report results in a timely way to whatever body funds your work, or to produce something publishable this year to get tenure, a raise, or a promotion; not to mention the need to publish quickly to ensure that it's you, and not your rival, who gets credit for a discovery.

In any scientific community, probably, some will be more ready to try a new conjecture when the existing hypothesis most of their colleagues are content to work with encounters difficulties, and others will be more disposed to keep trying to adapt the old idea. Still, consensus will gradually form. *Ideally*, we would find agreement among scientists in a field when, and only when, the evidence is sufficient to indicate that it's probably safe to rely on this conjecture, but likely to be a waste of time pursuing work on that rival idea. But there's absolutely no guarantee that consensus on a scientific question will always faithfully track the state of the evidence. The work is hard; and scientists are only human. Sometimes they cut corners; sometimes they deceive themselves into thinking that the evidence points more decisively to a commercially-desired or politically convenient conclusion, or to the conclusion they have long defended, than it really does; and they are almost always subject to pressures both from

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the larger world and from within their profession. There's a considerable gap between science as it would ideally be conducted, and science as it actually *is* conducted in the real world.

Ideally, again, given their core task, the culture of the sciences would accord with the values Robert Merton described long ago,¹⁷ with respect for evidence and willingness to share results taking center-stage. But scientists don't work in a vacuum; they are part of, and depend on, a larger society. *In practice*, the core values of science can be eroded by the competing priorities —political, commercial, legal, and institutional— of the bodies that support scientific work. Governments, pharmaceutical companies, etc., that fund scientific research almost always have other concerns besides discovering the truth, and sometimes have an interest in playing down, disguising, or suppressing unpalatable facts.¹⁸ And recent changes in the management of universities have created a raft of perverse incentives —especially, the insistent demands that everybody be “productive” all the time— which encourage haste and carelessness, and damage the scientific ethos. The result? —an explosion of journals, a torrent of publications, salami publishing, misleading multiple attributions of authorship, an increasingly overburdened and corrupt peer-review system, and an enormously time— and energy-consuming preoccupation with “writing grants,” and the vast bureaucracy that accompanies it.

Over time, the *physical* and *intellectual* helps to scientific inquiry —the instruments, techniques of measurement and calculation, etc.— have grown steadily more sophisticated, more discriminating, more powerful. But the *social* helps, the institutional arrangements to encourage creativity, honesty, evidence-sharing, and the like, have not. Instead, as science has grown larger, more expensive, potentially more profitable, and politically more consequential, as scientific publishing has become big business, and as university administra-

¹⁷ Robert K. Merton, ‘Science and Democratic Social Structure’ in Merton, *Social Theory and Social Structure* (Free Press 1946) 307-16.

¹⁸ See, e.g., Susan Haack, ‘Scientific Secrecy and “Spin”: The Sad, Sleazy Saga of the Trials of Remune’ (2006) in Haack, *Putting Philosophy to Work* (n 6) 141-52 (text) and 289-93 (notes).

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tors have come increasingly to rely on badly flawed surrogate measures of the quality of the work produced, such social helps as the pre-publication peer-review system¹⁹ are under severe strain. The titles of recent articles —“Who’s Afraid of Peer Review?”²⁰ “The Corruption of Peer Review is Harming Scientific Credibility,”²¹ “Peer Review Fraud,”²² “Scientific Regress”²³— hint at a disturbing truth: the integrity of the scientific enterprise is presently under real threat.²⁴

It is sometimes said that the purpose of a trial, like the purpose of scientific work or historical research, is to discover the truth.²⁵ But a common-law trial²⁶ isn’t remotely like a scientific experiment or a historical investigation; indeed, no one involved in such a trial is trying to discover who committed the crime, who or what caused the injury, etc. When judges, attorneys, or jurors try to figure out the truth of factual questions, no doubt they go about it in much

¹⁹ See Susan Haack, ‘Peer Review and Publication: Lessons for Lawyers’ (2007) in Haack, *Evidence Matters: Science, Proof, and Truth in the Law* (Cambridge University Press 2014) 156-79.

²⁰ John Bohannon, ‘Who’s Afraid of Peer Review?’ (2013) 342(6154) *Science* <<http://science.sciencemag.org/content/342/6154/60.full>> accessed 28 August 2018.

²¹ Hank Campbell, ‘The Corruption of Peer Review is Harming Scientific Credibility’, *Wall Street Journal*, (New York 14 July 2014) A15.

²² Charlotte Haug, ‘Peer Review Fraud-Hacking the Scientific Publication Process’ (2015) 373(25) *New England Journal of Medicine* 2393-95.

²³ William Wilson, ‘Scientific Regress’ (May 2016) *First Things* 37-42.

²⁴ I have drawn in this section on my *Defending Science—Within Reason* (n 8); ‘The Integrity of Science: What it Means, Why it Matters’ (2006) in *Putting Philosophy to Work* (n 6) 121-40 (text) and 283-88 (notes); and *Scientism and its Discontents* (Rounded Globe 2017) <<https://roundedglobe.com/books/1b42f98a-13b1-4784-9054-f243cd49b809/Scientism%20and%20its%20Discontents/>> accessed 28 August 2018.

²⁵ Janet Reno, ‘Message from the Attorney General’ in Edward Connors, Thomas Lundgren, Neal Miller and Tom McEwen (eds), *Convicted by Juries, Exonerated by Science* (National Institute of Justice [NIJ] Research Report 1996) iii-iv, iii.

²⁶ I will speak here only of common-law trials, and mostly of U.S. legal procedures. Elsewhere, however, I have written briefly about civil-law evidentiary procedures. Susan Haack, ‘La justicia, la verdad y la prueba: No tan simples, después de todo,’ in Jordi Ferrer Beltrán and Carmen Vázquez (eds), *Debatiendo con Taruffo* (Marcial Pons 2016) 311-36 (English version available from the author).

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the same way as the rest of us; but this *isn't* what they're doing at trial. The task of the attorneys is to make the best case they can for their side; which may require them to turn the peculiar constraints imposed by our evidence law to their side's advantage. The task of the judge presiding over a trial is to ensure that the legal rules are followed and the law appropriately applied; which may require him to exclude relevant, but inadmissible, evidence. And the task of the jury (or of judges acting as triers of fact in a bench trial)²⁷ is to determine whether the party with the burden of proof has presented admissible evidence sufficient to establish guilt, liability, etc., to the degree required by the standard of proof; which may oblige them to ignore relevant evidence that has been ruled inadmissible.

I would say, rather, that the purpose of a trial is to arrive at a determination of guilt, responsibility, punishment, liability, or whatever, in a legally-correct way. This, however, is not to suggest that factual truth is just irrelevant to our legal process. For the hope is that arriving at conclusions in a legally-correct way will mean that, by and large and on the whole and in the long run, justice is served; and part of what this means is that, if all those involved do their jobs adequately, often enough *the truth about what actually happened will come out*. Insofar as there is an epistemological rationale for our adversarial legal system, it would be this: provided that attorneys are effective in seeking out evidence favorable to their side, discovering the weaknesses of evidence favoring the other side, and exposing those weaknesses in cross-examination, that judges rule correctly on the (in)admissibility of evidence, and that juries understand their duty and perform it adequately, this admittedly-oblique procedure is a tolerably-efficient way —given the inevitable limitations of time and resources— of arriving, often enough, at factually-correct verdicts.²⁸

²⁷ When, in what follows, I speak of the distinctive kind of reasoning used by judges, of course I refer to the role of a judge presiding over a case, not the role of a judge serving as trier of fact (or, in a common, but potentially misleading, phrase, as "finder of fact") in a bench trial.

²⁸ As I suggested in 'Epistemology Legalized: Or, Truth, Justice, and the American Way' (2004), in Haack, *Evidence Matters* (n 19) 27-46.

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There *is* a distinctive kind of reasoning used by attorneys and judges —the process we describe by the phrase “analogize and distinguish”. But this isn’t a distinctively legal way of discovering truths about the world, but something quite different. Like all truths about human societies, truths about what the law is in some place at some time are made true by things people do; but unlike a legal scholar, whose task is to discover such truths, a judge interpreting statutes and precedents may be *contributing to making* something legally true. Sometimes the application of a legal provision to a specific case is a matter of deduction from a statute or a rule, or from a statute or a rule plus legal principles of statutory interpretation;²⁹ and when it is, what’s involved is, simply, deduction, not some peculiarly legal kind of reasoning. But where this doesn’t suffice, a judge may argue that relevant similarities between this case and these other previous cases and relevant differences between this case and those other previous cases mean that that the legally-most-reasonable upshot is this rather than that; and the attorneys for each side will try to persuade the judge that this case is analogous to these previous cases where the upshot was the one their side seeks, but distinguishable from those other previous cases going the other way. In short, the peculiarly legal reasoning in which judges engage is aimed at *determining how to interpret and extrapolate a law or a rule* in the light of the precedents; while the peculiarly legal reasoning in which the parties to a case engage is aimed at *persuading the judge to stress these precedents, these analogies, these distinctions, rather than those*.

Perhaps this too sounds radical; and from the perspective of those who suppose that judges and attorneys are *finding out the real meaning* of the law or statute concerned it *is* radical. But it would have been entirely familiar to Oliver Wendell Holmes, who wrote that the appearance that a judge is deducing the legally-cor-

²⁹ “American statutory interpretation is not a simple exercise,” but involves attention both to text and to context: William N. Eskridge, Jr., *Interpreting Law: A Primer on How to Read Statutes and the Constitution* (Foundation Press 2016) 26-27. A long appendix (407-45) lists the Supreme Court’s observations on the canons of statutory interpretation from 1986-2016.

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rect result is often just the “evening dress” the proposed interpretation puts on “to appear respectable”;³⁰ or to Benjamin Cardozo, who wrote of the “creative element” in a judge’s work, the frequent forks in the road, and the incompleteness of the signposts.³¹ You only have to look at some of those tangled plurality rulings from the Supreme Court —*Allegheny v. ACLU*,³² say, or *Williams v. Illinois*³³— to realize how implausible it is to suppose that, when judges disagree about the interpretation of some legal provision, at least one of them must have made a logical mistake and got the wrong answer. Rather, they disagree about how to apply a law in the circumstances of this case because they give different weights to various historical, legal, moral, political, economic, etc., considerations.

Granted, scientists often argue analogically,³⁴ and even, sometimes, go through a process that might be described in terms of analogizing and distinguishing. Molecular biologists working in the wake of Marshall Nirenberg and Johan Mattaei’s successful solution of the first “word” of the code by which RNA builds proteins,³⁵ for example, were presumably thinking that their word would be like Nirenberg and Mattaei’s in certain respects, but unlike it in others. True, too, legal reasoning by analogy³⁶ involves spotting a pattern common to

³⁰ Oliver Wendell Holmes, ‘Book Notices’ (1880) 14 *American Law Review* 233, 234 [reviewing William Anson, *Principles of the English Law of Contracts* [Clarendon Press 1879] and Christopher Columbus Langdell, *Selection of Cases on the Law of Contracts* [2nd edn, Little, Brown, & Co. 1879]]. See also Susan Haack, ‘On Logic in the Law: “Something, but Not All” (2007) 20(1) *Ratio Juris* 1-31.

³¹ Benjamin N. Cardozo, *The Growth of the Law* (Greenwood Publishers 1924) 57.

³² *Cnty. of Allegheny v. ACLU*, 492 U.S. 573 (1989).

³³ *Williams v. Illinois*, 567 U.S. 50 (2012).

³⁴ C. S. Peirce provided a plausible analysis of such arguments as mixed inferences, i.e., inferences involving abduction, deduction, and induction. Charles Sanders Peirce, *Collected Papers* (Charles Hartshorne, Paul Weiss and [vols. 7 and 8] Arthur Burks [eds], Harvard University Press 1931-58) 2.733 (1883) (references are by volume and paragraph number).

³⁵ What Nirenberg and Mattaei discovered was that “[o]ne or more uridylic acid residues appear to be the code for phenylalanine.” The story is told by Horace Freeland Judson in *The Eighth Day of Creation* (Simon and Schuster 1979) 480 ff. (the quotation is from p. 481).

³⁶ See Scott Brewer, ‘Exemplary Reasoning: Semantics, Pragmatics, and the Ra-

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a class of previous cases and the case at hand; which is in some respects quite similar to the first, abductive step of a scientific argument by analogy, which involves spotting a pattern common to these and those natural or social things, phenomena, or events. But there's a crucial difference: the upshot of a scientific analogy is a factual conjecture, a potential explanation that then needs to be checked against what is taken to be already known, explored for explanatory power, and tested (directly or indirectly) against the world; the upshot of a legal analogy, by contrast, is an interpretation or application of a rule or a law—which may be better or may be worse, and may become established in the legal system or may not, but which just isn't susceptible to this kind of testing against the facts.

Unlike the work of scientists, the work of attorneys and judges is formally constrained in many ways: by rules of procedure, timeliness, etc., and by the rules about burdens and standards of proof, admissibility, and discovery to which the legal scrutiny of evidence must conform. Moreover, in the law promptness and finality are, rightly, valued: we want legal decisions to be reached without interminable delay, and (normally) to stand once the appeals process has been exhausted. In this, the law is very different from the sciences: while scientists would no doubt *prefer* that they find the answers to the questions they're struggling with sooner rather than later, the idea that they might simply decide to accept whatever answer looks best now, regardless of how inadequate present evidence may be, is just bizarre; and while, again, they would no doubt *prefer* that the currently-accepted theory stand firm, the idea that they might simply decree that it *will* stand firm no matter what new evidence comes in is no less so.

To be sure, the sciences aren't wholly open and improvisational, and our legal system isn't wholly rigid and formalized. The sciences have their rigidities: protocols for going about this kind of experiment or that kind of study, the routinized "write-by-numbers" style

tional Force of Legal Argument by Analogy' (1996) 108 *Harvard Law Review* 923-1028; and, for a detailed comparison of Peirce's analysis of analogical arguments in science with Brewer's analysis of legal analogizing, Susan Haack, 'On Logic in the Law' (n 30) 22-23.

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of almost all scientific articles today, and so on. And our legal system is constantly adapting to changing circumstances: developing, for example, a raft of ways of handling mass tort claims,³⁷ diversion programs in sentencing,³⁸ a whole body of internet law,³⁹ alternative forms of dispute resolution,⁴⁰ and so forth. And it also gradually adapts to scientific developments —as, for example, in response to new DNA identification techniques and databases, some jurisdictions issued “John Doe” warrants or tolled the statute of limitations on certain crimes.⁴¹

Still, the culture of our legal system is very different from the culture of the sciences: strongly adversarial, focused on advocacy rather than on inquiry; shaped by a thicket of rules and formalities; and encouraging, and in some circumstances even mandating, the sequestering of information. True, there’s sometimes competition in the sciences (between rival individuals, or rival teams); but this isn’t, as in our legal system, built-in, structurally-required. True, there’s sometimes advocacy in the sciences (for one approach to a problem, or one proposed explanation, over another); but this isn’t, as in the law, built-in, structurally-required —indeed, it is something to be regretted. And true, again, results in the sciences are sometimes withheld (to prevent a rival’s beating you to a discovery, to disguise a weakness in your approach, etc.); but this isn’t, as with the law’s

³⁷ See, e.g., Judith Resnik, ‘From “Cases” to “Litigation”’ (1991) 54 *Law & Contemporary Problems* 6-68.

³⁸ See, e.g., William R. Kelley, *Criminal Justice at the Crossroads: Transforming Crime and Punishment* (Columbia University Press 2015), chapter 5.

³⁹ See, e.g., Ian C. Ballon, *E-Commerce & Internet Law: Treatise with Forms* (2nd edn, Thomson Reuters 2015).

⁴⁰ See, e.g., Deborah R. Hensler, ‘Our Courts, Ourselves: How the Alternative Dispute Resolution Movement is Reshaping our Legal System’ (2003) 25 *Penn State Law Review* 65-97.

⁴¹ ‘Tossing out the Clock: Some States are Repealing Some Statutes of Limitations’ *National Law Journal* (June 23, 2003). Regrettably—since “recovered memory” testimony is, to say the least, far from reliable—some jurisdictions also repealed statutes of limitations in response to claims of supposedly recovered memories of long-ago crimes. See, e.g., Martin Gardner, ‘The False Memory Syndrome’ (1993) 17 *Skeptical Inquirer* 370-75.

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protection of proprietary information, something required —but, again, something to be regretted.

Of course, just as there's a considerable gap between science as it would ideally be conducted and science as it actually is conducted, there's also a considerable gap between our legal system as theoretically conceived and justified, and our legal system as it actually functions in practice. After all, like scientists and the rest of us, lawyers, judges, and jurors are only human: not every cross-examination is equally well-informed and searching,⁴² not every judicial interpretation of the law is equally reasonable, not every verdict based strictly or appropriately on the evidence presented. Moreover, our legal system is seriously overburdened; and as fewer and fewer cases go to trial and more and more go to arbitration or are concluded with a plea bargain or a settlement, the assumptions on which the epistemological rationale for adversarialism depends become less and less realistic.

Scientific inquiry, I have argued, is continuous with everyday empirical inquiry, but enormously refined and amplified by specialized tools and techniques. So one reason it's hard for those outside the relevant field fully to understand scientific evidence is, simply, that they are unfamiliar with those tools and techniques and with the technical vocabulary that grows up in every serious scientific specialty —vocabulary that can be at best superficially understood by someone who lacks real knowledge of the subject-matter. Yes, sometimes it's possible even for someone without specific knowledge of the field to spot methodological flaws: an epidemiological study has no controls, say, or wasn't conducted double-blind, or relied on self-reported disorders; but some flaws are very hard for an outsider —whether a judge, an attorney, a juror, or for that matter a scientist from a different field— to detect.

Moreover, the adversarial culture of our legal system not only encourages lawyers and judges to assume that, on any scientific ques-

⁴² Reportedly, the now-notorious lab technician Annie Dookhan had been cross-examined *a hundred and fifty times*, but her gross malfeasance was never revealed. Sean K. Driscoll, 'I Messed Up Bad': Lessons on the Confrontation Clause from the Annie Dookhan Scandal,' (2014) 56 *Arizona Law Review* 717-40.

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tion, there must be two sides, but also tends to draw in, as expert witnesses, those scientists who are ready to settle on a conclusion when most people in their field take the view that they must wait for further evidence (and who, to make matters worse, are apt to grow more dogmatically certain as they testify over and over to the same effect). And though, on many scientific questions—including, very often, the scientific questions at the heart of toxic-tort litigation—the most reasonable position is “As yet, we just don’t know,” neither party is likely to want to hire as their expert a scientist who would candidly say just that. At the same time, the procedures of the law can come as quite a culture-shock to legally-naïve scientists, who may find their treatment under cross-examination disrespectful, the legal preoccupation with conflicts of interest baffling,⁴³ and being “Dauberted out” by a judge who rules that their work isn’t really scientific downright insulting.

And it’s not surprising if, faced with the need to reach some conclusion when the relevant science is as yet unsettled —“to decide,” as Learned Hand put it long ago, “where doctors disagree”⁴⁴— lawyers and judges often look for something like procedural principles to appraise scientific testimony, in effect falling back on their skills in scanning verse to scan the potato crops. This, I suspect, partly explains *Daubert*’s preoccupation with “methodology.”⁴⁵ It also suggests why statistical significance, relative risk > 2,⁴⁶ faithfulness to

⁴³ See generally Deposition Transcripts of National Science Panel, *In re Silicone Gel Breast Implants Prods. Liab. Litig.* 887 F. Supp. 1447 (N.D. Ala. 1999) <<https://web.archive.org/web/20170303061311/http://www.fjc.gov/BREIMLIT/mdl926.htm>> accessed 28 August 2018.

⁴⁴ Learned Hand, ‘Historical and Practical Considerations Regarding Expert Testimony’ (1901) 15 *Harvard Law Review* 40-58, 54.

⁴⁵ *Daubert* (1993) 595. The preoccupation with methodology continues even though, in *Gen. Elec. Co. v. Joiner*, 522 U.S. 136, 146 (1997), the Supreme Court distanced itself from *Daubert*’s distinction between methodology and conclusions.

⁴⁶ The final ruling in *Daubert* on remand from the Supreme Court, *Daubert v. Merrell Dow Pharm., Inc.*, 43 F.3d 1311, 1320 (9th Cir. 1995), seems to have been very influential in encouraging the idea that proof of RR > 2 is required for epidemiological evidence to be even admissible. For history and critique, see Susan Haack, ‘Risky Business: Statistical Proof of Specific Causation’ in Haack, *EvidenceMatters* (n 19) 264-92.

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the “Bradford-Hill criteria,”⁴⁷ and the like are sometimes given a completely artificial legal importance; why the pseudo-rigorous ACE-V fingerprint-identification “methodology” passes legal muster;⁴⁸ why adversarial testing under cross-examination in court is sometimes confused with real-world empirical testing;⁴⁹ and why the scientific peer-review process, developed for very different purposes, is sometimes taken as a straightforward indication of evidentiary reliability.⁵⁰

Well, as I warned you, this is only the twenty-minute version of that hypothetical Ph.D. dissertation; and now I’m just about out of words. I hope, however, at least to have shown that, if we are to understand why the interactions of science and the legal system can be so problematic, we need to do much more than gesture vaguely towards supposed “different legal and scientific modes of thinking”—to think through the consequences of the different purposes, constraints, and cultures of science and of the law.⁵¹

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⁴⁷ Austin Bradford Hill, ‘The Environment and Disease: Association or Causation’ (1965) 56 *Proceedings of the Royal Academy of Medicine* 295-300. For a history and critique of how Hill’s work has been used by attorneys and judges, see Susan Haack, ‘Correlation and Causation: The “Bradford Hill Criteria” in Epidemiological, Legal, and Epistemological Perspective,’ in Haack, *Evidence Matters* (n 19) 239-63.

⁴⁸ See, e.g., *United States v. Havvard*, 117 F. Supp. 2d 848, 854 (S.D. Ind. 2000).

⁴⁹ *ibid.*

⁵⁰ See again Haack, ‘Peer Review and Publication: Lessons for Lawyers’ (n 19).

⁵¹ My thanks to Pamela Lucken for help in finding relevant materials and Nicholas Mignanelli for help in formatting the references; and to Mark Migotti and my audiences in the Faculty of Law at the University of Amsterdam, the Faculty of Law at the University of Alicante, and the Instituto de Investigaciones Jurídicas at UNAM for helpful comments and suggestions.

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