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Falsifiability Revisited: Popper, *Daubert*, and Kuhn

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Abstract
The Supreme Court's 1993 *Daubert v. Merrell Dow Pharmaceuticals* decision acknowledged a change in the Federal Rules of Evidence for the admissibility of expert scientific testimony in legal proceedings. Two of the most controversial aspects of the decision were the Court's general comments about science, and its appeal to Karl Popper's notion of falsifiability as "a key question to be answered in determining whether a theory or technique is scientific knowledge that will assist the trier of fact." Indeed, Chief Justice Rehnquist acknowledged in his dissenting opinion that he did not know what falsifiability meant and that he thought other judges would not understand it either. This paper explains what Popper meant by falsifiability, why it has been misunderstood, why it is important today, and how the Court's decision reflects the larger move from foundationalism to fallibilism that has taken place in epistemology over the course of the twentieth century.

I

> *The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game*. — Karl R. Popper (1959, 53)

In its 1993 *Daubert v. Merrell Dow Pharmaceuticals* decision (henceforth "*Daubert*"), the Supreme Court ruled that the seventy year-old "*Frye* test," which says that scientific knowledge is inadmissible as evidence unless it is generally accepted in the field,[1] had been superseded in 1975 by the Federal Rules of Evidence. The Court went on to assign a "gatekeeping" role to federal trial judges according to which they "must ensure that any and all scientific testimony or evidence admitted is not only relevant, but reliable" (1993, 589). And, in an effort to guide them in fulfilling this role, it made some "general observations" about science, in which testability (or falsifiability), peer review, reliability, and general acceptance were cited as important factors in determining whether the theory or technique upon which expert testimony is based constitutes scientific knowledge.

The Court ruled unanimously that *Frye* is no longer valid federal law. But the gatekeeping role it envisioned and its general observations about science were more controversial. Perhaps the most controversial of these was its reference to Karl Popper's notion of "falsifiability" as a "key question to be answered in determining whether a theory or technique is scientific knowledge that will assist the trier of fact" (1993, 593). Chief Justice Rehnquist, for example, wrote a dissenting opinion in which he said:

> I defer to no one in my confidence in federal judges; but I am at a loss to know what is meant when it is said that the scientific status of a theory depends on its "falsifiability," and I suspect some of them will be, too (1993, 600).

Many philosophers and sociologists of science agree. Some, echoing Rehnquist, say that the Court ventured outside its expertise and made judgments whose reach exceeded its grasp. They say that the very fact that the Court attempted to define science—a goal that has eluded the best efforts of 20th century philosophers—shows the superficiality of its view. Others seem more disturbed by the fact that the Court cited *Popper* in its decision. They say that its survey of the philosophy of science was too narrow—and, ironically, that Popper's idea is not generally accepted in the field. And they joke that dragging falsifiability into the courtroom was an attempt to solve a problem about junk science with junk philosophy. There is, however, more at stake here than the meanings of "science" and "falsifiability." The shift from *Frye* to *Daubert* reflects the epistemological shift from foundationalism to fallibilism that has occurred in philosophy and science over the past hundred years, and it attempts to resolve practical problems regarding the authority of scientific knowledge that result from it. The problem in *Daubert* is how to ensure that expert testimony rises above subjective belief and unsupported speculation—*given that scientific knowledge is fallible and cannot be rationally justified or shown to be true*.

The issue about the admissibility of expert scientific testimony is important because expert testimony is given privileged treatment under the Federal Rules of Evidence,[2] because juries usually give greater credence to scientists than to other witnesses, and because the philosophy of science that is most influential today makes it difficult to understand how scientific knowledge can be regarded as objective and rational at all. The *Frye* test made perfect sense when we could assume that the general acceptance of a scientific theory was due to its justification and that its justification guaranteed its truth, or at least its probable truth. Most philosophers of science assumed these things when *Frye* was introduced in 1923. They regarded science as objective and rational as a result. Rudolf Carnap expressed it best when he said that verifiability distinguishes science from nonsense. Carnap described science as a communal activity in which "each collaborator contributes only what he can endorse and justify before the whole body of his co-workers." He said that in this way "stone will be carefully added to stone and a safe building will be erected at which each following generation can continue to work" (Carnap 1928, vii). But Popper changed all that. Popper argued that no scientific theory can be justified or shown to be true, that scientific theories are thus inherently fallible and subject to revision, and that the objectivity and rationality of science depends not upon our ability to verify theories but upon our ability to articulate them in language and test them against reason and experience. He thus proposed falsifiability, instead of verifiability, as the distinguishing mark of science.

Falsifiability, for Popper, was an attempt to show how scientific knowledge can be both rational and objective—how we can use reason and the ideal of truth to exercise critical control over it—*despite the fact that it cannot be shown to be true*. This idea is poorly understood and badly misrepresented in the literature. Many philosophers now regard it as a degenerative research program. And today's most influential philosophy of science—Thomas Kuhn's theory of scientific paradigms and revolutions—says that science begins where criticism leaves off, and that truth plays almost no role in it at all. Indeed, Kuhn wrote that scientists accept and reject their most important theories, their "scientific paradigms," through "gestalt shifts" that are more akin to religious conversions than anything else. He wrote, in distinguishing his views about science from Popper's, that "it is precisely the abandonment of critical discourse that marks the transition to a science" (1970[a], 14). He said that "scientists sometimes correct bits of each other's work, but the man who makes a career of piecemeal criticism is ostracized by the profession" (1977, 10). And while he said very little about truth, the little he did say seems to deny that it plays any role in science at all. Kuhn seems, in fact, to have been proud of this idea. He thus points out, toward the end of *The Structure of Scientific Revolutions* that:

> ...until the last very few pages the term 'truth' had entered this essay only in a quotation from Francis Bacon. And even in those pages it entered only as a source for the scientist's conviction that incompatible rules for doing science cannot coexist except during revolutions when the profession's main task is to eliminate all sets but one (1962, 171).

And he goes on to ask:

> Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal? (1962, 172).

Kuhn wrote that paradigm shifts are made "not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt shift" (1962, 122). He said that scientists must "commit" themselves to scientific paradigms; that they must do so on "faith"; and that they must try to "convert" others, if at all, by "persuasion". And he wrote that:

> The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith (1962, 158).

Kuhn described science as an expert community whose membership depends upon the acceptance of a reigning "paradigm", and whose research is directed by competition for grants, appointments, and social prestige. And this, to put it bluntly, raises serious questions about whether and to what extent consensus in the scientific community is objective.

But Kuhn's description of science is only the tip of the iceberg. Science today is more closely associated with big business, big government, and big money than ever before. This association has encouraged us to equate rationality with self-interest. A growing number of scientists and universities have become entrepreneurs, a growing number of tort cases allege ill effects of various technologies, and a growing number of voices are asking how science should be regulated. These developments make it questionable whether consensus among scientists or within the scientific community should be the criterion for admitting expert testimony in legal proceedings in which large financial interests are at stake. And the rejection of the *Frye* test is forcing us to reexamine the objectivity and rationality of science, the nature of expertise, and potential conflicts of interest.

In my view, the Court's appeal to falsifiability was a welcome move that has helped to reshape our understanding of science and the extent to which the consensus of expert opinion can be used to underwrite its authority. These issues are likely to dominate our attention for years to come. And they should encourage jurists, as well as philosophers and scientists, to take a more careful look at Popper's epistemology.

II

Daubert began when two minor children and their parents (henceforth "the Dauberts") sued the Merrell Dow pharmaceutical company, alleging that the birth defects of the children had been caused by the prescription anti-nausea drug, Bendectin, which the mothers had taken while pregnant. Merrell Dow submitted an affidavit from a well-credentialed expert stating that he had reviewed the literature on Bendectin and human birth defects, and that no study had found it capable of causing malformations in human fetuses. It then moved for summary judgment, contending that Bendectin does not cause birth defects in humans and that the Dauberts would be unable to offer admissible evidence that it does.

The Dauberts did not contest Merrell Dow's characterization of the published studies. They responded instead with the testimony of eight well-credentialed experts who claimed—on the basis of their *in vitro* and *in vivo* animal studies, pharmacological studies of the chemical structure of Bendectin, and "reanalyses" of the published epidemiological studies—that the drug can cause birth defects after all.

The District Court admitted Merrell Dow's expert testimony as evidence, but excluded the testimony of the Dauberts' experts—saying that scientific evidence is admissible only if the principle upon which it is based is "sufficiently established to have general acceptance in the field to which it belongs" (*Daubert* 1993, 583). It held, more specifically, that their animal and chemical studies could not raise a causation issue given the vast amount of epidemiological data concerning Bendectin,[3] and that their reanalyses of the published studies were inadmissible because they had not been published or subjected to peer review.

The Appellate Court affirmed this decision, citing *Frye* and stating that expert opinion based on a scientific technique is inadmissible unless the technique is generally accepted in the relevant scientific community. It held that opinions based on methods that differ significantly from “procedures accepted by recognized authorities in the field...cannot be shown to be ‘generally accepted as a reliable technique’” (*Daubert* 1993, 584). And it rejected the reanalyses of the epidemiological studies as “unpublished, not subjected to the normal peer review process and generated solely for use in litigation” (1993, 584).

The Supreme Court granted *certiorari* in *Daubert*, citing sharp divisions among the lower courts regarding the proper standard for the admission of expert testimony. The *Frye* test had been the standard for seventy years. But the Dauberts said that it had been superseded by the Federal Rules of Evidence—and the Court, citing Rules 402 and 702, agreed.

Rule 402 states that:

> All relevant evidence is admissible, except as otherwise provided by the Constitution of the United States, by Act of Congress, by these rules, or by other rules prescribed by the Supreme Court pursuant to statutory authority. Evidence which is not relevant is not admissible.[4]

Rule 702 specifically governs expert testimony. It says that:

> If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise.

The Court ruled that “nothing in the text of this Rule establishes “general acceptance” as an absolute prerequisite to admissibility” (*Daubert* 1993, 588). It then emphasized that the “gatekeeping” role it envisioned has two functions: to exclude evidence that is unreliable; and to include reliable evidence that is not generally accepted. And it replied to fears that its repudiation of *Frye* would produce a scientific “free for all” by saying that “vigorous cross examination, presentation of contrary evidence, and careful instruction on the burden of proof are the traditional and appropriate means of attacking shaky but admissible evidence” (1993, 596), and that “these conventional devices, rather than wholesale exclusion under an uncompromising “general acceptance” test, are the appropriate safeguards where the basis of scientific testimony meets the standards of Rule 702” (1993, 596). Finally, it dismissed the fear that the “recognition of a screening role for the judge…will sanction a stifling and repressive scientific orthodoxy” (1993, 596) in a passage that is remarkable both for the distinction that it draws and the balance that it seeks between the quest for truth in science and the quest for truth in law:

> It is true that open debate is an essential part of both legal and scientific analyses. Yet there are important differences between the quest for truth in the courtroom and the quest for truth in the laboratory. Scientific conclusions are subject to perpetual revision. Law, on the other hand, must resolve disputes finally and quickly. The

> scientific project is advanced by broad and wide ranging consideration of a multitude of hypotheses, for those that are incorrect will eventually be shown to be so, and that in itself is an advance. Conjectures that are probably wrong are of little use, however, in the project of reaching a quick, final, and binding legal judgment—often of great consequence—about a particular set of events in the past. We recognize that in practice, a gatekeeping role for the judge, no matter how flexible, inevitably on occasion will prevent the jury from learning of authentic insights and innovations. That, nevertheless, is the balance that is struck by Rules of Evidence designed not for the exhaustive search for cosmic understanding but for the particularized resolution of legal disputes (*Daubert* 1993, 596-7).

III

Daubert is sometimes criticized for attempting to define science, and for then defining it in a way that is too difficult for judges to use. But *Daubert* does not pretend to define science at all. It says that when "faced with a proffer of expert testimony...the trial judge must determine at the outset…whether the expert is proposing to testify to (1) scientific knowledge that (2) will assist the trier of fact to understand or determine a fact in issue" (1993, 592); and that "this entails a preliminary assessment of whether the reasoning or methodology underlying the testimony is scientifically valid, and of whether that reasoning or methodology properly can be applied to the facts at issue" (1993, 592-3). But it also says that "many factors will bear on the inquiry and we do not presume to set out a definitive checklist or test" (1993, 593). It says that testability is "*ordinarily*, a key question to be answered in determining whether a theory or technique is scientific knowledge that will assist the trier of fact" (1993, 593);[5] that "the court *ordinarily* should consider the known or potential rate of error" of a scientific technique (1993, 594);[6] that publication is "but one element of peer review" and "not a *sine qua non* of admissibility" (1993, 593);[7] and that "general acceptance" can still have a bearing upon it, since "widespread acceptance can be an important factor in ruling particular evidence admissible, and "a known technique which has been able to attract only minimal support within the community,"…may properly be viewed with skepticism" (1993, 594).

It is, moreover, difficult to see how these guidelines could possibly function, either individually or collectively, as defining criteria. For the main point of *Daubert* was to *deny that general acceptance is a necessary condition for the admissibility of scientific evidence*. It thus cites "general acceptance" as "an important factor in ruling particular evidence admissible" (1993, 594) while, at the same time, saying that it "is not a necessary precondition" (1993, 597).[8]

Here, someone might object that, regardless of what *Daubert* says, the lower courts will inevitably treat the features it cites as criteria for the admissibility of scientific evidence. But this is why we have appeals. And the Court has been clear in subsequent decisions that the features of scientific knowledge that it lists in *Daubert* are not intended to be definitive.[9]

The fact of the matter is that *Daubert* and the decisions based upon it stubbornly resist attempts to interpret its general observations about science as a definition. And this, I submit, is what makes it important from an epistemological perspective.

IV

Daubert recognizes that science is or involves a quest for truth. But it also recognizes that scientific knowledge is uncertain; that it is more a matter of method than content; and that its reliability cannot be inferred from whether it is generally accepted in the field, or from whether it has been published or even submitted for peer review. It thus calls upon judges and juries to make *judgments* about the admissibility of scientific evidence in particular cases, as opposed to applying the criteria of a definition. It is these features, more than anything else, that base the decision upon Popper's epistemology.

V

This brings me to falsifiability. It is easy enough to get confused about the idea. There is, first of all, the use of the term "falsify" to describe what scientists do when they *misrepresent* the results of their research. This is not what Popper had in mind. Nor was he suggesting that theories are scientific if and only if they are false. Falsifiability is better understood as an explanation of testability, since a test is not really a test unless there is some way to fail it. But Popper introduced the idea as a solution to the problems of demarcation and induction. And it can, in my view, be best understood as an attempt to account for the objectivity and rationality of empirical scientific knowledge, *given the collapse of foundationalism*.

Foundationalism said that scientific knowledge is objective and rational because it is *justified*, and that it is justified because it is validly deduced from knowledge derived from an indubitable and infallible source. Descartes identified the God-given intellect as such a source, and said that whatever we clearly and distinctly perceive with it must be true. But empiricists soon grew wary of *a priori* intuition. They said that theories must be justified *inductively* by empirical observations—or sense impressions—and demanded that we eliminate theories that cannot be based upon them. But Hume then argued that the attempt to justify scientific theories with empirical observations leads to irrationalism since inductive arguments are logically invalid—it simply doesn't follow from the fact that we have seen only white swans, no matter how many, that all swans are white—and can thus provide only psychological, as opposed to rational, justification through custom and habit. Hume thought that scientific knowledge is justified in just this way, and said that reason is and ought to be the slave of the passions. Kant, however, rejected Hume's irrationalism, and—thinking that Hume was right to say that empiricism entailed it—proclaimed that there must be *a priori* certain knowledge after all. He pointed to Euclid's Geometry and Newton's Mechanics as examples of what he called "*a priori* synthetic" knowledge, and tried to explain how it is possible by saying that the mind imposes its laws upon nature in order to observe and understand it, and that all rational minds impose the same laws.

This, as Popper understood it, was the situation in epistemology before Einstein. But Kant's attempt to salvage the objectivity and rationality of scientific knowledge collapsed when Einstein imposed a non-Euclidean geometry and a non-Newtonian physics upon nature. This shattered all hope of explaining the objectivity and rationality of science in terms of its *a priori* certain foundations. For if Kant could be mistaken about the *a priori* certainty of Newtonian Mechanics and Euclidean Geometry, then how could anyone ever claim to be *a priori* certain again? But this did not stop foundationalists, who, forgetting about Hume's demonstration that induction provides only a psychological and not a rational form of justification, attributed the objectivity and rationality of science to its inductive justification by sense impressions. Wittgenstein and the logical positivists argued—as Hume had argued before them—that the meaning of a term is reducible to sense impressions, and that verifiability is what distinguishes both science from non-science, and sense from nonsense.

Popper, however, concluded that the attempt to explain the objectivity and rationality of science by its justification had failed. We cannot rationally ground scientific knowledge upon *a priori* cognition because *a priori* cognition is subjective; and we cannot ground it upon sense impressions because inductive inference is subjective too. If we want to avoid Hume's conclusion that scientific knowledge is irrationally grounded in custom and habit, then we have to explain how it can be rational and objective *given the fact that it cannot be rationally and objectively justified*. This was Popper's problem. To solve it, he had to offer an alternative to the view that science is distinguished by its inductive method, and an alternative to the idea that the objectivity and rationality of scientific knowledge depend upon its justification. Falsifiability was his way of doing both.

Here, Popper agreed with Hume that inductive justification leads to irrationalism—but denied that scientists need, or typically use, induction at all. He agreed with Kant that observation and understanding presuppose *a priori* ideas—but denied that *a priori* ideas are objectively and certainly true. And he agreed with the positivists that we can no longer appeal to *a priori* certain truths to justify scientific knowledge—but argued that verifiability cannot demarcate science from non-science, let alone sense from nonsense, because scientific laws cannot be verified. Popper argued that the so-called "basic statements" that report our observations—statements such as "I see a white swan here and now"—can *never* entail the truth of a strictly universal statement, such as "All swans are white," no matter how many of them you have. But he also argued that one *genuine* counter-example can show that a universal theory is false—and that some universal theories are *falsifiable* because they can be refuted by basic statements that contradict them.

Popper, moreover, argued that any attempt to explain the objectivity and rationality of scientific knowledge in terms of its justification will fail—since any attempt to justify our knowledge must, in order to avoid infinite regress, accept the truth of some statement without justification. He thus proposed that falsifiability—and not verifiability—is what distinguishes empirical science from non-science, and what accounts for its objectivity and rationality. For inductive inferences do not entail their conclusions, and thus cannot show that an empirical theory is true. And scientists, in any event, do not usually reason inductively—making repeated observations and then generalizing universal theories from them—but typically propose their theories as speculative solutions to problems, and appeal to reason and experience to test them.

Popper thus thought that science is empirical because we can test our theories against observations and experience; that it is fallible because our theories can fail those tests; that it is objective because we can describe problems, theories, observations, and criticisms in language, thereby making it possible for others to work on them; and that it is rational, because we can use the valid argument forms of deductive logic to criticize theories that contradict the basic statements that we think are true—and because we do not conclude from the fact that a theory has survived our tests that it has been shown to be true.

VI

Much has been written about falsifiability and much of it badly misrepresents the idea. I cannot go into great detail about this here. But I want to make a few philosophical and historical remarks to set things straight.

Falsifiability is often said to suffer from the same problems as verifiability, and to give an inaccurate description of how science actually works. David Goodstein repeats these criticisms in the Federal Judicial Center's *Reference Manual on Scientific Evidence*. He writes that Popper's ideas "fall short in a number of ways in describing correctly how science works" (2000, 71):

> The first of these is the observation that, although it may be impossible to prove a theory is true by observation or experiment, it is nearly just as impossible to prove one is false by these same methods. Almost without exception, in order to extract a falsifiable prediction from a theory, it is necessary to make additional assumptions beyond the theory itself. Then, when the prediction turns out to be false, it may well be one of the other assumptions, rather than the theory itself, that is false (2000, 71).

Goodstein goes on to say that:

> The apparent asymmetry between falsification and verification that lies at the heart of Popper's theory thus vanishes (2000, 71).

and that:

> Popper's notion that the scientist's duty is then to attack that theory at its most vulnerable point is fundamentally inconsistent with human nature. It would be impossible to invest the enormous amount of time and energy necessary to develop a new theory in any part of modern science if the primary purpose of all that work was to show that the theory was wrong (2000, 71).

Finally, Goodstein writes—in a section ironically entitled "Some Myths and Facts About Science" containing paragraphs labeled "Myth" juxtaposed to paragraphs labeled "Fact"—that "the incorrect notion that all theories must eventually be wrong is fundamental to the work of both Popper and Kuhn" (2000, 71).[10] Each of these points distorts Popper's views about falsifiability. This is easy to see by reading what Popper himself wrote. These misconceptions are widespread in the literature, and I suspect that Goodstein is merely repeating what he found there. But Goodstein presents them as established facts. And his account, for this reason, is likely to have a strong effect upon federal judges who use this manual without checking whether or not what he says is true. I would, for this reason, like to consider each of them in turn.

Goodstein's first point—that "it is nearly just as impossible" to falsify a theory as it to verify one—*is* true. But Goodstein thinks that it is an unintended problem with falsifiability, whereas Popper regarded it as part of the idea. Popper held that falsifications are *never* conclusive or final, that it is *always* possible to reexamine the evidence, and that our conjectures and refutations are *always* subject to revision. He thus wrote:

> In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding (1959, 50).

And Popper was very clear that the basic statements that report our observations, and that falsify or corroborate a theory, are not justified by our observations, but accepted or rejected by *decisions* that we can always reevaluate:

> Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we *decide to accept*. If we do not come to any decision, and do not accept some basic statement or other, then the test will have led nowhere. But considered from a logical point of view, the situation is never such that it compels us to stop at this particular basic statement rather than at that, or else give up the test altogether. For any basic statement can again in its turn be subjected to tests, using as a touchstone any of the basic statements which can be deduced from it with the help of some theory, either the one under test, or another. This procedure has no natural end. Thus if the test is to lead us anywhere, nothing remains but to stop at some point or other and say that we are satisfied, for the time being (1959, 104).

Finally, a falsifying basic statement does not necessarily contradict the specific theory we are testing, but the conjunction of that theory with a long list of assumptions—the so-called "background knowledge"—that we may not even be able to fully articulate. Any or all of the statements in this conjunction may be false, and we may well have different opinions about which of them is actually false.

I can, perhaps, put the matter in an entirely different way. Valid deductive arguments *never* show that their conclusions are true. The most they can do is to present us with a choice. Either the conclusion deduced from the premises of a valid argument is true, or some of the premises from which we deduced it are false. The value of the argument is that it clarifies the alternatives between which we must choose if we do not want to contradict ourselves. But it cannot make the choice for us, and it cannot force us to avoid contradicting ourselves.

The need to choose, or to make a decision, is thus fundamental to Popper's idea of falsifiability—and Popper, ironically enough, likened it to the verdict in a trial by jury.[11] Goodstein, however, thinks that the fact that falsifications are never conclusive and that they depend upon decisions means that Popper and the inductivists are in the same epistemological boat—or, as he puts it, that "the apparent asymmetry between falsification and verification that lies at the heart of Popper's theory thus vanishes" (2000, 71). And this is also true—*if, but only if, we mean the same boat of uncertainty*. Everybody is in *that* boat. But Popper, once again, *begins* with the idea that scientific knowledge is fallible and uncertain. The issue for him is not whether it is certain or can be shown to be true, but whether and how it can be objective and rational *given that it is not certain and cannot be shown to be true.* And here, the point to be made is that the epistemological boats are very different when it comes to objectivity and rationality.

There are no such things as conclusive falsifications because human beings are fallible and may always be mistaken in thinking that a statement is true. But we cannot verify universal theories with observations because the inductive inferences that we would use to do so are *invalid*. Valid arguments are inconclusive because we can always be mistaken about their premises. But the conclusions of invalid arguments *do not follow* from their premises. This means that they may be false *even if all of their premises are true*. The upshot is that we can regard them as justifying their conclusions only by taking a subjective leap—only, that is, by going beyond the information their premises give. Such leaps may no doubt be motivated by observation. But they always go beyond the evidence—which is why the conclusion of an inductive argument may be false even if all of its premises are true.

This is the asymmetry between falsification and verification. If we could have infallible knowledge of our observation statements, then they could conclusively falsify the conjunction consisting of the theory under test, its initial conditions, and our background knowledge. This would not tell us that the theory we want to test is false. But it would tell us that *something* in that conjunction is false—and force us to make a choice in order to avoid contradicting ourselves.

The situation is entirely different with inductive arguments. Infallible knowledge of their premises is not enough to verify their conclusions, because their conclusions do not follow from their premises in the first place.

But why can't inductivists say, à la Popper, that "nothing remains but to stop at some point or other and say that we are satisfied, for the time being"?

Popper would not object were this all that inductivists said. But they typically want to say much more: not merely that they are satisfied with their theory, but that their theory is *justified* by the evidence and that its justification makes it objective and rational—*and that you must accept it too*.

This brings me to Goodstein's third point—the idea that Popper's prescription that scientists should try to falsify their own theories is "fundamentally inconsistent with human nature.” Now whether or not a scientist actually tries to falsify his own theory is not crucial to Popper’s view of science, *so long as other scientists are not prevented from trying to falsify it*.[12] But “fundamentally inconsistent with human nature” is hyperbole, and it is refuted by the fact that many scientists actually *have* searched for flaws in their own theories. And Popper, contrary to Goodstein, did not think that that “the primary purpose” of developing a new theory is to show that it is wrong. He thought that the primary purpose of a scientific theory is to solve a scientific problem—and that our primary purpose in *testing* a theory is not to show *that* it is false, but to discover, *as best as we can*, whether and to what extent it is true. Indeed, Popper actually envisioned a role for dogmatism in science, because someone who is dogmatically committed to a theory will help us to test it by defending it in ways we might not otherwise imagine. Goodstein, however, says that it is “fundamental” to Popper “that all theories must eventually be wrong.” And this is simply false. Popper held that science aims at truth; that truth is its most important regulative ideal; and that a theory may be true despite the fact that we cannot prove that it is. Goodstein may be right that the idea that all theories must eventually be wrong is a myth that needs correction. But his idea that it is fundamental for Popper is another myth and needs correction too.[13]

But *should* scientists try to falsify their own theories? *Should* they try to expose them to the most difficult tests they can design? *Should* they regard it as their duty to do so? The answer—so long as they are trying to discover truth—is “Of course they should!” And we need not talk about “duty” to see why. People are often in a better position to criticize their own theories. They often know their weaknesses better than others do. And, knowing what their weaknesses are, they often know the best ways to test them. Goodstein says that he knows of no one who has won the Nobel Prize by falsifying his own theory. But there is a long list of Nobel Prize winners—Peter Medawar, John Eccles, Friedrich Hayek, to name just three—who have attested to the influence that Popper had upon their own scientific development.

VII

Here I would like to make some more specific comments about falsifiability, and about why Popper’s ideas about it have been misrepresented. Popper used to tell his students that he proposed falsifiability in an effort to replace Science with a capital “S” with science with a small “s”: that he wanted, in other words, to show that science is a human affair, and a highly fallible affair; that scientists make mistakes just like everyone else, and perhaps even more than other people because they have more opportunities to make them; that the best we can do in science is to try to eliminate our errors; and, most important, that there is no such thing as a Scientific Knowledge that can speak *ex cathedra*. Popper used to explain falsifiability to his students with his so-called “tetradic schema”:

$$P_1 \rightarrow TT \rightarrow EE \rightarrow P_{2,}$$

Here, P_1 is a problem that we want to solve, TT is a theory that we tentatively offer to solve it, EE is an attempt to eliminate errors through criticism, and P_2 is a new problem that emerges from our criticism. Popper knew that the schema is an oversimplification. For we typically work with several different problems and theories at once, and our attempts to criticize our theories are typically interrelated in ways that simply cannot be captured by an arrow. But the point of the schema was to emphasize the role of problems in scientific inquiry and his vision of science as a never-ending project.

This, indeed, was Popper's first rule of scientific method:

> The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game (1959, 53).

Carnap had an entirely different view, according to which science has special authority because it uses *empirical facts* to *verify* its theories, and scientists thus have the right to speak with authority because they are the ones who really *Know*. Kuhn rejected the idea that theories can be verified, but replaced it with the idea that the scientific community should protect its fundamental theories—and hence its authority—from criticism. They both differed with Popper about whether the objectivity and rationality of science result from exposing our theories to contrary evidence or from insulating them from criticism. And the difference in their methodologies and Popper's, when it comes to objectivity and rationality, is about whether and to what extent they try to protect scientific theories and the authority of the scientific community from criticism. Carnap's early "bedrock foundationalism" tried to protect them by demanding rational justification. But his later philosophy was very similar to Kuhn's—despite their obvious differences in idiom. They both said that our most important theories—the paradigms for Kuhn, and the answers to the so-called "external questions" for Carnap—cannot be rationally justified or rationally criticized, but must be accepted or rejected by conventions they regard as non-rational. I call their position "floating foundationalism," since it retains the structure and purpose of traditional foundationalism, while leaving the foundations themselves floating in midair.

Popper intended his rules of method to promote the critical attitude. He did not insist upon *conclusive* proof or disproof, which, he said, "is the very reverse of that critical attitude which" is "the proper one for the scientist" (1959, 50). He wanted, on the contrary, to discourage us from treating scientific theories as incontrovertible truths. Carnap, however, said that Popper exaggerated his differences with the positivists. And Kuhn portrayed him as a "naïve falsificationist" who believed that conclusive falsifications are possible; that one falsifying observation is, or ought to be, enough to reject a theory; and that anyone who does not reject a theory after one falsifying observation is acting irrationally. Popper's own writings stubbornly resist this interpretation. But while Kuhn eventually admitted that Popper was not really a naïve falsificationist, he nonetheless said he could be legitimately treated as one (1970[a], 14).

Both Carnap and Kuhn thus tried to reduce falsifiability to a straw man they could knock down and replace with an epistemology in which the collective decisions of the scientific community both underwrite the authority of scientific knowledge and insulate it from criticism. What is at stake in this dispute is whether and to what extent science is objective and rational. Protecting a theory from criticism can go a long way toward producing a feeling of conviction among its adherents. But Popper thought that "a feeling of conviction can never justify a scientific statement," even if it is shared by an entire community, and that the idea that it can is “incompatible with the idea of scientific objectivity” (1959, 46).

VIII

Here, I do not want to be misunderstood as denying the affinities between Popper and Kuhn. They both focused upon the ways in which established scientific theories are replaced by new ones. They both saw new theories as addressing problems that established theories are unable to solve. They both rejected the idea that science grows through the accumulation and generalization of facts. They both emphasized the logical and psychological priority of theory to observation. They both held that scientific change cannot be explained by logic alone, and that we must be sensitive to the human values that govern the traditions in which scientific problems arise if we want to understand it. And they both rejected the view that scientific knowledge can be rationally justified.

There are, however, differences. And their differences are most telling when it comes to the objectivity and rationality of science and to the roles they envision for truth, criticism, and the scientific community. Consider Kuhn’s approach to the problem of theory-choice:

> ...take a group of the ablest available people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the ideology, current in their discipline (and to a great extent in other scientific fields as well); and, finally, let them make the choice (1970[b], 237).

This may seem uncontroversial. But it has a clear potential for abuse when we combine it with Kuhn’s ideas that normal scientists must have faith in a paradigm, and commit themselves to it, and that “the profession” should ostracize those who are critical of it. Indeed, the very idea of a “profession” has always been more closely associated with *professing*—with religious vows and proclamations of faith—than with scientific inquiry. Yet this is the idiom Kuhn uses to describe what he calls “normal science.”[14] And he apparently was well aware of its implications. For he wrote in *The Structure of Scientific Revolutions* that:

> Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? That would be like admitting that they had been wrong and their opponents right. To them, at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see past history in the same way (1962, 166).

The authority of science, according to foundationalism, was predicated upon the rational justification of its theories—and not the justification of its theories on its authority. But rationality, in Kuhn, gives way to solidarity. His philosophy advises scientists to commit themselves to a paradigm, and to ostracize those who do not. And this raises serious questions when scientists are accorded special authority in legal proceedings regarding issues that put their own interests on the line.

IX

But what about reliability? *Daubert* says that a trial judge must ensure that the scientific testimony admitted as evidence is reliable. And this seems to conflict with Popper's view that scientific knowledge is not—and should not be regarded as—reliable. But here, it is useful to recall the distinction that *Daubert* draws between the quest for truth in science and the quest for truth in law—and the distinctions that Popper draws between the theoretician and the man of practical action on the one hand, and the corroboration and confirmation of theories on the other.

Popper acknowledged that a theoretician might decide to accept no theory at all "if he cannot make sure of finding the true theory among competing theories" (1972, 21). And he wrote that "we should not 'rely' on any theory for no theory has been shown to be true, or can be shown to be true" (1972, 21). But he also wrote that "a man of practical action has always to choose between some more or less definite alternatives, since even inaction is a kind of action" (1972, 21). And he said that we should choose the best-tested theory as a basis for action, and that such theories may even be called "reliable"—if by this we mean only that they have thus far survived critical testing, and not that they have been shown to be true or that they will continue to survive critical testing in the future. He wrote:

> Of course, in choosing the best-tested theory as a basis for action, we 'rely' on it, in some sense of the word. It may therefore even be described as the most 'reliable' theory available, in some sense of this term. Yet this does not say that it is 'reliable'. It is not 'reliable' at least in the sense that we shall always do well, even in practical action, to foresee the possibility that something may go wrong with our expectations (1972, 22).

These remarks suggest a distinction between different senses of "reliability" and "rely", which are captured by Popper's distinction between confirmation and corroboration. Popper thought that inductivists too often appeal to "empirical evidence" that, while consistent with the truth of their theories, does little or nothing to support it. He also thought that they generally misunderstand the nature of the support that observation and empirical tests can provide a theory—and that these errors have allowed pseudo-sciences, such as Marxism and psychoanalysis, to claim high degrees of confirmation without being put to the test. He thus wrote that:

> Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory—an event which would have refuted the theory (1963, 36).

And that:

> Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory (1963, 36).

The difference between "corroborating evidence" and "confirming evidence" bears directly upon how we should interpret *Daubert's* remarks about reliability. Popper did not want to use "confirmation" to describe how well a theory has stood up to tests, because it suggests that theories that have stood up well to tests have been "firmly established" or "put beyond doubt" or "proved". He preferred the term "corroboration" precisely because it does not have these connotations. Here, the difference between corroborating and confirming evidence, and between corroboration and confirmation more generally, lies not so much in the evidence itself as in our interpretation of how it supports a theory. Corroborating evidence supports a theory only in the sense that it *does not contradict* what the theory would lead us to expect. It cannot firmly establish a theory or put it beyond doubt. Its strength *as evidence* lies partly in the fact that it is what we would expect were the theory in question true, *and partly in the fact that it runs counter to what we would expect if one of its competitors were true*. Corroboration, for this reason, is essentially concerned with a theory's *past* performance relative to other theories. A high degree of corroboration may lead us to prefer one theory to another, but it says nothing whatsoever about whether that theory will continue to perform reliably in the future.

This brings me back to Popper's comparison of the theoretician with the man of practical action. The distinction, of course, is an idealization. Many scientists now see themselves more as men of practical action—if not businessmen and entrepreneurs—than theoreticians. This is also a result of the collapse of foundationalism. And it may even be a positive development, since the Cartesian refusal to accept a theory unless we can show it to be true was a virtual recipe for irrationalism. But it is worrisome to the extent that it encourages scientists to forgo that "broad and wide-ranging consideration of a multitude of hypotheses" that the Court presupposes as a condition for its quest for truth in the courtroom. This is because the sort of reliability that we get from testing our theories depends entirely upon the severity and integrity of those tests.

I want to emphasize this point, partly because many people are attracted to Kuhn's idea that normal science begins where criticism leaves off, and partly because it is easy to see the potential dangers of this idea when science relies upon the support of big business and big government. Popper said that the rise of normal science would be the end of science as he knew it (1974, 1146). He thought that it was linked to the rise of big science, and that the two might together destroy the growth of great science (1994, 72). I think that it is, or should be, clear that the alliance between normal science and big science might weaken our judicial system as well. It is neither here nor there whether individual scientists try to criticize and test their own theories—so long as others do so and are not prevented from doing so. But this is precisely where Kuhn's idea of normal science becomes problematic. And the problem is magnified when *normal* science becomes *big* science.

I do not see why Popper's idea that scientists should subject their theories to serious and severe tests is incompatible with human nature, or why it should make it impossible to invest the time and energy necessary to develop a new theory, as some people say. But it is all too easy to see why it might not be in the interests of the agencies that fund scientific research—or the scientific community and the individual scientists that rely upon their support. It is also easy to see how the scientific community and individual scientists might be tempted to support theories and projects that secure them funding.

And it is easy to see how and why individual scientists, the scientific community, and the agencies that support their work might be tempted to cut corners, to resort to tests that are neither serious nor severe, and to ignore negative results—if this is what it takes to "justify" the costs of developing their theories and bringing the products that are based upon them to market. These temptations are not merely theoretical possibilities. The fear that we may succumb to them lies at the very heart of the gate-keeping role that the Court envisioned in *Daubert*. And I emphasize them here because I think that the success of our judicial system depends not so much upon the courts always getting things right, but upon our belief that their mistakes are due to honest human fallibility—and not to special interests, bias, and money. The alliance between normal science and big science has not always inspired faith in this regard. And if science is no longer able, or willing, to engage in the serious consideration and testing of hypotheses that the Court envisioned in *Daubert*, then it may weaken its ability to make decisions, and our trust in the decisions that it makes.

X

I want to end with a few words about junk science, and junk philosophy.

Popper once told me about the theory of cosmic ice. The theory is now largely forgotten, but it was in vogue in Austria in the early twentieth century and even made its way into Robert Musil's *The Man Without Qualities*. It was essentially an attempt to explain the phenomenon of hail. Its basic idea was that hail, which sometimes seems to appear quite literally out of the blue, results from the sudden breakup of huge balls of ice that exist in outer space—hence, the term "cosmic ice."

Popper explained the theory to me in great detail, emphasizing the phenomena it was designed to explain and the problems it encountered and was unable to solve. He seemed to take the theory very seriously while he told me about it. But I remember him saying, as he came to an end, that "This, of course, is part of what one might call 'the lunatic fringe of science'." This last sentence was followed by a long pause, after which he added, "Of course, it may be very difficult at the time to tell whether or not something is part of the lunatic fringe of science."

Some sociologists and philosophers now joke that Popper's falsifiability criterion is junk philosophy that is used by junk scientists to defend junk science. But it is important to remember that science is and has always been a *project*—and that we have no guarantee that the project will ultimately succeed, or that our theories would necessarily be true even if it did, or that we will continue to get funding for it. It is also important to remember that the project of science—the project, that is, of explaining what we do not understand by appealing to natural causes instead of supernatural forces—is not itself falsifiable: that our failure to give natural explanations of what we do not understand does not mean that natural explanations cannot be given, and that no matter how many times we try and fail we can always try again.

The heyday years of scientism that we call ‘the twentieth century’ are now at an end—not because today’s science is any worse (it is not) but because we have, as a society, become more realistic about what it can and cannot do. There are, no doubt, plenty of people who still deify Science. And there are, of course, still plenty of people who demonize it. But we are, as a society, more closely approximating Popper’s own attitude—a deep admiration for science and its achievements, coupled with a clear awareness that even the best of it is fallible and subject to revision—even if some people still think that we can show that certain theories are true, or that “the science is settled,” or “the debate is over.” Scientism may well see a resurgence in the era of big data and big science. And I do not, of course, have any doubt that junk philosophy and junk science exist. But my own sense is that the quest for truth in philosophy and science should, in this respect, be more like the quest for truth in law—where vigorous cross examination, presentation of contrary evidence, and careful instruction on the burden of proof are the traditional and appropriate means for attacking shaky but admissible theories.

I say this, partly because I suspect that terms like “junk science” and “junk philosophy” are too often bandied about when we do not have rational and objective arguments to offer—and partly because I think that the success of our judicial system does not depend upon its always getting things right, but upon our trust that the mistakes it makes are due to honest human fallibility and not to the fact that old blind justice has her thumbs on the scales.

About the Author

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[1] The test takes its name from *Frye v. United States*, a case in which the U.S. Court of Appeals, D.C. Circuit, held that:

Just when a scientific principle or discovery crosses the line between the experimental and demonstrable stages is difficult to define. Somewhere in this twilight zone the evidential force of the principle must be recognized, and while courts will go a long way in admitting expert testimony deduced from a well recognized scientific principle or discovery, the thing from which the deduction is made must be sufficiently established to have gained general acceptance in the particular field in which it belongs (*Frye*, 1014).

[2] Experts, unlike other witnesses, need not testify from personal knowledge; they need not testify to the

foundation for their opinions; and they may testify directly on the “ultimate issues” to be decided by the jury. (See Federal Rules of Evidence, 704.)

[3] More than 30 published studies involving over 130,000 patients.

[4] Here, “relevant evidence” is defined as that which has “any tendency to make the existence of any fact that is of consequence to the determination of the action more probable or less probable than it would be without the evidence” (*Daubert* 1993, 587).

[5] My italics.

[6] My italics.

[7] “The fact of publication (or lack thereof) in a peer reviewed journal thus will be a relevant, though not dispositive, consideration in assessing the scientific validity of a particular technique” (*Daubert* 1993, 594).

[8] The justices explicitly reiterate this point in their summary:

To summarize: “General acceptance” is not a necessary precondition to the admissibility of scientific evidence under the Federal Rules of Evidence, but the Rules of Evidence—especially Rule 702—do assign to the trial judge the task of ensuring that an expert’s testimony both rests on a reliable foundation and is relevant to the task at hand. Pertinent evidence based on scientifically valid principles will satisfy those demands (*Daubert* 1993, 597).

[9] In *General Electric Co. v. Joiner*, the Supreme Court held that “abuse of discretion is the appropriate standard” that “an appellate court should apply in reviewing a trial court’s decision to admit or exclude expert testimony under Daubert v. Merrell Dow Pharmaceuticals” (1997, 128-9). It reiterated the point in *Kumho Tire Co. v. Carmichael*:

As the Court stated in Daubert, the test of reliability is “flexible,” and Daubert’s list of specific factors neither necessarily nor exclusively applies to all experts or in every case. Rather, the law grants a district court the same broad latitude when it decides how to determine reliability as it enjoys in respect to its ultimate reliability determination (1999, 141-2).

Each of these decisions upheld decisions that were made at the district level to exclude expert testimony as evidence. In *Joiner*, for example, the district court had decided to exclude expert testimony, based upon studies of mice, that workplace exposure to PCBs was the likely cause of Robert Joiner’s cancer. This decision was reversed by the appellate court, which held that “because the Federal Rules of Evidence governing expert testimony display a preference for admissibility, we apply a particularly stringent standard of review to the trial judge’s exclusion of expert testimony.” But the Supreme Court ruled “that the Court of Appeals erred in its review of the exclusion of Joiner’s experts’ testimony,” saying that “in applying an overly “stringent” review to that ruling, it failed to give the trial court the deference that is the hallmark of abuse of discretion review.” It also indicated that the district court’s discretion extended to judgments regarding the reliability of specific expert testimony based upon specific scientific studies.

[10] It is interesting to contrast this idea with the following equally erroneous account of Popper’s philosophy that appears in the article on Popper in the *Encyclopedia Britannica*:

Popper argued…that hypotheses are deductively validated by what he called the “falsifiability criterion.” Under this method, a scientist seeks to discover an observed exception to his postulated rule. The absence of contradictory evidence thereby becomes proof of his theory (*Encyclopedia Britannica* 1979, Volume 9, 609).

[11] Popper writes:

The *verdict* of the jury (*vere dictum* = spoken truly), like that of the experimenter, is an answer to a question of fact (*quid facti*?) which must be put to the jury in the sharpest, the most definite form. But what question is asked, and how it is put, will depend very largely on the legal situation, *i.e.* on the prevailing system of criminal law (corresponding to a system of theories). By its decision, the jury accepts, by agreement, a statement about a factual occurrence—a basic statement, as it were. The significance of this decision lies in the fact that from it, together with the universal statements of the system (of criminal law) certain consequences can be deduced. In other words, the decision forms the basis for the *application* of the system; the verdict plays the part of a ‘true statement of fact’. But it is clear that the statement need not be true merely because the jury has accepted it. This fact is acknowledged in the rule allowing a verdict to be quashed or revised.

The verdict is reached in accordance with a procedure which is governed by rules. These rules are based on certain fundamental principles which are chiefly, if not solely, designed to result in the discovery of objective truth. They sometimes leave room not only for subjective convictions but even for subjective bias. Yet even if we disregard these special aspects of the older procedure and imagine a procedure based solely on the aim of promoting the discovery of objective truth, it would still be the case that the verdict of the jury never justifies, or gives grounds for, the truth of what it asserts.

Neither can the subjective convictions of the jurors be held to justify the decision reached; although there is, of course, a close causal connection between them and the decision reached—a connection which might be stated by psychological laws; thus these convictions may be called the 'motives' of the decision. The fact that the convictions are not justifications is connected with the fact that different rules may regulate the jury's procedure (for example, simple or qualified majority). This shows that the relationship between the convictions of the jurors and their verdict may vary greatly (1959,109-10).

[12] Goodstein, incidentally, makes much the same point: "Scientists are…not Popperian falsifiers of their own theories, but they don't have to be. They don't work in isolation. If a scientist has a rival with a different theory of the same phenomena, the rival will be more than happy to perform the Popperian duty of attacking the scientist's theory at its weakest point" (2000, 73-4).

[13] Kuhn is another story. But this is not because Kuhn thought that all theories must eventually be wrong, but because he thought that it did not even make sense to talk about scientific paradigms as *being* true or false in the first place.

[14] Kuhn distinguished normal science from revolutionary science. Normal science is the kind of science that is done under the rubric of a reigning scientific paradigm that sets the worldview, problems, methods of inquiry, and models of successful work within a particular scientific discipline. Revolutionary science—or, more accurately, a revolution in science—occurs when a reigning scientific paradigm is itself called into question.