

Daubert vs. Frye: **Logical Empiricism and Reliable Science**

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Jan Dejnoška, *Daubert vs. Frye: Logical Empiricism and Reliable Science* (1996). On the U. S. Supreme Court decisions. I argue that the logical empiricists have a better understanding of the nature of scientific reliability than the so-called “revised empiricists,” and that *Frye* was a better decision than *Daubert*. This is mainly a critique of Heidi Li Feldman, but I also discuss the views of Richard D. Friedman and Brian Leiter. Others discussed include Alfred Jules Ayer, Mario Bunge, Pierre Duhem, Kenneth R. Foster, David E. Bernstein, Oswald Hanfling, Carl G. Hempel, Peter W. Huber, Otto Neurath, Karl Popper, W. V. O. Quine, Moritz Schlick, Julius R. Weinberg, and Ludwig Wittgenstein.

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1. Introduction

I shall argue in this paper that the logical empiricists have a better understanding of the nature of scientific reliability than the so-called “revised empiricists,” and that *Frye* was a better decision than *Daubert*. I shall discuss the views of scholars Heidi Li Feldman, Richard D. Friedman, and Brian Leiter. In the course of that, I shall discuss the logical empiricists and allied thinkers such as Karl Popper and Carl Hempel. I shall also summarize the four *Daubert* briefs which cite Popper.

In 1993, The Supreme Court of the United States made a revolutionary change in the federal rules of evidence. In *Daubert v. Merrell Dow Pharmaceuticals*, 113 S.Ct. 2786 (1993), the Court overturned the seventy-year-old rule of admissibility of expert scientific testimony in *Frye v. United States*, 293 F. 1013 (D.C. Cir. 1923). The older *Frye* rule was that expert scientific “testimony was to be admitted only when it had received ‘general acceptance’ in the relevant scientific community” (Huber 1991: 731). The *Daubert* rule is a directly evidential “relevancy test, which would admit any expert testimony deemed helpful and germane to the scientific issue before the court” (Huber 1991: 732). Thus the legal focus on evidential relevance shifts from the scientific community to the evidence itself. Instead of passively deferring to established scientific consensus, federal courts across the country are now to engage actively in assessing the value of scientific evidence for themselves in mass toxic torts cases, and indeed in any case involving expert scientific witnesses. The *Daubert* Court indicated that there are at least four factors to consider: testability, specifically Popperian falsifiability; publication and peer review; “the known or potential rate of error” (*Daubert* at 2797); and widespread acceptance in the relevant scientific community. Thus the *Frye* test reappears as merely the fourth factor.

The legal world immediately perceived the magnitude of the shift; a search of the Westlaw legal journals computer database on June 4, 1996 using the keyword “Daubert!” revealed that 263 legal articles have already been published at least mentioning *Daubert*. In contrast, the philosophical world seems unaware of what happened. An identical search of The Philosopher’s Index on Westlaw on June 4, 1996 revealed that not a single article title or article abstract mentions *Daubert*; there are only seven papers about Richard Daubert, an obscure German realist phenomenologist who flourished in the 1930s. Yet philosophers of science ought to be interested in this interface of science, law, and philosophy. Philosophy of science is very much implicated, since the Court’s majority opinion, written by Justice Blackmun, went out of its way to cite Popper and Hempel in support of the factor of testability, and cited Jasanoff, Horrobin, Ziman, and Relan and Angell in support of the factors of peer review and publication, as well as many more specifically legal scholars on scientific evidence. That too is a big change from old *Frye*, an opinion which refers informally only to a brief by able counsel.

In this paper, I will of course offer an answer to the main question, How much can or should courtroom judges actively assess scientific research as scientists themselves would, and why? Briefly and in favor of *Frye*, I say that they should not because they cannot. But the first topic I am principally concerned to discuss is, How much old-fashioned logical empiricist philosophy of science did the *Daubert* Court rely on--and just how old-fashioned is it? While the *Daubert* opinion is too cryptic to allow a definitive answer, I think I can at least refute a philosophical interpretation of *Daubert* in the legal literature offered by Feldman. Briefly, Feldman’s view is that logical empiricism is an out-of-date movement which committed a crucial elementary blunder in insisting on an epistemological foundationalism of absolutely certain

observation statements (or protocol statements, or what have you), a blunder fortunately corrected beginning in the 1950s by a new, very different, and more sophisticated philosophy of science she calls “revised empiricism.” Feldman approves of revised empiricism’s new insistence on the social or collectivist aspects of science, deeming publication and peer review to be very fundamental aspects of science. Indeed, it follows from Feldman’s interpretation that the *Daubert* Court should not have cited old-fashioned philosophers like Popper and Hempel, but should have cited newer and more insightful philosophers like Thomas S. Kuhn and Patricia S. Churchland, though luckily the *Daubert* Court somehow managed to come out in favor of revised empiricism in the end anyway.

In contrast, my view involves attributes less luck to the Court because I am more charitable to Popper, Hempel, and the logical empiricists in general--and to the *Daubert* Court as well. I see no radical turn from logical empiricism to revised empiricism in the 1950s. I start from the great philosopher of science Pierre Duhem as propounding in 1906 the philosophy of science Feldman places as beginning in the 1950s. I see logical empiricists as well aware of Duhem and as honoring Duhem’s insights, and as merely playing with the foundationalism Feldman accuses them of without admitting it in any significant way. Thus I conclude that the chief thing Feldman’s revised empiricism revises is history. I find that Popper and Hempel expressly repudiate absolutely certain empirical foundations for science in the very works Feldman cites as showing the opposite. I also find that in discussing only Popper and Hempel, Feldman ignores the entire movement of logical empiricism that deeply influenced merely allied thinkers like Popper and Hempel: the Vienna Circle of logical positivism; the Circle’s 1929 manifesto of logical positivism honoring Duhem and other scientific figures well aware of the importance of the scientific community to the

progress and reliability of science; and the Circle's vast *International Encyclopedia of Unified Science* project, approved in 1935 by the First International Congress for the Unity of Science--the actual execution of which happened to include Kuhn's 1962 book, *The Structure of Scientific Revolutions* as volume 2, monograph 2 of *Foundations of the Unity of Science* (Hanfling 1981: 103-4ff.) Feldman even ignores America's most famous living philosopher, Willard Van Orman Quine, an anti-foundationalist and pro-communalist philosopher of science if there ever was one. As is well known, Quine was converted to those views in the 1930s by leading members of the Vienna Circle. Quine also admits to an even earlier pragmatist heritage emphasizing the social and revisable nature of science, a heritage which also influenced the Vienna Circle. It follows from my picture of a basic underlying continuity in twentieth century philosophy of science on the importance of testability, revisability, and social community that not only did the *Daubert* Court not grievously err in its scholarship by citing Popper and Hempel, but it may well have understood those major philosophers and the general trend of twentieth century philosophy of science better than Feldman did.

2. Overview of *Daubert*

The *Daubert* Court was careful to state it was not overruling *Frye* on the scientific merits but only for the strictly legal reason that FRE 702 *legally* supersedes *Frye*. But like everybody else I will ignore this (I think very debatable) legal argument (it was debated by the parties), and talk about scientific relevance and reliability.

As Feldman puts it:

Based upon the phrase “assist the trier of fact,; the [*Daubert*] Court set a standard of relevance. The expert testimony must “fit” the case for which it is proffered; for the testimony to aid the factfinder appropriately, there must be a “valid scientific connection” between the testimony and the facts of the case. [The Court suggested] that the reliability and relevance of scientific information differs [sic] in kind from the reliability and relevance of other sorts of information. (Feldman 1995: 708)

Basically, the *Daubert* holding was that FRE 702 superseded the *Frye* test, replacing it with a “relevancy standard” of the admissibility of scientific evidence (Huber 1991: 733). This of course brings FRE 702 on expert testimony much more in line with the Federal Rules of Evidence in general, since in general relevant evidence is deemed admissible (FRE 402). However, expert testimony poses a special problem for the relevance standard. In a nutshell, it soon transpired that the new relevance standard was so liberal that all kinds of junk science of dubious probative value were being admitted. As Judge Patrick Higginbotham put it, “The decision to receive expert testimony was simply tossed off to the jury under a ‘let it all in’ philosophy” (Huber 1991: 734) (quoting Higginbotham). This has most recently led many courts to use “strict scrutiny” of the *Daubert* relevance standard. Strict scrutiny takes place in various ways, such as putting heavier weight on FRE 403, which balances probity against prejudicial effect (Huber 1991: 734). Here the idea is that junk science is prejudicial and therefore inadmissible.

We must distinguish two questions: What is scientific reliability considered in itself? and What is best for courts of law to count as scientific reliability? I emphasize this because in my

opinion, neither *Frye* nor *Daubert* is very good at explaining scientific relevance considered in itself. *Frye* does not even attempt to do so, but says only, “Whatever most of the relevant scientists believe, I believe.” In contrast, *Daubert* frames itself as officially a relevance rule, i.e., as mandating that courts use all and only scientific evidence which really helps them decide the issue, i.e., which is really relevant under the FRE 401 rule of changing the probability that some material fact is true or false. That, of course, gives no guidance. Thus far *Daubert* is saying only, “Treat as relevant evidence that which really is relevant evidence.” Thus far, *Frye* is superior to *Daubert* in that right or wrong, at least *Frye* offers *significant* guidance.

But *Daubert* goes on to say two important things. First, many factors govern what is scientifically relevant in any given situation. That is correct but again gives little guidance. Second, there are four factors in particular which are not necessary or sufficient conditions of scientific relevance, and which are not an exhaustive list, but which are at least important enough to be the only four factors the *Daubert* Court mentions (or, perhaps, can discover, or, perhaps, thinks worthy of mention). These four factors are: testability, especially falsifiability; publication and peer review; “the known or potential rate of error” (*Daubert* at 2797); and widespread acceptance in the relevant scientific community. (Thus the *Frye* test reappears as the fourth factor.) *Pace* Feldman, there is no suggestion that these factors have “equal footing” or are “equally distinctive features of science,” or have “equal and independent weight as guidelines for identifying respectable science” (Feldman 1995: 10). Feldman is reading something into the decision that the Court never says. The Court does not even address such equality issues. Probably what the Court has in mind is what is usual in multi-factor tests, that courts must weigh the factors as is appropriate in any give situation; and probably all four will never be assigned

equal weight. In fact, such ideas are simply too absurd to impose on the Court. [This] reminds me of my own usual criticism of people who claim incommensurable kinds of factors have equal weight: namely, there is not even a common yardstick to measure these four factors by, so that it is not meaningful to speak of them as equal.

The *Daubert* Court itself acknowledges that these factors are neither necessary nor sufficient conditions of scientific reliability. The *Daubert* Court is not even attempting to state what scientific relevance is, i.e., to offer a theoretical definition of scientific relevance, but to sketch the test it wants courts to use, and presumably which it thinks will be a helpful *practical* test for them to use. Thus the *Daubert* Court is scarcely putting the four mentioned factors on an “equal footing” as “equally distinctive” necessary conditions of science. The factors are just a checklist, and not even a complete list at that.

Perhaps part of the reason why Feldman thinks all the factors are equally important is that she ignores the difference between theory and practice. Even if something, say peer review, is important to the practice of good science, or is even, as we say, practically necessary, that does not make it theoretically necessary or even theoretically interesting to science.

Of course, there might *be* no set of logically necessary and sufficient conditions for scientific reliability. The expression “scientifically reliable” might be like the word “game,” and might have a cluster of different appropriate uses which bear only family resemblances to each other à la later Wittgenstein. But this is to abdicate the effort of looking for such conditions before even starting to look.

Two *Daubert* factors, publication-and-peer-review and widespread acceptance, concern the public character of science. But if you put Einstein on a desert island with superb research

facilities, it is easy to imagine that Einstein logically could be doing better science than everybody else. Perhaps the tranquillity of the island may even cause him to improve his scientific reliability. That is, it seems that these two factors are not part of what scientific reliability really is, and are merely of practical importance. My view is that they have even practical importance only because it is part of what science really is to aim to state universal laws, and that to be universal is to be repeatable in the same kinds of situations. That is why there is such a heavy emphasis on other scientists' being able to repeat the results you obtain. The reason science aims at universality is that it wants to describe uniformities in nature.

It follows that where kind K of situations occurs only once, such that an event of kind E1 causes an event of kind E2, and these two events are the only ones of their kind in the history of the universe, the scientific law "Whenever E1, then E2" can be universally true. That is, it could be true that *were* there more E1's, they *would* be followed by E2's *because* they would cause the E2's. Yet there could be no peer review or general acceptance based on successful repeatable testing, simply because there could be no repeatable testing at all. Remember, I am describing only the logical possibility of a unique K-situation, so as to probe the logically necessary conditions of scientific relevance.

Having made a preliminary point that *Daubert* factors (2) and (4) are merely practical considerations (admittedly, extremely important ones in the normal course of events), I proceed to wipe out all four *Daubert* factors, considered as necessary and sufficient conditions of scientific relevance, with a more radical counterexample than Einstein on a desert island.

Consider now this logical possibility. Einstein offers the theory, "Whenever bloop, then floop." He defines this theory as false just in case marshmallows suddenly materialize in

earthbound refrigerators. Einstein then performs the experiment of observing his own refrigerator for one year. He sees no marshmallows materialize. The jubilant Einstein now publishes his bloop-theory using the marshmallow definition of when the theory is false, and describing his experiment in great detail. The theory is successfully peer-reviewed and accepted by an adulating scientific community. It is not that they admire Einstein so much as that they test their own refrigerators and easily see that no marshmallows suddenly materialize in them. Nobody in the whole scientific community is able to falsify the bloop-floop theory, though it is clearly *falsifiable*. And the bloop-floop theory has no actual or even potential rate of error.

Greedy trial lawyers across the nation now seduce gullible citizens with all sorts of medical conditions into suing marshmallow producers and refrigerator manufacturers under the great bloop-floop theory, since these entities are as intimately implicated by the bloop-floop theory as any entity could ever hope to be. The court determines that all four of the *Daubert* factors have been amply met. All marshmallow and refrigerator producers now file for bankruptcy, for they have all been *scientifically determined* to be at fault for every ailment in the nation. After all, the bloop-floop theory passed the four-factor *Daubert* test of reliable science with flying colors. Never again are marshmallows or refrigerators seen in America. The insurance rates are too high, and corporations are too afraid of litigation to invest any money in research and development to design new kinds of marshmallows or refrigerators.

The importance of my second counterexample to Feldman's theory of science and conception of *Daubert* is that it is a caricature of a scientist who emptily goes through the motions of doing science as Feldman's *Daubert* conceives of science. In my own opinion, the *Daubert* Court wisely refrained from saying what scientific reliability really consists of, and

refrained even from attempting to list all the factors a judge or even a scientist might use to assess scientific reliability.

The refrigerator counterexample goes in the opposite direction from that of the desert counterexample to Feldman's theory of scientific reliability (and to Feldman's interpretation of *Daubert*). The desert Einstein had great science despite flunking the *Daubert* factors of publication/peer review and widespread acceptance. Here all four *Daubert* factors are satisfied, yet the science is ludicrous and could never have any scientific relevance or significance to determining a matter of fact. The question is, why?

My answer is that the ostensibly theoretical terms "bloop" and "floop" are idle wheels. They *do* nothing to relate to observations in ways that even attempt to predict or explain natural events. They do not organize natural events into a pattern. This quantitative requirement of universality of scientific law is a rock bottom, logically primitive, logically necessary condition of adequate science. But the idleness is not merely a quantitative problem. It is a qualitative problem as well. Namely, the theory does not even make scientific sense in light of any previously established science. That is, it fails to satisfy qualitative Henle-Koch-Evans (HKE) postulate 10, or what amounts to the same, Hill's qualitative criterion 9 (Foster 1993: 7-9 lists the HKE postulates and the Hill criteria). This qualitative requirement seems to be logically primitive as well, so here we touch on rock bottom a second time concerning scientific reliability. The requirement that scientific theories make scientific sense does not mean making sense *to* peers or *to* a larger community, but making intrinsic scientific sense regardless of who grasps it. In light of the foregoing considerations, I define ideally reliable science as science that truly describes a true universal pattern in nature in a way that allows for causal explanation, or at least

prediction, of natural events of at least one kind E. This definition is austere, indeed, as I said, ideal, in its insistence on *true* scientific laws. Yet by the same token, by definition it cannot go wrong in its guarantee of scientific reliability. Therefore I deem this definition a perfectly adequate definition of ideal scientific reliability.

My second definition is my theoretical definition of actual scientific reliability. I define actually reliable science as science which sufficiently approximates to ideal scientific relevance, i.e., that which is sufficiently likely to be true that a reasonable person would deem it to be reliable. The seeming weakness of the second definition is its lack of guidance in telling what *is* sufficient approximation to the ideal. But that is actually a measure of its theoretic strength and correctness. The further theory is from practice, the deeper and better theory will be, other things being equal. In fact the notion of degree of reliability is simple and clear: reliability increases as experimental replication and verification increase. It is only the cut-off point which is debatable; and in epidemiology we might simply stipulate a 95% accuracy rate for our predictions. Any definition of scientific reliability must live with the fact that such cut-off points are ultimately pragmatic and are ultimately fixed by practitioners. That is not a problem faced by my definition alone.

The chief difference between my definition of ideal reliability and my definition of actual reliability is that the former is realist; it requires true descriptions of real patterns in nature. The latter allows an instrumentalist theory of science. That is my way of cutting actual science some slack. Of course, it is perfectly possible for an instrumentalist to demand greater reliability in practice than do his realist colleagues. But nothing could be more reliable in theory than a true description of a real universal pattern in nature.

For trial judges, the most important thing about my definition of actual scientific reliability is that where any great complexity or subtlety is involved, you have to be a scientist to apply it. In contrast, both the *Frye* and *Daubert* rules are really designed for nonscientists, for outsiders. Yes, scientists themselves attach great weight to the four mentioned *Daubert* factors, and in the normal course of human affairs they could scarcely get along without them. But the *Daubert* factors are not scientific rock bottom as I just explained it. Not even the *Daubert* factor of experiment-replication-and-verification is scientific rock bottom. Replication and verification are not even mentioned in my two definitions. I mentioned universality, explanation, and prediction instead. Replication is important only because science aims to be universal. Verification is important only because science aims to be explanatory and predictive. Unlike publication-and-peer-review and widespread acceptance, replication and verification are theoretically necessary to scientific reliability. But for all that they are not theoretically essential to scientific reliability. That is, where a definition states the essence of a thing, they do not belong to the proper theoretical definition of actual scientific reliability, but [at most] they follow as necessary consequences--as corollaries, if you will--of that definition.

How can there be strong uncertainties in science? Science deals with mountains of data and it is terribly difficult to discern the best patterns by which to organize and make sense of it all. Even an Einstein, whether on the desert island or off it, is a limited, frail human being compared to the complexities that seem to exist in nature.

My distinctions may appear hair-splitting scholastic pedantry. But part of the business of philosophy is to point out things before everybody's noses, which they never or rarely pay attention to. And when we look as best we can at what scientific reliability really is, we can put

the *Daubert* factors in perspective, we can see them as the derivative and sometimes merely practical considerations they really are.

I rate peer review and general acceptance as merely practical considerations, and falsifiability and rate of error as necessary but derivative features of science. None of them belongs to the essential definition of what science is, but at most follow from that definition. Even the *Daubert* concentration on assessing the adequacy of scientific methodology as the key to the importance of the four factors it lists is a concentration on a necessary but derivative feature of science. The reason scientific methodology must be adequate is just that we want to approximate closely enough to the ideal of describing true patterns in nature on the basis of the empirical evidence, and good methodology is the primary means to that essential end. Surely we can all see that methodology is a means to an end, that it is not basic as an end. We do not do science for its own methodological sake, but for the sake of understanding, or at least predicting, nature. Perhaps understanding and predicting are in turn means to a more basic end of controlling nature as much as we can; but whether that is so might vary from scientist to scientist.

It would be useful in any case to attempt to articulate other factors besides the *Daubert* four. Commentators such as Feldman merely give lip service at best to the fact that there are supposed to be other factors to consider, and simply fall into the automatic habit of discussing only the four the *Daubert* Court mentions, even though the court itself says, “Many factors will bear on the inquiry, and we do not presume to set out a definitive checklist or test” (*Daubert* at 2796).

Listing other factors is easy. The HKE postulates amount to ten factors for assessing the scientific relevance of epidemiologic studies of nonchronic diseases, and the Hill criteria amount

to nine factors for assessing the scientific relevance of epidemiologic studies of chronic diseases (Foster 1993: 7-9). On a more basic level, Mill's five canons of inductive reasoning amount to five general factors for assessing scientific relevance. However, no list of factors relieves us of the duty of thinking through the individual data. In my opinion, let the scientists do it, then wait for their consensus à la *Frye*.

3. Feldman's views

Feldman favors *Daubert* over *Frye* because she thinks that *Daubert* comes closer to how the scientific community determines scientific reliability than *Frye* does. But on my theory of scientific reliability as explained in the last section, neither *Daubert* nor *Frye* attempts to tell us what scientific reliability is, or even how scientific relevance is really determined in the scientific community. Both *Daubert* and *Frye* aim only at stating practical rules useful for judges, rules which are logically derivative and sometimes even logically superficial for scientists. *Frye* does that transparently. It is no use to a scientist to "test" science on the basis of its widespread acceptance by the relevant scientific community. The whole question for the scientist is whether the science *ought* to be accepted by the relevant scientific community in the first place. I speak theoretically; in practice scientists rely on corporate scientific knowledge all the time; they cannot keep retesting everything at every step. But science could never *begin* that way without the most egregious bootstrapping.

If I am correct as to what *Frye* and *Daubert* are really up to, then Feldman's argument for preferring *Daubert* unravels. That is because for legal purposes, *Frye* may well be the better test for judges to use. It is more strict and conservative, and functions to prevent error far better than

Daubert. As compared to *Frye*, *Daubert* is (and was expressly intended to be) a liberal test that functions to allow more science in as relevant. I agree with Huber that *Daubert*'s liberalism has led to problems of junk science, and problems of fairness to defendant manufacturers. If Huber is right that the latest trend is toward putting restrictions on *Daubert*, then I applaud the trend. Indeed, *Daubert* may be closer to what scientists themselves do in practice. But courtroom approximation to science not a desideratum in the first place. A little knowledge is a dangerous thing.

Why does Feldman go wrong? I think there are two reasons. I described the first reason in the previous paragraph. The second reason is that Feldman uncritically swallowed the "revised empiricist" account of science, even misreading contemporary accounts. She accepts more recent figures like Kuhn and Churchland without question, and just as uncritically accuses older philosophers like Popper and Hempel of committing an elementary blunder. As I shall show, Feldman is unaware that Popper himself repeatedly warns against the very blunder she accuses him of committing, in the very book she cites as showing he commits the blunder. This is not to mention her overlooking Popper's very reasonable critique of Kuhn, raising among other things the question who is more recent than whom (Popper 1974: 1144-53).

To substantiate what I called the second reason why Feldman goes wrong, I shall now show that what Feldman's revised empiricism chiefly revises is history of philosophy. She offers an account of logical empiricism as more primitive than it really was, thus making contemporary philosophers (as she interprets them) appear more original than they really are. Insofar as revised empiricism insists with Feldman that the four *Daubert* factors are of independent and equal theoretical significance to the nature of reliable science, it is a pseudo-scientific Pythagorean

number mysticism, as if four of the factors were exactly twice as great as two. This is reminiscent of the view that the four elements are earth, air, fire, and water. But insofar as revised empiricism rejects absolutely certain epistemological foundations of science, holds that a scientific theory is a holistic web of scientific statements no one of which is immune from revision, and emphasizes the huge practical collectivist role publication, peer review, and widespread acceptance play in determining which parts of a scientific theory actually are revised in the face of recalcitrant experiences (but not necessarily which parts are best revised), it is a widely held theory of science that was advocated not only by the logical empiricists, but by even earlier figures whom they revered for developing that theory.

Feldman says:

According to logical empiricism, the dominant philosophy of science in the mid-twentieth century, testability is the sole distinguishing feature of science.

When the *Daubert* Court cited Carl Hempel and Karl Popper, two distinguished logical empiricists, as authorities for the testability guideline, the Court seemed to be committing itself to logical empiricism and its position on the distinctiveness of science. If this were the case, however, it would not have made sense for the Court to assign peer review, publication, and general acceptance equal and independent weight as guidelines for identifying respectable science. A Court fully in the grips of logical empiricism would, at most, have treated these guidelines as indicia of testability. Instead, the *Daubert* Court suggested that they possess independent significance....

Logical empiricists sought to describe scientific beliefs as logically structured systems in an effort to distinguish “real science” from “unconstrained speculation, metaphysical posing and assorted mush.” The logical empiricists held that scientifically justified beliefs either followed logically from sensory beliefs or simply were sensory beliefs. According to the empiricists, sensory beliefs are expressed in observation sentences, such as “red here now” or “there is a red chair there,” which report privately experienced sense impressions in a most minimal fashion....

Logical empiricism oversimplifies scientific experimentation, regardless of whether the scientist aims at confirmation or falsification. **The logical empiricist account presupposes that both initial conditions and observational results can be specified in basically incontestable terms**, thereby enabling the straightforward experimental confirmation or falsification of the tested hypothesis. Here is the logical empiricist model of testability, presented schematically:

1. Hypothesis
2. Initial conditions, including background assumptions.
3. Expected observational results.
4. Experiment.
5. Observed results.

If (5) coincides with (3), the hypothesis is confirmed. If (5) contradicts (3), the hypothesis is falsified. [Feldman now presents her criticism of this model:]

Presented in this form, it is easy to see that if the observed results contradict the

expected results, it is logically possible for either the hypothesis or one of the propositions in the specification of the initial conditions to be false. This complicates both confirmation and falsification.... Confirmation is not ruled out, nor is falsification established [where (5) contradicts (3)]: the falsehood may lie not in the hypothesis, but in the initial conditions, specifically within the background theories and assumptions that underpin any experiment.

Moreover, observations and observation sentences are themselves theory-dependent....This means that observations themselves are revisable not only in response to additional empirical data, but also in the name of preserving a hypothesis or a background theory....

To preserve the idea of testability, there must be some principled way of distinguishing observation sentences from theoretical ones, along with a principled way of deciding what to reject and what to revise when hypotheses and observation clash. Much post-logical empiricist philosophy of science attacks these problems....Unlike logical empiricism, however, revised empiricism vindicates [its “commitments to empiricism and to the idea that science is a distinctive human enterprise”] by emphasizing the holistic nature of scientific theory and the sociology of scientific practice....

Revised empiricists start from the recognition that logic alone cannot distinguish observation sentences from theoretical ones, nor can it determine what to revise or reject in a clash between hypothesis and observation....In principle, the truth of any sentence can be maintained by making adjustments elsewhere in the

theory....

Revised empiricism retains but transforms the idea of testability. **Logical empiricists viewed testability as a product of straightforward observation** and deductive inference; revised empiricists see testability as a matter of theory-dependent observation, deductive inference,, and a hefty dose of contingent judgment. The revised empiricist preserves empiricism by using the standard of collective judgment to establish the distinction between theory and observation that the logical empiricists took for granted. The revised empiricist also relies on the standard of collective judgment to select which tenets will be maintained despite recalcitrant experimental outcomes, at least for the time being. (Feldman 1995: 9-16, boldface emphasis mine)

4. Critique of Feldman

I have seventeen criticisms of Feldman's views, but I shall first state an overview.

The logical positivists put things in their proper place in science. They treated as theoretical the things that are theoretical, and treated as practical the things that are practical. They and allied thinkers basically followed the scientific holism of Duhem as expounded in 1906. Like Duhem, they were experienced and capable thinkers who knew science well and did not commit elementary blunders. While the key principle of verifiability which united them as a movement ultimately failed to convince (they were aware of its limitations and experimented with several reformulations of it), in my view it can still serve as an excellent methodological principle to regulate the natural and social sciences. These people in the 1930s and earlier were the real

“revised empiricists,” and I think sounder in their judgment than Feldman’s crew concerning what is basic to science theoretically and what is only a practical necessity. Their view of scientific relevance more directly concerns the relationship between theory and observation (and concerns such as rate of error) than peer review or general consensus, which are only of practical interest, not the real meat of science. And all or nearly all of them rejected the view that observation statements (or statements of initial conditions, for that matter) provide an absolutely certain epistemological foundation for science.

My sixteen comments on Feldman follow.

1. *Logical empiricism is the same as logical positivism; the two terms are synonymous; yet Feldman ignores the entire Vienna Circle of logical positivists in her discussion of logical empiricism.*

Feldman keeps referring to “logical empiricism.” Most philosophers call it logical positivism. As Oswald Hanfling says in his book *Logical Positivism*, “The Vienna Circle’s philosophy became known as Logical Positivism or Logical Empiricism” (Hanfling 1981: 6). The Vienna Circle of logical empiricism had eleven leading members (Ayer 1959: 3). Feldman cites none of them. Nor does she cite any of the Circle’s lesser known members such as Gustav Bergmann. Nor does she cite any later followers of or adherents to logical empiricism, such as Ayer. That is to say, in her critique of logical empiricism, she cites no logical empiricists at all.

2. *Popper and Hempel were merely allied to logical empiricism. The only essential thesis defining logical empiricism is the verificationist theory of meaning. Popper and Hempel both expressly reject that thesis, and Popper does so in the very book Feldman cites.*

To support her interpretation, Feldman cites two figures whom she claims to be logical

empiricists (in effect for her purposes), Popper and Hempel, and cites secondary literature written long after the demise of logical empiricism. In my lengthy quotation of Feldman, which cites to her footnotes 45-73 (or 43-74, depending on what footnote 74 is a note to), Feldman refers to pages 3-51 in Hempel's 1965 *Aspects of Scientific Explanation*, and to pages 40-41 and 41-42 in Popper's 1932 *The Logic of Scientific Discovery*. That's a grand total of forty-nine pages in Hempel and three pages in Popper. The Hempel citation is to a single paper Hempel wrote in 1945 along with a 1964 post script. The paper is "Studies in the Logic of Confirmation." As to Popper, Feldman admits in her footnote 47, "The contours of logical empiricism can be described in various ways, including some that would characterize Hempel, but not Popper, as a logical empiricist. For purposes of understanding Daubert, it is useful to focus on the logical empiricist commitment to testability understood in a particular way....Popper shared this commitment, which is why, for present purposes, I count him a logical empiricist" (Feldman 1995: 48). This commits several errors. First, Popper had his own theory of testability which, as he and the logical empiricists well knew, he did not share with them. His test centered on falsifiability, while their test(s) required verifiability *or* falsifiability. Second, even if Popper shared a general commitment to testability with them, that would not make him a logical empiricist. The one essential feature which defines logical positivism is a commitment to some sort of verificationist theory of meaning. Popper emphatically repudiated that sort of theory of meaning in the very book Feldman cites. His test was not a test of scientific meaningfulness, but merely of scientific reliability. Thus he was no logical empiricist, and both he and the logical empiricists were well aware of that.

It is a poor tactic to try to show how the *Daubert* Court might be committed to logical

empiricism by pretending that someone it cites is a logical empiricist when in fact he expressly repudiates logical empiricism. Feldman would have done better to quote the logical empiricist Alfred Jules Ayer: “Popper was not in fact a member of the Circle and would at no time have wished to be classified as a positivist, but the affinities between him and the positivists he criticized appear more striking than the divergencies” (Ayer 1959: 5).

Hempel was not a logical empiricist either. He was not a member of the Vienna Circle of logical empiricists, nor a follower of that school like Ayer, but a member of the closely allied Berlin school of philosophy of science (Ayer 1959: 5). Of course, like Popper, he had many close affinities with logical empiricism. But with respect to the essential thesis of logical empiricism, the thesis that the meaningfulness of a synthetic statement is in some sense dependent on its empirical verifiability, let us turn to Hempel’s major work on the subject, his famous paper, “The Empiricist Criterion of Meaning.” There Hempel criticizes various formulations of the thesis without upholding any of them in the end. And that is merely a way of saying he rejects the thesis, and with it, logical empiricism, though he hopes he may be able to accept the thesis someday, if an adequate formulation of it is ever found (Hempel 1959: 108-29).

3. Feldman accuses some of the greatest philosophers of science who ever lived of an elementary blunder.

Feldman criticizes what she presents as the “logical empiricist model of testability” for committing an elementary blunder. Referring back to the long block quote of Feldman, the blunder is the failure to see that if (5) [observed results] clashes with (3) [expected observed results], you logically need not reject the hypothesis (2), but logically could reject one of the initial conditions, including background assumptions (2). You could reject any or all of (1) and (2). This

is no more than to say that if the conclusion of a valid argument is false, you cannot logically tell which or how many of its premises are false. I think that to attribute such a blunder to some of the best and sharpest philosophers of the twentieth century is incredible. Feldman's explanation is apparently that they were blinded by their insistence that all the initial conditions and background assumptions be "incontestable" (Feldman's term), incorrigible, absolutely certain, conclusively verified. But the very stupidity of the blunder ought to make one think twice about ascribing such a model to the logical empiricists in the first place.

4. The very paper Feldman cites as showing that Hempel commits the blunder shows instead that Hempel expressly rejects the thesis that observation reports are incorrigible.

Feldman's model appears to come from Hempel's paper (Hempel 1965: 40-41). The model is there, exactly where she says it is. However, it is merely a schema which does not explain what observation statements are, or whether they are incorrigible. In fact, elsewhere in the paper, Hempel expressly rejects the view that observation sentences are incorrigible. He says, "We do not propose to enter into a discussion of this question here except for mentioning that various considerations militate in favor of the convention that no observation report is to be accepted definitively and irrevocably" (Hempel 1965: 45). In a footnote appended to this sentence, Hempel cites two leading members of the Vienna Circle, Rudolf Carnap and Otto Neurath, and a member of his own closely allied Berlin school, Hans Reichenbach. Hempel, Carnap, Neurath, and Reichenbach all reject the thesis of incorrigibility of observation reports. Thus all would allow indefinitely many ways to adjust things if new observations conflict with a scientific theory. For if observations are not incorrigible, then nothing in science is incorrigible.

5. The very book Feldman cites as showing that Popper commits the blunder shows

instead that Popper repeatedly rejects the incorrigibility thesis which Feldman accuses him of.

Popper, the only other alleged logical empiricist Feldman cites, also rejects the thesis of the incorrigibility of observation sentences. In fact, Popper repeatedly rejects that thesis in the book Feldman cites. As in Hempel's case, Feldman cites a schematic model whose observation statements Popper explains elsewhere in the book. Popper says, "Now I hold that scientific theories are never fully justifiable or verifiable, but that they are nevertheless testable" (Popper 1959: 44; this is a reprint of the original 1934 edition). Popper says in the very first chapter:

In demanding objectivity for basic statements as well as for other scientific statements, we deprive ourselves of any logical means by which we might have hoped to reduce the truth of scientific statements to our experiences. Moreover we debar ourselves from granting any favoured status to statements which represent experiences, such as those statements which describe our perceptions (and which are sometimes called 'protocol sentences'). They can occur in science only as psychological statements; and this means, as hypotheses of a kind whose standards of inter-subjective testing (considering the present state of psychology) are certainly not very high.

Thus if the basic statements...are to be inter-subjectively testable, *there can be no ultimate statements in science*: there can be no statements in science which cannot be tested, and therefore none which cannot in principle be refuted....

(Popper 1959: 46-47)

Thus Popper's view is that far from being "incontestable" as Feldman claims, observation statements are not even very reliable! Popper continues:

By means of [the falsifying] mode of inference we falsify *the whole system* (the theory as well as the initial conditions) which was required for the deduction of the statement *p*, *i.e.* of the falsifying statement. Thus it cannot be asserted of any one statement of the system that it is, or is not, specifically upset by the falsification (Popper 1959: 76; see 77).

Thus, far from committing the blunder which Feldman accuses the logical empiricists of committing, Popper clearly avoids it in the very book Feldman cites (and "for present purposes, I count him a logical empiricist," Feldman 1995: 48). Does this make Popper a "revised empiricist"? If so, most of the logical empiricists themselves were already "revised empiricists" by the 1930s, much as Popper was in 1934. Some of them flirted with conclusive verification, but they soon abandoned it (see Ayer 1959: 14).

It would be tedious to quote all the statements in Popper's 1934 book which refute Feldman's interpretation of it (Popper 1959: 96-101, 104-5, 108-11, 278-81).

Popper's main discussion is in a section of the book on what he calls *the problem of empirical basis*. In that section he argues in detail for what we have already seen to be his view: that basic statements are as corrigible and revisable as any other objective scientific statements. Feldman overlooks this entire section of Popper's book. Popper concludes later in the book by saying:

The old scientific ideal of *episteme*--of absolutely certain, demonstrable knowledge--has proved to be an idol. The demand for scientific objectivity makes it inevitable that every scientific statement must remain *tentative for ever*. (Popper 1959: 280).

One might ask, “But what help for Feldman’s view is provided by the pages that she *does* cite, and how do they reconcile with the passages I cite?” The answer to the first question is, no help whatsoever, since the pages she does cite are not even *about* the question of absolutely certain foundations. Her pages are completely irrelevant to resolving the question. They merely sketch the logical structure of scientific investigation as Popper conceives it. They do not *explain* what observation statements *are*. They tell us nothing of the epistemological features of observation statements, such as whether they are absolutely certain. Whether observation statements are absolutely certain is what is in question. The theory that observation statements are absolutely certain is expressly discussed and rejected by Popper in the passages *I* cite, which are the only passages in the whole book which are relevant to the question whether observation statements are absolutely certain foundations of science for Popper.” The answer to the second question is, they reconcile perfectly, since her passage states an unexplained formal *schema* to the observation statements within which my passages provide a consistent, coherent and perfectly adequate *interpretation*. Again, Popper speaks of not of observation statements but of “*basic* statements,” and calls the question whether there are any absolutely certain foundations of science “the problem of empirical *basis*.” Feldman does not even use Popper’s terminology for discussing

Popper on the problem of empirical basis.

6. *Daubert's conception of science is far closer to Popper's than Feldman realizes.*

Perhaps the *Daubert* Court or its briefers read Popper more carefully than Feldman did, since they could have easily taken the factor of publication-and-peer-review out of Popper's 1934 book, right along with the factor of reproducibility-testability. As to the communal side of "revised empiricism," Popper says in the very first chapter:

Every experimental physicist knows those surprising and inexplicable apparent 'effects' which can perhaps even be reproduced in his laboratory for some time, but which finally disappear without trace. Of course, no physicist would say in such a case that he had made a scientific discovery (though he might try to rearrange his experiments so as to make the effect reproducible). Indeed the scientifically significant *physical effect* may be defined as that which can be regularly reproduced by anyone who carries out the appropriate experiment in the way prescribed. No serious physicist would offer for publication, as a scientific discovery, any such 'occult effect', as I propose to call it--one for whose reproduction he could give no instructions. The 'discovery' would be only too soon rejected as chimerical, simply because attempts to test it would lead to negative results. (Popper 1959: 45-46)

The logical empiricists were surely very sympathetic to all this. Ayer reports:

The missionary spirit of the Circle found a further outlet in its publications. In 1930 it took over a journal..., renamed it *Erkenntnis* and made it...the principal organ of the positivist movement. In the following years there appeared a series of monographs with the collective title of *Einheitwissenschaft* --Unified Science--and a series of books under the general editorship of Schlick and Philipp Frank, with the collective title of *Wissenschaftliche Weltauffassung* [*Scientific World-View*]....Among the...volumes to appear in it [was] Karl Popper's famous *Logik der Forschung* [*Logic of Scientific Discovery*]. (Ayer 1959: 6)

Thus leading members of the Vienna Circle of logical empiricists peer-reviewed Popper's book with its basic insistence on replication-testability, its rejection of incontestable observation statements, its consequent scientific holism of theories any part of which can be revised to accommodate recalcitrant experiences, its practical requirements of peer review and of communal testing of scientific claims, and found it worthy of publication in their own series of books, even though Popper himself was no logical empiricist.

For at least sixty years, Popper's later books have repeated his 1934 views on "empirical basis" almost word for word, insisting on "the conjectural and theoretical character of all observations, and all observation statements" (Popper 1971: 30; see Popper 1994: 86; Popper 1962: 385-88). For a reliable account of Popper, correctly describing him as rejecting epistemic certainty as not foundational to science's procedure of testing hypotheses, see Mario Bunge (1964: 36-39).

Even if we merely count Hempel and Popper as logical empiricists for the purpose of

Feldman's discussion, due to their being allied to logical empiricism, they still say the opposite of what Feldman says they say.

7. What Feldman calls "revised empiricism" was basically advocated by the scientist and philosopher of science Pierre Duhem in 1906. Duhem was a major influence on logical empiricist philosophy of science in the 1920s and 1930s.

Having discussed the two so-called logical empiricists Feldman cites, I turn now to my own discussion of logical empiricism. I shall begin with philosophers of science who wrote before the movement of logical empiricism started and influenced that movement. I shall concentrate on Pierre Duhem, Ludwig Wittgenstein, and the early Moritz Schlick.

In 1906 Duhem's landmark book *The Aim and Structure of Physical Theory* was published. The book is famous for arguing that when you have a theory faced by a recalcitrant observation, the theory can be adjusted in many different ways to compensate. "According to Duhem, there are no genuine crucial experiments because it is the ensemble of a theory forming an indivisible whole which has to be compared with experiment" (Duhem 1974: xi, Foreword by Prince Louis de Broglie). One need read only chapters four through seven to see that what Feldman calls "revised empiricism" began in 1906 (omitting the Pythagorean number mysticism). Duhem was singled out in the Vienna Circle's 1929 official manifesto of logical positivism as one of the Circle's "main precursors [in] philosophy of science" (Ayer 1959: 4). The manifesto is entitled "Wissenschaftliche Weltauffassung--Der Wiener Kreis" ("Scientific World-View--The Vienna Circle"). Rudolf Carnap, Otto Neurath, and Hans Hahn wrote it.

Popper cites Duhem five times in the book which Feldman cites. Hempel cites Duhem seven times in the book which Feldman cites. While the citations are peripheral to our concerns,

they show that Popper and Hempel were well acquainted with Duhem.

Here I shall do no more than quote main headings from Duhem's table of contents:

PART II

THE STRUCTURE OF PHYSICAL THEORY

Chapter IV. Experiment in Physics.

1. An experiment in physics is not simply the observation of a phenomenon; it is, besides, the theoretical interpretation of this phenomenon. 2. The result of an experiment in physics is an abstract and symbolic judgment. 3. The theoretical interpretation of phenomena alone makes possible the use of instruments....

Chapter V. Physical Law

1. The laws of physics are symbolic relations. 2. A law of physics is, properly speaking, never true or false but approximate. 3. Every law of physics is provisional and relative because it is approximate. 4. Every physical law is provisional because it is symbolic....

Chapter VI. Physical Theory and Experiment

....2. An experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group. 3. A "crucial experiment" is impossible in physics.... 10.

Good sense is the judge of [which hypotheses] ought to be abandoned.

Chapter VII. The Choice of Hypotheses

....2. Hypotheses are not the product of sudden creation, but the result of progressive evolution. (Duhem 1974: xx-xxi)

In the main text, Duhem argues that no scientific statement can be purely observational, then argues further that because of that, no statement is immune from revision in the light of ongoing science.

Duhem is basic to twentieth century history of philosophy of science. Duhem was revered by logical empiricists precisely for being the first to detect and reject Feldman's elementary blunder. Yet Feldman ignores Duhem completely.

8. Feldman ignores Ludwig Wittgenstein, who wrote the first "bible" of the logical empiricists, which was their single greatest inspiration in developing logical empiricism in the first place.

The first Bible of logical positivism was Ludwig Wittgenstein's *Tractatus Logico-Philosophicus*. This book had a tremendous impact on the Vienna Circle, which discussed it in great detail twice. The book may not be empiricist at all, since Wittgenstein never says what his objects are. But it is well known that precisely in order to avoid problems like Feldman's elementary blunder, specifically the problem strong verificationism would raise for science, Wittgenstein makes science a web of "directives" which are neither true nor false (T 6.34-6.361). Wittgenstein was not an official member of the Vienna Circle, but kept in close touch with Schlick and Friedrich Waismann (Ayer 1959: 5). His book, one of the seminal works of the twentieth century, was first published in 1921, some thirty years before Feldman thinks revised empiricism began. Yet Feldman ignores Wittgenstein completely.

9. Feldman ignores Moritz Schlick, the founder of the logical empiricist movement.

Moritz Schlick was the leader of the Vienna Circle. He preferred to use the term "logical empiricism" to describe the Circle's philosophy (Hanfling 1981: 6). He received his doctorate in

physics, not philosophy. He did his dissertation under the great physicist Max Planck. Like the rest of the Vienna Circle, Schlick was deeply influenced by both Duhem and Wittgenstein.

In his major early book, *General Theory of Knowledge*, written before he founded the movement of logical empiricism (the first edition was in 1918 and the second in 1925; he began the logical empiricist movement around 1929), Schlick points out and ridicules the elementary blunder Feldman accuses the logical positivists of committing (Schlick 1985: 109-11, criticizing Sigwart and Kant). He denies that there is any feature such as self-evidence in experience. He admits empirical acquaintance but finds it too primitive to be of interest to science. The moment it is brought under a concept and becomes of interest to science, it goes beyond immediate experience and is merely hypothetical. Indeed, even without being brought under a concept, any empirical acquaintance becomes a mere hypothesis the moment it slides from present to past. Science is an interconnected network of hypotheses (Schlick 1985: xiii, xv, xvi, 67-71, 73, 110, 148, 162, 389). “The factual sciences, as a system, constitute a network of judgments the individual meshes of which are coordinated to individual facts” (Schlick 1985: 69). Schlick says:

All knowledge of reality consists, strictly speaking, of hypotheses....No scientific truth is in principle secure against the danger that at some time it may be refuted and thus become invalid. [No truths about the real world] can be completely stripped of their hypothetical character. (Schlick 1985: 389).

In 1932, in the heyday of logical empiricism (Schlick 1985: xx-xxi), Schlick remained faithful to his earlier views. In “Positivism and Realism,” Schlick says:

It is perfectly true that every statement about a physical object or an event *means* more than is verified, say, by the occurrence of a single experience. It is rather presupposed that the experience occurred under very definite conditions....In this manner one can and must give an account of illusions of sense, and of error, and it is easy to see how those cases are to be included in which we should say the observer was merely dreaming, that the pointer indicated a definite line, or that he did not carefully observe, etc. The assertions of Blondlot about N-Rays which he believed himself to have discovered were certainly *more* than statements that under certain conditions he had experienced certain visual sensations; and because of this, of course, they could be refuted. Strictly speaking, the meaning of a proposition about physical objects would be exhausted only by an indefinitely large number of possible verifications, and we gather from this that such a proposition can in the last analysis never be shown to be absolutely true. It is indeed generally recognized that even the most certain propositions of science are always to be taken as hypotheses, which remain open to further refinement and improvement.

Once again: the meaning of a physical statement is never determined by a single isolated verification....Hence if any positivist ever said that the only objects of science are the given experiences themselves he was certainly quite mistaken.... (Schlick 1959a: 91-92).

Thus like Hempel, Popper, and Duhem, the leader of the Vienna Circle would agree with the

“revised empiricists” that science is a web, and that “there is no such thing as a once-and-for-all crucial experiment in which a hypothesis is demonstrated to be false” (Feldman 1995: 13, quoting Patricia Churchland in 1986).

Schlick’s 1934 article, “The Foundation of Knowledge,” is I think the deepest and most paradoxical work written on this subject. Concerning the characterization of protocol or observation statements as directly and purely describing facts, Schlick says:

That is to say: when protocol statements are conceived in this manner, then directly one raises the question of the certainty with which one may assert their truth, one must grant that they are exposed to all possible doubts.

There appears in a book a sentence which says, for example, that N. N. used such and such an instrument to make such and such an observation. One may under certain circumstances have the greatest confidence in this sentence. Nevertheless, it and the observation it records, can never be considered *absolutely* certain. For the possibilities of error are innumerable. N. N. can inadvertently or intentionally have described something that does not accurately represent the observed fact; in writing it down or printing it, an error may have crept in. Indeed the assumption that the symbols of a book retain their form even for an instant and do not “of themselves” change into new sentences is an empirical hypothesis, which as such can never be strictly verified. For every verification would rest on assumptions of the same sort and on the presupposition that our memory does not deceive us at least during a brief interval, and so on.

This means, of course--and some of our authors have pointed this out almost with a note of triumph--that protocol statements, so conceived, have exactly the same character as all the other statements of science: they are hypotheses, nothing but hypotheses. They are anything but incontrovertible, and one can use them in the construction of science only so long as they are supposed by, or at least not contradicted by, other hypotheses. We therefore always reserve the right to make protocol statements subject to correction, and such corrections, quite often indeed, do occur when we eliminate certain protocol statements and declare that they must have been the result of some error.

Even in the case of statements which we ourselves have put forward, we do not in principle exclude the possibility of error. We grant that our mind at the moment the judgment was made may have been wholly confused, and that an experience which we now say we had two minutes ago may upon later examination be found to have been an hallucination, or even one that never took place at all.

Thus it is clear that on this view of protocol statements they do not provide one who is in search of a firm basis of knowledge with anything of the sort. On the contrary, the actual result is that one ends by abandoning the original distinction between protocol and other statements as meaningless. Thus we come to understand how people come to think³ that any statements of science can be selected at will and called "protocol statements," and that it is simply a question of convenience which are chosen.

3. K. Popper as quoted by Carnap, *op. cit.*, *Erkenntnis*, Vol. III, p. 223. (Schlick

1959: 212-13)

Schlick calls this the search for an absolutely certain basis of scientific knowledge “the problem of basis” and searches for “basic statements” from which science can begin as either logically or temporally first statements, and upon which science can rest as foundational statements. In the end he finds no such thing.

Schlick then recharacterizes observation statements in terms of their origin in one’s own private perceptions or sensations, as opposed to characterizing them as pure and direct descriptions of facts. In that way he is able to admit infallible observation statements after all. But he denies that they can be used in scientific publications because they are too fleeting. My observation statements are absolutely certain only in the here and now, and only to me. They do play a private confirmatory role at the moment of my observation of an experimental result. In that sense they come not at the beginning of science as its foundation, but at the end as an asymptote or limit, i.e., only as my private and personal immediate confirmation or disconfirmation of a predicted result. And then their absolute certainty vanishes into the past forever. Thus they can never become part of science, since science is an ongoing and public enterprise. Their momentary absolute certainty cannot be communicated to another scientist even in the here and now. They cannot even be written down or spoken without losing their absolute certainty. Thus they cannot be published or reviewed by peers. That is just why Schlick’s view is so paradoxical: How can there be statements which cannot be spoken or written down, or even said to oneself in memory? Schlick prefers rather to call them confirmations rather than statements at all (Schlick 1959; compare Hanfling 1981: 89-95)

I speculate it is possible that Schlick might allow observation statements to be spoken to oneself in the present moment. But that would only land us in another paradox, that of the duration of the present moment. A statement logically must say something about something; it logically must have a logical subject and a logical predicate. If these are expressed or thought in linear fashion, then the present moment cannot be a mere point in time without duration. But if the present moment has any duration at all, then the logical subject is already fading into the past when you come to the logical predicate (or vice versa, as in “Green is grass”).

In any case, except for my speculation, that is Schlick’s considered view, given at the end of the article. Note Schlick’s discussion suggests that numerous people had already come to reject the infallibility of observation or protocol statements before 1934. Besides citing Popper as quoted by Carnap in *Erkenntnis* 3 (1932-33), Schlick also cites Neurath’s “Protocol Sentences” from the same volume of *Erkenntnis* (Neurath 1959: 199-208).

10. *Feldman ignores Rudolf Carnap and Otto Neurath, the two greatest logical empiricists after Schlick.*

Neurath, a famous member of the Vienna Circle, was an outspoken holist in philosophy of science and the principal advocate of the Vienna Circle’s “unified science” project. See Neurath’s 1931-32 paper, “Sociology and Physicalism” (Neurath 1959a: 282-317). Neurath was a radical materialist, and he was the chief influence on Carnap’s turn to methodological physicalism. Neurath’s protocol statements are corrigible, and Carnap follows Neurath on this point (Ayer 1959: 20). Feldman never mentions Neurath or Carnap.

11. *Feldman ignores Alfred Jules Ayer, who wrote the second “bible” of logical empiricism summarizing the achievements and outlook of the Vienna Circle, which book did*

more to spread the ideas of logical empiricism across the planet than any other work.

Ayer was not a member of the Vienna Circle, but he attended its meetings in 1933, and he published the most famous treatise of the whole logical positivist movement, *Language, Truth, and Logic*. It has been called “the Bible of logical positivism.” In this book, Ayer rejects conclusive verification in favor of weak verificationism. Ayer says, “Indeed, it will be our contention that no proposition, other than a tautology, can possibly be anything more than a probable hypothesis (Ayer 1952: 38). Ayer adds:

It appears, then, that the “facts of experience” can never compel us to abandon a hypothesis. A man can always sustain his convictions in the face of apparently hostile evidence if he is prepared to make the necessary *ad hoc* assumptions. (Ayer 1952: 95)

This is another famous book Feldman ignores. The book was published in 1936, and the second edition came out in 1946.

12. *Feldman ignores Quine, [who was] arguably the world’s greatest living philosopher.*

Quine, [who was] America’s most famous living philosopher, is a verificationist. That makes him a logical empiricist. He is also a revised empiricist. His famous thesis of the underdetermination of science by experience is what holism in science is all about.

Quine repeatedly attributes the key insight of holism to Pierre Duhem. Quine acknowledges Duhem as the progenitor of Quine’s thesis that no scientific statement is immune from revision, not even a so-called observation statement, and of Quine’s theory of science as

being an interconnected web.

Quine's most famous teacher was Rudolf Carnap. Quine travelled to Europe in the 1930s to study under Carnap and others. Quine adopted Carnap's weak verificationism, and expanded Carnap's empirical holism, which included all natural science, so as to include all statements whatsoever, even statements in what are normally regarded as pure logic and pure mathematics. Quine's seminal book, *Word and Object*, which is dedicated to Rudolf Carnap, begins with an epigram from Otto Neurath:

Wie Schiffe sind wir, die ihr Schiff auf offener See umbauen müssen, ohne es
jemals in einem Dock zerlegen und aus Bestandteilen neu errichten zu können.

(Quine 1975: first epigram)

If I may casually translate that without a dictionary, Neurath's metaphor for science is that we are all on a ship at sea, need to make repairs, but cannot find a firm dock to rest on, so we must rebuild the ship using its own planks while we are sailing along. The ship is the scientific enterprise, the battering of the sea is recalcitrant experience, the dock we cannot use is incontestable experience, and the planks of the ship are the hypotheses and initial conditions and background conditions.

One of Quine's most famous slogans is "no immunity from revision," meaning that no statement is immune from revision, not even mathematical theorems or observation sentences. Like so many others, Quine emphasizes the social and communal requirement of intersubjective checkability. Feldman never mentions Quine either. Quine's studies under Carnap and others in

Europe in the 1930s wreck her 1950s-1960s revisionist timetable as much as everyone else I have discussed does.

Quine has been the world's most famous logical empiricist and empirical holist for half a century. Yet Feldman ignores him completely.

13. Feldman's collectivist rescue of science from the supposed elementary blunder of the logical empiricists leaves much to be desired.

Feldman insists that the falsifiability thesis must be saved in "some principled way" (Feldman 1995: 13), and that this way is "the standard of collective judgment" as revealed to us by the revised empiricists (Feldman 1995: 16). I see nothing principled about that. Calling something principled does not make it so. I do not even see that a collection of scientists need be any better or worse than a single scientist in rationally deciding the best way to revise a theory falsified by a recalcitrant experience. Collective judgment is a practical factor. It is circumstantial evidence at best, unless you are putting scientific matters to a vote, as if science were a democracy or a matter of subjective confidence.

When philosophers are faced by a puzzle they cannot solve, they often pass the buck. In first philosophy, the buck is often passed to God or to the mind. How does the body coordinate with the mind? "God does it." How do we abstract our awareness of properties from individual things? "The mind does it." In ethics or political philosophy, the pseudo-solution is often the group or community. Feldman's collectivist solution is a community black box on which she has written, "In here it is decided how to revise scientific theories." By why mysterious alchemy this takes place, she does not say. You could say, "Many hands make light work," but you could also say, "Too many cooks spoil the broth." But nothing will be solved here by appealing to proverbs

or anecdotes. Is deciding things by popular vote in the relevant scientific community scientifically reliable?

The four *Daubert* factors are not the mechanism of the black box. The black box is what scientists use, not lawyers.

Feldman is committing a sort of inverse fallacy of composition. Just because it is so hard to explain how an individual can tell how best to revise a theory, it does not follow that everything suddenly becomes reliable when a community of individuals revises a theory.

In my opinion, the community is theoretically no better off than the individual in telling how best to *revise* a theory. The value of the community is pragmatic. This must not be confused with the value of the community in *replication and verification*, a very different logically and temporally prior step of scientific method. We have no reason to revise a theory unless it has already been falsified.

14. *We must not confuse the logical empiricists' tendency in metaphysics to solipsistic "logical constructions" of "other minds" with their common-sensical acceptance of the need for an ordinary scientific community to do science effectively.*

The logical empiricists were already using collective judgment as a practical standard, so far as I can see. They would have to be idiots not to accept that standard as crucial to the actual practice of science.

One must not confuse assaying scientific activities as bundles of sensory impressions with denying the publicity of scientific activities. *All* public activities will be constructed as bundles of sensory impressions across *different* persons, individual persons being the only ordinary things constructed as *private* bundles of sensory impressions. Thinking otherwise is to confuse neutral

monism with solipsism. And it would be foolish indeed to think that the logical empiricists denied that science is a public enterprise in the *ordinary* sense of the word “public.”

I would hold this view based on pure charity in interpretation alone. But charity is scarcely the word. The tremendous importance of science’s being a community enterprise has been known for centuries. The kings of England knew well by the time of Newton that they had to support a big community of scientists--including a Royal Society, if you please--if English science was to get anywhere. I myself would date publication and peer review as known throughout western Europe to be practically essential (not: theoretically essential) to science by the time of Descartes. It would be strange if highly cultivated philosophers of science in Vienna and Berlin in the early twentieth century somehow let the whole modern European scientific experience slip through their fingers.

Not every logical empiricist, by the way, constructed other minds from his own sense-experiences. Neurath and the later Carnap, who is still early enough (1930s) to foil Feldman’s timetable, constructed all minds, even their own, out of public physicalist items.

15. The essential tenets of logical empiricism and “revised empiricism” are logically compatible.

The verificationist theory of meaning is logically consistent with “revised” empiricism’s rejection of absolutely certain, epistemically foundationalist observation statements, and also with an emphasis on the need for a whole scientific community to be involved in order for scientific work to be reliable. In fact, as we saw, the logical empiricists were already what Feldman calls “revised” empiricists.

If the number mysticism of revised empiricism is incompatible with logical empiricism, so

much the worse for the number mysticism. I am referring to Feldman's idea that peer review and widespread acceptance are equal in significance to replication and verifiability, not to mention explanatory and predictive power, in the theoretical definition of reliable science.

If philosophers of science today are elevating peer review into a factor of importance equal to that of scientific method, i.e. to the requirement that experiments be performed, that would be a big change from historical of logical empiricism. But it is not clear to me that very many of them would do so. Feldman cites some *Daubert* briefs as her chief evidence that "Revised empiricism holds sway among contemporary scientists..." (Feldman 1995: 48 n.20). I have read some *Daubert* briefs too, and they seem to me to be evidence of nothing of the kind. The four briefs I read highly value peer review, but make scientific method the key. In fact, the only reason peer review is important is precisely to check on scientific method. Scientific method is the sun, peer review the moon which borrows its light from the sun. The briefs I read had that right. The *Daubert* opinion may have given some readers different impression when it merely listed both on a "factor list" without further ado. But it never said that the four factors it lists are of equal importance. Indeed, looking at the four corners of the opinion, *Daubert* seems to follow the briefs I read in making scientific method the key to reliable science.

16. *Mainstream logical empiricism, i.e., weak verificationism, logically implies holist empiricism.*

No discussion of the logical empiricists on empirical holism versus observation statement incorrigibility would be complete without mentioning that the elementary blunder Feldman accuses the logical empiricists of committing directly contradicts the defining principle of mainstream logical empiricism. The weak verification principle which came to prevail is that a

statement has cognitive (empirical) meaning if and only if there is logically possible empirical *evidence* for or against it. It directly follows that an observation sentence is cognitively meaningless unless empirical evidence logically can count against its truth. And that is just to say that no observation statement is incorrigible. To sum up this point, it is an immediate corollary, not to say restatement, of the weak verification principle which defines mainstream logical empiricism, that all cognitively meaningful statements, including all observation statements, are corrigible. This is to say that weak verificationism logically implies empirical holism for synthetic statements.

Compare Popper, for whom any statement, hence any observation statement, must be falsifiable, hence corrigible, in order to count as a scientific statement. Seeing these things calls not for research, but for thinking about what the logical empiricists and Popper are saying. Possibly Feldman is mistaking strong verificationism for mainstream verificationism. If so, she is attacking a view most logical empiricists came to reject. There was a brief initial phase of strong verificationism within the logical empiricist movement. The strong verificationist principle is that a statement is cognitively meaningful if and only if it is *conclusively verifiable* by sense-experience. The logical empiricists quickly rejected it precisely because they quickly saw that no statement about the past or future could be conclusively verified on the basis of present-time observation statements, and that therefore the strong verification principle condemned all statements about the past or future, even “Rocks existed before humans existed,” as cognitively meaningless. That is, the logical empiricists quickly perceived and avoided the elementary blunder Feldman accuses them of, and adopted weak verificationism with its holist implications. See Hempel (1959: 111-16) and Ayer (1952: 9-11, 36-37).

I do not wish to be simplistic in my own portrayal of the logical empiricists. I therefore note four qualifying points.

First, I have not done a survey of how many logical empiricists initially accepted strong verificationism, or of how many came to reject it in favor of weak verificationism.

Second, among ostensible weak verificationists, the Ayer of the second edition of *Language, Truth and Logic* is technically a strong verificationist. Ayer admits that observation statements *can* be conclusively verified *in the present moment they describe*. But as soon as the moment has passed into the past, they can no longer be conclusively verified (Ayer 1952: 10). This narrow exception does not significantly change the holistic nature of his views.

Ayer defines strong verificationism and weak verificationism as follows:

[I]t will be seen that I distinguish between a “strong” and a “weak” sense of the term “verifiable,” and that I explain this distinction by saying that “a proposition is said to be verifiable in the strong sense of the term, if and only if its truth [logically] could be conclusively established in experience,” but that “it is verifiable, in the weak sense, if it is [logically] possible for experience to render it probable.” And I then give reasons for deciding that it is only the weak sense of the term that is required by my principle of verification. What I seem, however, to have overlooked is that, as I represent them, these are not two genuine alternatives. For **I subsequently go on to argue that all empirical propositions are hypotheses which are continually subject to the test of further experience;** and from this it would follow not merely that the truth of any such proposition

never was conclusively established but that it never could be; for however strong the evidence in its favour, there would never be a point at which it was impossible for further experience to go against it. But this would mean that **my “strong” sense of the term “verification” had no possible application**, and in that case there was no need for me to qualify the other sense of “verifiable” as weak; for on my own showing it was the only sense in which any proposition could be conceivably verified.

I have come to think that there is a class of empirical propositions of which it is permissible to say that they can be verified conclusively, It is characteristic of these propositions, which I have elsewhere called “basic propositions,” that they refer solely to the content of a single experience, and what may be said to verify them conclusively is the occurrence of the experience to which they uniquely refer. Furthermore, I should now agree with those who say that propositions of this kind are “incorrigible,” assuming that what is meant by their being “incorrigible” is that it is impossible to be mistaken about them except in a verbal sense.... **It is, however,...a case of “nothing venture, nothing win,” since the mere recording of one’s present experience does not serve to convey any information either to any other person or indeed to oneself....**I seem not to have perceived that what I was really doing was to suggest a motive for refusing to apply the term “proposition” to statements that “directly recorded an immediate experience”; and this is a terminological point which is not of any great importance [because]....

Whether or not one chooses to include basic statements in the class of

empirical propositions, and so to admit that some empirical propositions can be conclusively verified, **it will remain true that the vast majority of the propositions that people actually express are neither themselves basic statements nor deducible from any finite set of basic statements.**" (Ayer 1952: 9-11, boldface emphasis mine)

Thus basic statements play no role in science for Ayer, since for Ayer science is public and communicable, and conveys information. And even if, per impossibile, purely private statements could play a role in science, their role would be ephemeral, not merely because they convey no information, but also because my basic statements cease even to be basic statements the moment after I have the experience they immediately describe.

Note that Ayer defines weak verificationism in terms of possible "probable" confirmation, not in terms of possible evidence, however slight. Does he mean possible preponderance of the evidence, i.e. possible evidence making the statement (or its denial) more probable than not? Or does he mean "probable" in the sense of probable cause, meaning a reasonable likelihood of truth (or falsehood) less than a preponderance but more than a scintilla of evidence? Or does he merely mean any possible evidence at all, even the slightest possible empirical evidence? Only the third alternative makes any sense to me as defining, grounding, or finding the test or criterion of the cognitive meaning of a statement in the possibility of empirical evidence for or against it, since we would wish to make a clean sweep of all cognitive meaning, however minimal. But Ayer's language is unclear.

The distinction between actual verification and possible verification (verifiability,

confirmability) and the distinction between strong verification and weak verification yields a mix-and-match matrix of four options: actual strong verification, possible strong verification, actual weak verification, and possible weak verification. The matrix would be even larger if we allow three sub-kinds of weak verification: more probable than not, probable, and evidence however slight. But I will consider weak verification as consisting of only the third sub-kind here. Of the four options, then, actual weak verification does not seem to have been actually used by the logical positivists. Perhaps that is because the progression of their thought was from actual strong verification to possible strong verification to possible weak verification, so as to allow science the maximal cognitive meaningfulness. In this progression, actual weak verification would have been an absurd retrograde step insofar as it returned to requiring actual verification. That battle had already been fought, on behalf of the cognitive meaningfulness of statements about the future or the past, as well as any other situations we are not now in a position to have evidence about (the stock example was that of a rock on the far side of the Moon).

Now, how does Feldman's critique of logical empiricism apply to the three logico-historical stages of this progression in the thought of the logical positivists? I think it applies to the initial and most widely abandoned stage, actual strong confirmation. Feldman's critique arguably applies also to the second stage, possible strong confirmation, but only insofar as statements are actually strongly confirmed or strongly disconfirmed, since once they are actually strongly confirmed or disconfirmed, our judgment of their truth-values cannot change. But it does not apply to the second stage insofar as that scarcely any statements will ever be strongly confirmed or strongly disconfirmed. In fact, substantially avoiding Feldman's elementary blunder is obviously the entire point of the second stage. And the third stage, possible evidence (or, more

strongly, possible probability, or even possible preponderance of the evidence), obviously allows for the logical possibility of the complete revisability of any cognitively meaningful statement. As explained earlier, Schlick's position is similar to Ayer's. Schlick admits observation statements (not: protocol statements) as "immune from all doubt," but "maintained, as did Neurath, that such statements could not be fitted into the system of science like other statements" (Hanfling 1981: 90). Schlick "admitted...that they could not be regarded as 'foundations' of knowledge, for 'as soon as I put down the demonstrative terms "here" and "now", they lose their meaning'" (Hanfling 1981: 90). They are "crucial" as "'fleeting' confirmations" of all other statements, and as such "come 'at the end' and not the beginning of knowledge" (Hanfling 1981: 93). Hanfling quotes Schlick as saying:

Upon affirmations no logically tenable structure can be erected, for they are already gone at the moment building begins. If they stand in time at the outset of the process of knowledge, they are logically of no use. It is quite otherwise, however, when they come at the end; they complete the act of verification (or falsification), and at the moment of their appearance, have already performed their duty. (Hanfling 1981: 93-94)

The position is subtle. Observation statements cannot be in the system of science at all. The question of keeping or rejecting them cannot arise, since after the moment of the experience they describe, they lose all their meaning forever. They are meaningless as soon as they are written, and therefore there is nothing in them to accept or reject. Hanfling is right to find the notion of

such statements “puzzling” and to criticize Schlick by observing, “That which ‘completes the act of verification’ is not a statement but an observation” (Hanfling 1981: 94). I would add that we never or almost never in fact assert or verbalize such statements to ourselves, and that the only person to whom the statement could have any meaning is oneself. But this is Schlick criticism, not Schlick scholarship.

Third, the strong-weak verification distinction itself has some difficulties, and is not easy to formulate (Hanfling 1981: 37-44).

Fourth, it has been argued that weak verifiability presupposes strong verifiability. The argument is that without strong verifiability, one would not understand what one is weakly verifying. I cannot find the cite, but I think Julius R. Weinberg made the argument somewhere in his (1936). I have two comments. First, if the presupposition is valid, it is criticism, not scholarship. For it is quite clear that weak verificationists such as Neurath and Carnap denied that any statements about the world were completely verifiable. Second, the presupposition is invalid. For a weak verificationist would hold that an observation statement (or for that matter, any statement) is cognitively meaningful just in case it is weakly verifiable. Thus one would understand what one is weakly verifying after all. That is precisely what defines weak verificationism. It is Weinberg who is presupposing strong verificationism, if the argument is his.

17. Feldman’s attack on logical empiricism is a red herring. Daubert does not cite Popper and Hempel on what Feldman calls the elementary blunder, but on another topic.

Even if Feldman were right about logical empiricism--that every logical empiricist and allied thinker, including Popper and Hempel, committed her elementary blunder, which was not corrected until the revised empiricists came to the rescue in the 1950s and 1960s--it would be

beside the point. For the *Daubert* opinion cites Popper and Hempel as authorities only regarding one of its four factors, the factor of replication and verifiability (falsifiability in Popper's case).

Most legal articles and opinions attempt to cite the best authorities on point, whether or not those authorities are any good for other points one may wish to make. Thus, even if Popper and Hempel had been as bad on revising initial observations and assumptions as Feldman thinks, it would still be best to cite them on replication and verifiability, if they remain the authorities on point today.

The *Daubert* opinion is merely attempting to cite the best authorities on point for each point.

But the chief reason Feldman's attack is a wild goose chase is that logical empiricism is not a movement which *Daubert* cites in the first place. The *Daubert* Court did not confuse logical empiricism with revised empiricism. The *Daubert* Court does not even mention logical empiricism. Why is Feldman talking about it at all?

Feldman could have easily deleted all her talk of logical empiricism and "revised empiricism" and just talked about Hempel and Popper (and Green) as individual philosophers, and looked more thoroughly at what they have to contribute to *Daubert*. It is easy to talk about the themes of experiment replication, falsifiability, observation statements, publication, and peer review as thematic topics without making them out to be part of some imagined great shift from one historical school of empiricism to another. The four *Daubert* briefs I read had no problem in doing exactly that. If she had discussed Popper more thoroughly, we might have learned whether *Daubert* did or ought to have endorsed verifiability specifically in the form of Popperian falsifiability, both from the philosophical point of view (is Popper right?) or from the legal point of view (is the Rehnquist minority opinion right in warning that judges will not understand Popper?). That would have been a contribution. Here "verifiability" concerns not verificationist theory of

meaning, but verifying the truth or falsehood of a prediction, i.e. testing the reliability of a theory.

Alternatively, Feldman could research the history more thoroughly. She could then speak of “holistic empiricism” as a movement that exists fairly continuously from 1906 to the present day, from Duhem, through logical empiricism, through Popper and Hempel, and then to today’s figures such as Quine. That would lend historical depth, not to mention accuracy, to her paper. Yes, there have been big changes in philosophy of science. But holistic empiricism has been an underlying basic continuity since 1906, for the most part.

I have not discussed the Vienna Circle logical empiricists Feigl, Waismann, Zilsel, Kraft, Frank, Menger, Gödel, Hahn, or Bergmann. Nor have I discussed allied philosophers such as Reichenbach, Mises, Nagel, Grelling, Kaila, Naess, Petzäll, Joergensen, Scholtz, Mannery, Morris, or Ramsey, not to mention many others. But then neither has Feldman.

5. Friedman’s views

Friedman’s paper, “The Death and Transfiguration of *Frye*,” makes many technical points well but avoids deciding any major questions such as whether *Daubert* is better than *Frye*. Friedman accepts the *Daubert* Court’s opinion that “the *Frye* rule is incompatible with the Federal rules” (Friedman: 1994: 133). He deems *Frye* an “austere standard” that goes against the liberal character of the rules (Friedman: 1994: 134). That is broadly accurate as a general statement, and literally true as a statement of policy. But FRE is not liberal about admitting absolutely everything. Some things are definitely excluded. Thus I think we need a far more specific argument about FRE 702 in particular--an argument Friedman does not give us.

Friedman complains about *Frye* that its “general acceptance [test] sounds more like

judicial notice than acceptability. In trials we expect evidence to be conflicting” (Friedman: 1994: 133-34). The premise is true, but in my view the conclusion does not follow. In my view, something like judicial notice is far more appropriate for judges to engage in than multi-factor tests. For in my view, multi-factor tests sound more like thin disguises for factual findings best left to the trier of fact, the jury. (This is not an original criticism of multi-factor judicial tests.) Even worse than stepping into the shoes of the jury, the judge is stepping into the shoes of the scientist. Indeed, the judge is trying to second-guess the entire scientific community!

That is somewhat hidden from view. For under *Daubert*, judges are now in effect making scientific factual decisions *about whether* to send scientific factual evidence to the jury.

Granted, sometimes we do want the judge to decide factual matters. For example, when the defense to a breach of contract is oppression of one of the parties by the other, the jury is so likely to be inflamed that even though deciding whether there was oppression is a mixed question of fact and law, virtually every jurisdiction in the country has the judge decide the matter. The question then becomes whether we can analogize from oppression or other paradigms of judges’ best deciding factual issues to cases involving scientific evidence. I shall not attempt that task in this paper, but surely not every case involving complex science is likely to inflame the jury.

We might also ask how much judges already decide factual issues using FRE. I think they are deciding factual issues all the time. For instance, every hearsay exception has a fact-intensive fuzzy border. If all the facts were clear as to how the evidence law applied to proposed items of evidence, there probably would be far fewer lawsuits. Indeed, if you left literally *every* question of fact to the jury, the jury would be overwhelmed and federal evidence law would be very different from the way it is now--almost unrecognizable.

While there are two sides to this question, I would think that we want to leave facts to the jury as much as possible, and that the burden is on Friedman to substantiate his view with an argument more specifically addressing *Daubert-Frye* situations. Merely asserting that “*Frye* is more like judicial notice than *Daubert* is, therefore *Frye* is to that extent worse than *Daubert*,” is not enough.

Friedman himself poses a basic dilemma which implies the problem I just described as an instance, though he seems unaware of the implication:

[T]he basic dilemma is clear. If we exclude the expert evidence from the factfinders’ consideration, we may be depriving the truth-determining process of information that would assist in learning the truth. On the other hand, it may be that the evidence is in fact worthless, or of far less value than the factfinder is likely to attribute to it. Thus, allowing the factfinders to hear and use the evidence may actually lead them further away from the truth. (Friedman: 1994: 134-35)

Friedman poses this as the post-*Frye* problem. But it always was the problem, and in particular it was a dilemma *Frye* faced too. *Frye* resolved it by impaling itself on the horn of excluding too much. At least, that is what many of its pro-plaintiff critics would have us think.

Since I largely agree with the rest of Friedman’s paper, I shall not discuss it much further. He does have a good answer to the question, Where did the *Daubert* element of reliability come from? Rule 702 does not expressly make reliability a test of admissibility. Reliability, of course, is a far stronger test than relevance; and the *Daubert* rule is *supposed* to be a mere relevance rule.

Friedman says that the Court merely equates reliability with “scientific validity” (Friedman: 1994: 138-39). And that is a very straightforward and reasonable interpretation of how reliability fits into the picture in FRE 702. For FRE 702 does speak of “scientific...knowledge,” which can be taken as implying scientific validity.

At least by implication, Friedman makes the point well that *Daubert*'s concern is not to duplicate or recreate the scientific laboratory in the courtroom, but instead to describe how much *deference* to give to expert scientific evidence (Friedman: 1994: 146-48). *Daubert* creates a free-flowing, activist role for the judge that more or less accords with the more activist managerial role judges now have in complex litigation in general. The far more deferential *Frye* accords better with the old conception of a judge as a passive umpire, and in that sense *Daubert* is a sign of the changing times.

As Friedman says, *Daubert* reflects not only distrust in juries' ability to assess complex and subtle scientific evidence, but confidence in judges' ability to assess such evidence in an activist role effectively (Friedman 1994: 144).

Intimately related to the question of judicial deference is the *Daubert* admonition that science and law have very different objectives. Friedman quotes Justice Blackmun as saying:

[T]here 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.

(Friedman: 1994: 146)

This important point obviously favors *Frye*, which is far quicker and easier to apply than *Daubert*. The point may also be found in the Foster-Huber work on junk science in the courtroom. In fact, the point is at least a hundred years old. Pitt Taylor says in 1897:

The search for abstract truth, scientific or otherwise, is not usually limited in time. No fact at all is too remotely relevant to deserve consideration. No pressing necessity usually exists that the precise fact should be ascertained this year or next year, or, indeed, within the next century. Under such conditions, logic is given its unimpeded course. All facts logically relevant demand and receive consideration.

But the course of trials in Courts of law by no means admits of such extended search into the minutiae of proof. The tribunal sits for a limited time....The proceedings are expensive....There is a recognized necessity that matters should be as speedily disposed of as the interests of justice will admit....

(Taylor 1897: 2⁶-2⁸)

The point is repeated in contemporary literature on mass torts caused by mass hysteria due to the public perception of risks which science must take a long time to establish as veridical or as only phantom risks, which is longer than courts can afford to take (Foster 1993: 19-20). Practically by definition, *Frye* would always go against such risks, while *Daubert* would at least leave the door open. Perhaps that is the most important consideration favoring *Daubert*.

Feldman says that the *Daubert* Court wished to make evidence law more like scientific procedure. But Popper, in the very book both Feldman and the *Daubert* Court cite, finds an

already existing close analogy between the two. Popper says that precisely because nothing can be conclusively established in science, not even what we observe on any occasion, we must always make a *decision* as to what to admit as a fact in science. And he finds this to be very *close* to what judges have to do. Perhaps scientists can postpone decisions longer than judges can. But scientists cannot do science at all unless they make decisions (Popper 1965: 108-11). Feldman is unaware of Popper's view here, and the *Daubert* Court seems to be unaware of it too, since it emphasizes the contrast between science and law without citing Popper to the contrary (*Daubert* at 2798). But it seems a well-known aspect of Popper's thought from 1935 on (see e.g. Bunge 1964: 38-39).

Legal cases do get postponed for years. We dwell on the disadvantages, but I think that sometimes there are advantages, precisely such as finally getting reliable evidence. And just as scientists must make some decisions now and can postpone deeper reflection until later, so judges must make decisions now but can continue thinking about them many years later. Indeed, most of our casebooks discuss at least some cases that are centuries old--far older than most technical or scientific problems discussed today.

Finally, a courtroom may not be a scientific laboratory, but it is a legal laboratory for the case study of human nature. This supports Popper's analogy from the other direction, so to speak. That leaves us with two closely related questions. First, do the similarities between science and law outweigh the differences? Second, granting that the law must defer to science to some degree, should that be to the greater degree of *Frye* or the lesser degree of *Daubert*? I shall argue later that the differences outweigh the similarities, favoring the greater deference of *Frye*.

6. Leiter's views

Leiter, in his unpublished paper, "The Epistemology of Admissibility," attempts more than Friedman, but in my opinion makes bigger mistakes in result.

The first part of Leiter's paper is about history of philosophy of science, and especially about the accuracy of Feldman's account of a big move from logical empiricism to revised empiricism. Leiter commits three blunders.

First, Leiter unquestioningly accepts Feldman's account of logical empiricism. It would appear that Leiter has read no more about the logical empiricists than Feldman has.

Second, Leiter cites Duhem, but apparently without any idea of Duhem's influence on the logical empiricists. Leiter apparently has no idea that Duhem's great work was first published in 1906, thus throwing a big monkey wrench into Feldman's timetable. For Leiter unquestioningly accepts Feldman's timetable that the two main revised empiricist criticisms of logical empiricism came into being "in the 1950's and 1960's" (Leiter 1996: 3). Leiter even cites a 1951 translation of Duhem as a source of the second of these revised empiricist criticisms, as if Duhem wrote it in 1951!

Third, while unlike Feldman, Leiter has the great merit of recognizing that Duhem is a revised empiricist, Leiter fails to recognize that Duhem was the progenitor of *both* of revised empiricism's principal criticisms of logical empiricism. Leiter attributes to Duhem only the criticism that "the problem of 'auxiliary hypotheses' renders all testing (and especially falsification) problematic" (Leiter 1996: 4). But Duhem is also famous for arguing that observational statements in science are themselves "theory laden" (Leiter 1996: 4). Leiter cites the "classic sources on this point" as books and papers ranging from 1958 to 1981, overlooking the

classic source, Duhem in 1906.

In Duhem's 1906 book, that observation in science is always and essentially theory-laden is the principal thesis of part 2, chapter 4; that auxiliary hypotheses make it problematic which part of a falsified theory to reject is the principal thesis of part 2, chapter 6; and that neither experience nor strict logic provides any guidance in the framing or revision of scientific hypotheses is a huge thesis that stretches from part 2, chapter 6 through the whole of part 2, chapter 7. It is hard to see how anyone reading the table of contents of the book could miss this. Leiter goes on to relate Feldman's view that revised empiricism's answer to the problem that experience alone does not tell us which part of a falsified theory to revise is somehow found in recognizing science as a "collective process...which shape[s] and check[s] individual judgment" (Leiter 1996: 5, quoting Feldman). "Thus 'revised empiricism', unlike logical empiricism, assigns proper weight to the role of social factors in the constitution of social knowledge: 'scientists' collective judgments facilitated and established through devices such as peer review and publication and measured by general acceptance--are as distinctively characteristic of science as testability itself" (Leiter 1996: 5, quoting Feldman). Leiter unquestioningly enthusiastically deems this "Feldman's ingenious rationalization and defense" of the *Daubert* rule, admitting only that it is a post hoc defense the Court itself never used or thought of (Leiter 1996: 5). I agree with the admission, but am less happy with Feldman's "rationalization," which is a non sequitur. Recall my example of Einstein on a desert island doing better science than a community of lesser scientists. Note that Feldman's own words, "measured by general acceptance," foreshadow my arguments below that *Frye* is better than *Daubert*. For all agree that measurement by general acceptance is the essence of *Frye*.

On the bright side, Leiter attacks Feldman from the other end, so to speak, by criticizing revised empiricism as outdated. The concept of the attack is ingenious, but I have nothing to say about its execution.

Leiter's last main point is that Feldman fails to distinguish good philosophy of science from good legal rules. This is a good point, but it is ancient history (Taylor 1897). It would be more exciting if Feldman could rebut Leiter's and my general distinction with detailed argument in the particular case of the *Daubert* rule.

A minor scholarly criticism is that Leiter thinks that Quinean naturalized epistemology is non-normative. Quine expressly admits norms in natural science of perception in his greatest work, *Word and Object*. What could be more basic to naturalistic epistemology than natural science of sense-perception? "Norm" is listed in the index to Quine's book.

7. Arguments for *Frye*

My arguments follow. I am not arguing that *Frye* is a gift from heaven and *Daubert* a rule from hell. I am arguing only that *Frye* is better by a preponderance of the argumentation.

My first argument for *Frye* is this. As anyone can see, as a practical matter at the very least, there is going to be no general acceptance of a scientific theory these days without a great deal of testability, publication, and peer review already existing as basic preconditions. Thus general acceptance alone swallows up the other mentioned (and unmentioned) factors of *Daubert*. That is both a legal-theoretic (not: scientific-theoretic) and a practical simplification, replacing a whole flock of difficult kinds of vagueness with one rather simple vagueness, thus promoting certainty and reducing the tide of litigation and its debris of high insurance costs, stifled new product

development, and actual withdrawal of important medical products and services. Just look for widespread acceptance, and the rest of the factors will generally be included.

A second argument is that the *Frye* rule is more appropriate to the judicial role than *Daubert*'s, since it is closer to judicial notice, while *Daubert*'s is essentially fact-intensive and usurps the jury's role, not to mention the scientist's role. Whether the jury is any more able to assess the facts than a judge, it *is* the jury's great traditional role.

A third argument is that *Frye* better supports the great traditional rule of fault-finding in negligence torts that the defendant is innocent until shown guilty by a preponderance of the evidence (my use of criminal terminology is no mistake, since blame for wrongdoing is basic to negligence).

Fourth, *Frye* is accused of being harsh and simplistic. But that depends on your point of view. Harsh to whom? *Frye* is harsh to gullible plaintiffs and their greedy trial lawyers in *Daubert* itself! *Frye* would scarcely be harsh to the innocent defendant, the maker of Bendectin. Note that where corporations are in the wrong in mass torts, as in Dalkon Shield, they are terribly and multiply wrong. One of the cardinal rules I learned in my military service about major disasters in big organizations which have multiple and even redundant safeguards is that such disasters just don't happen unless the safeguards multiply fail, meaning that there is a *lot* of blame to go around within the big organization. The repeated egregious wrongs concerning Dalkon Shield are remarkably similar to those revealed in the training film, "USS FORRESTAL: A Carrier Fights for Life," a story about the great fire aboard FORRESTAL which took over fifty lives due to a great many problems with their damage control program. If even half the problems had been taken care of before the fire started, the fire probably could have been stopped before it

reached the aircraft fuel tanks. I suggest that much the same is true in the case of the infamous Dalkon Shield. If even half the procedures had been properly followed, it probably would never have reached the market.

Fifth, it is a wise principle for judges to resolve issues at the lowest theoretical level possible. *Frye* surpasses *Daubert* on that score.

Sixth and perhaps most importantly, *Frye* maximizes both scientific reliability and quickness of applicability of the legal rule. This is a remarkable achievement, considering what strange bedfellows scientific reliability and quickness of result typically make in science itself.

This concludes my own chief arguments for *Frye*, but there is more to follow.

8. *Daubert* briefs citing Popper

Of the twenty-five amici briefs to the *Daubert* Court, I read the four that cite Popper. (My “loc: Hempel” search of these twenty-five briefs turned up no citations of Hempel.) I shall now discuss the four briefs. I found much in them that supports *Frye*.

1. Amicus brief of the Carnegie Commission on Science, Technology, and Government.

Broadly speaking, amicus belongs to the scientific community, as opposed to mass tort plaintiffs or defendant corporations. But amicus reports on science rather than doing science. Amicus finds that while assessing scientific evidence today is hard enough to begin with, due to the complex problems of mass torts, “The enactment of the Federal Rules of Evidence in 1975 has caused a further problem for the courts as judges have sought to determine the impact of the new rules on handling these new problems.” The brief goes on to discuss primarily philosophy of

science.

Amicus emphasizes the importance of the scientific community, which is “overlain with sociological perturbations.” “Some philosophers and sociologists have gone so far as to assert that scientific ‘facts’ are socially constructed hypotheses that contain value-laden choices. See Sheila Jasanoff...” However, amicus does not seem to go quite as far as Jasanoff, since amicus goes on to offer a middle-of-the-road theory of reliable science.

Amicus says “appropriate scientific practices cannot be reduced to a single, definitive list,” but certain “kinds of factors” can be suggested: degree of acceptance in the scientific field; and capacity of the theory to withstand criticism, as through “peer review or other mode of institutionalized self criticism. Of central importance is whether the expert adhered to generally recognized forms of scientific inquiry.”

Amicus rejects *Frye*. “The unresponsiveness of ‘general acceptance’ to the scientific process leads to an unprincipled test that taken literally rejects valuable insights that bear all the hallmarks of acceptable science, but provides no guidance on how to distinguish the product of marginal or insupportable fringe activities.” “[G]eneral acceptance’ is an ineffective test that misconceives the nature of the scientific enterprise. It assumes that there is much more definiteness in science than actually exists, and that this precision takes the form of widely held beliefs about reality that can readily be found. See Sheila Jasanoff...”

I object that amicus fails to make the Taylor-Huber-Leiter distinction between what scientists ought to do in the laboratory and what judges ought to do in the courtroom. The general acceptance test does not misconceive the nature of the scientific enterprise because it never attempts to describe, reflect, or even approximate that nature in the first place. It is a legal

rule devised for legal purposes. Even amicus does not want to go so far as to make judges into mini-scientists, as we shall see in the next paragraph.

Amicus discusses *United States v. Downing*, 753 F.2d 1224 (3d Cir. 1985), which replaced *Frye* with an eight factor test. Amicus does not like the *Downing* rule because it goes in the opposite direction and stresses judicial evaluation of scientific testing for reliability so strongly that “[t]he *Downing* approach fails to accord sufficient deference to science to produce valuable results when a process consonant with scientific methods is employed.” This is consistent with *Daubert*’s strong sense of deference to scientific method as reliable in itself, and on *Daubert*’s whittling *Downing*’s eight factors down to four. Clearly use of scientific method is the most important factor. Accordingly, amicus discusses why scientific method is reliable. It is here that amicus cites Popper on the replicability and falsifiability of experiments as the key to reliability of scientific method. Thus Popper is the most fundamental figure in amicus’ brief. Peer review emerges as an indirect or secondary test of whether a scientific test has actually been made of a hypothesis. Thus amicus is no Pythagorean number mystic.

I approve of amicus as correctly distinguishing what is basic and what is peripheral to science. Amicus supports my position pretty exactly on that score, as against Feldman. The only problem with amicus is its failure to grasp the Taylor-Huber-Leiter distinction between science and law as favoring *Frye*.

2. *Amicus brief of the American Medical Association.*

Amicus belongs to the scientific community, as opposed to mass tort plaintiffs or defendant corporations. The brief was joined by several medical academies, colleges, and

societies.

Amicus supports FRE 702 for familiar reasons of justice and utility:

There is good reason--beyond achieving a just outcome in a particular case--to require testimony of a proffered expert to be based upon scientific knowledge. Recent history demonstrates that legal judgments based upon opinions not developed in accordance with scientific methodology have caused manufacturers to withdraw a number of safe and effective vaccines and drugs, including Bendectin and various contraceptives, from the market. Such legal judgments have also significantly stymied innovation in pharmaceutical and vaccine research. Finally, such judgments have forced certain medical specialists to discontinue providing certain services, particularly obstetrical care. The withdrawal of beneficial products and medical services has a serious and entirely unwarranted adverse effect on public health. Rule 702 promotes the factfinding function of the legal community and simultaneously avoids doing violence to the values that undergird science.

I think you can see it coming that, much as in the first brief I discussed, amicus #2 is going to find scientific method the factor most basic to scientific reliability, and Popper will be at the bottom of amicus' description of what makes scientific method reliable. So I invite your attention instead to the last sentence just quoted, as suggesting that FRE 702 is an *amphibious* rule that tries to live in two worlds: that of science and that of law. If the judge is a gatekeeper between those worlds,

then FRE 702 is the gate or passageway itself. This is clearly a middle-of-the-road compromise conception, and I value it for illuminating what *Daubert* is all about. This amphibian rule is designed to be able to crawl about in the world of science, and detect and ingest healthy science and avoid eating unhealthy science. But it really swims in the legal world as its proper home, bringing bounty from the world of science above to the waiting jury below.

This is a long brief that amplifies the first brief in considerable detail, but it is not much different in its outlook, and my critique of it would be much the same. Amicus does add that while science is never immune from revision, still a great deal of it is extremely reliable. With this I think we may all agree.

Amicus adds another important utility argument:

To be sure, the law will always remain something of a mystery to professionals in other disciplines, just as the nuances of science will lie beyond the competence of most attorneys. But the law loses credibility in the eyes of physicians and other scientists when it imposes liability, or even the risk of liability and the burdens of trial, on the basis of evidence that lacks serious scientific merit. Those trained in science and medicine quite rightly have little respect for the workings of law if it does nothing more than transfer resources from one party to another without regard for the basic methodologies and common understanding of scientific practitioners.

This is a strong argument for *Daubert*. But it is an even stronger argument for *Frye*, since *Frye* is

a sterner gatekeeper.

I also value this amicus for its authoritative words on reanalysis. Amicus twice indicates that it accepts the Ninth Circuit's view that the reanalysis in the *Daubert* litigation was a mere manipulation of "data in existing studies in a scientifically unprincipled way (solely in anticipation of litigation)....Dr. Adrian Gross did not publish his calculations or subject them to the scrutiny of other scientists through the process of peer review." Yet:

"Reanalysis" or "meta-analysis" of existing epidemiological data--when properly conducted and reviewed--may be an acceptable and scientific methodology. See H. Kahn and C. Sempos, *Statistical Methods in Epidemiology* 8-11 (1989); O. Miettinen, *Theoretical Epidemiology* 109-15 (1985); T. Colton, *Statistics in Medicine* 99-146 (1974). Indeed, a "meta-analysis" may find statistical significance across a range of studies where no individual study found a statistically significant result. See H. Sacks et al., *Meta-Analyses of Randomized Control Trials*, in *Medical Uses of Statistics* 427-41 (J. Bailar and F. Mosteller eds., 2d ed. 1992). However, there is no indication that a statistically valid meta-analysis--i.e. a meta-analysis performed in accordance with scientific methodology--was performed by any of the expert witnesses offered by petitioners.

3. *Amicus brief of Product Liability Council, Inc, National Association of Manufacturers, et al.*

Broadly speaking, amicus is on the side of defendant corporations. This is more or less a plain legal opinion, not concerned to discuss philosophy of science as such. It is valuable for its

arguments for a pro-*Frye* interpretation of FRE 702. Strictly speaking, that is not part of what I am arguing for in this paper. As I said at the start, I am ignoring the purely legal justification of *Daubert*'s conception of FRE 702 as overruling *Frye*. However, the brief is also good on the merits of the *Daubert-Frye* debate on scientific reliability.

The chief strategy of amicus is the absorption of most of the pro-*Daubert* argument as compatible with *Frye*. Amicus agrees that scientific method is the key to reliable science, and that Popper is the key to understanding what reliable scientific method is: replicability plus verifiability (the latter is close enough to falsifiability to report Popper's view fairly well). But "In sum, amici submit that the gateways affirmed by the Federal Rules of Evidence in effect incorporate the scientific method, and that the Frye test represents one appropriate way of adapting the scientific method to the courtroom setting." Thus amicus #3 is even more against petitioners' desire to admit the questionable meta-analysis tending to blame Bendectin than are amici #1 and #2.

The chief thing amicus adds to my own pro-*Frye* arguments is that some studies show that juries can be confused by complex epidemiological analysis admitted through the judicial gateway:

In a sobering study, Dr. Molly Treadway Johnson, now of the Federal Judicial Center, sought to determine experimentally the ability of lay jurors to comprehend epidemiological evidence bearing on causation. Molly Treadway, *An Investigation of Juror Comprehension of Statistical Proof of Causation* (1990) (Unpublished Ph.D. dissertation, Johns Hopkins University). In one experiment, 25 jurors who had been called for jury duty in Baltimore state court were shown two sets of epidemiological data and were asked to answer four questions about those data

(e.g., whether the results of the study indicate that being exposed to a given substance increases a person's risk of developing a certain abnormality). Out of 100 yes-no responses, only 41 were correct and only two subjects (or eight percent) answered all four questions correctly. In a second experiment, Dr. Johnson exposed one group of 30 jurors to a videotaped, simulated disposition in which an epidemiologist was being questioned by a lawyer. A second group of 15 jurors was not shown the tape. Overall, only three subjects answered all four questions correctly. Dr. Johnson found that "there was no difference...between the expert and non-expert groups in terms of the number of subjects who used a correct approach at least once." *Id.* at 82-83. "It appears, then, that the expert testimony did not provide subjects with an understanding of how to analyze and interpret epidemiological data.: *Id.* at 83. She concluded that

[t]he results of Experiments 1 and 2 paint a rather dismal picture of lay jurors' ability to understand epidemiological analysis. Subjects began with a poor understanding of epidemiological reasoning, and apparently were not helped when provided with expert testimony.

Admittedly, these are not the greatest studies in the world. But think of it-- 60% of the time the jurors guessed wrong about *which party* the epidemiological study supports! And an expert witness could not help them! I think that is a rather good argument against *Daubert*. It is also a good argument for a pro-*Frye* interpretation of FRE, since FRE 403 speaks directly against confusing the jury with evidence it cannot understand. Of course, we might superimpose FRE 403

on a *Daubert*-construed FRE 702 by using FRE 403 to exclude expert evidence admissible under FRE 702. But it would be simpler and more elegant to build this aspect directly into FRE 702, and not find the expert evidence admissible under FRE 702 in the first place. It is such *Daubertian* Rube Goldberg pathways or Ptolemaic epicycles that make the law look bad to the public, if not also to lawyers. And the FRE rules are supposed to be working together!

4. *Amicus brief of Nicholas Bloembergen et al.--“eighteen scientists, scholars, and teachers of science” including “six Nobel laureates--Nicholas Bloembergen in Physics, 1981...”*

Amicus belongs to the scientific community.

Amicus holds that publication and peer review are “vital” to the scientific community as a means of making data and methods available to be checked, but are not in themselves a guarantee of reliability of scientific method. “[T]he process of publication and peer review merely leads to a rebuttable presumption of scientific validity.”

Amicus finds that the primary reason the Ninth Circuit rejected the petitioners’ expert witnesses was not lack of general acceptance of their views in the scientific community, but their lack of reliable scientific methodology. Publication and peer review are important because only they make it possible for the community to test scientific methods and results for reliability.

“[P]ublication is only a starting point of peer review.” That is because “peer review journals do not replicate and verify the experiments....” “Publication is merely one aspect of peer review.”

Peer review is important because without it there will be no progress in efforts to improve scientific reliability. In turn, publication is important because without it there can be no peer review. (Amicus understands publication in a wide sense of communication.)

Thus for amicus, scientific method is the sun, peer review a moon reflecting the sun's importance, and publication a second-order moon reflecting peer review's importance. There is no Pythagorean number mysticism here.

Amicus holds that lack of publication of theories in a peer review journal "does not mean they are not generally accepted." For those are logically independent factors.

Amicus finds the chief difference between science and the courtroom is that the judge must arrive at a specific conclusion, while:

scientific journals are typically concerned...in making progress with sound methodology. The journal will let the conclusions fall where they may. Individual scientists who might have an interest in reaching a particular conclusion at the expense of rigor in methodology are challenged in the peer review process.

That is, the chief difference is that the scientific peer review process enables scientists to be disinterested in specific results, while the courtroom adversarial process encourages parties to be interested in specific results.

Widespread acceptance boils down to repeated acceptance in the peer review process:

The publication once of a new technique, no matter how esteemed the reputation of the author or the journal, gives it no inherent validity. That first publication simply says the editor and reviewers feel the article is of interest and do not immediately see why the methodology or theory will not work. The consequent

exposure, then, of the technique to the scientific community allows for discussion and hopefully of its replication. Its continued use, and acceptance in peer review journals, provides some assurance of validity, but does not provide any guarantee.

Thus the expertise of a scientific expert witness in a courtroom is not established by mere publication alone, but by the meeting the test of subsequent peer review challenges to the merits of the publication, based on attempts in the scientific community to replicate the conditions and verify the findings. Again, this is where Popper explains the bottom line: it is replication and verification that really establish the reliability of science. Replication and verification are the power behind the thrones of publication, peer review, and even of rationally based widespread scientific acceptance:

Replicability, which is noted by Popper and others as the hallmark and guarantee of scientific acceptability, involves other scientists testing the accuracy of observations or of the predictions of an hypothesis.

In this sense, verification is just an aspect of replication. Verification is just the replication of results or findings. I might add that the fourth *Daubert* factor, rate of error, is a by-product of establishing reliability. It is in fact the logical converse of reliability. If method M is 100% reliable, then its rate of error is 0%, and vice versa. This is not number mysticism, but mere logical conversion.

All this implies another “important difference between science and law”:

[T]he propositions to be tested in science are predictive while the facts to be proved in the legal process arise out of situations that occur in the past and which cannot be repeated exactly. n12 The legal process rarely has the luxury of being able to repeat experimentally a disputed chain of causation to corroborate the proffered hypothesis, even if, in some cases, it might be theoretically possible.

n12. As Judge Easterbrook observed in *Branion v. Gramly*, 855 F.2d 1256, 1264 (7th Cir. 1988):

Every event, if specified in detail, is extremely improbable; indeed, with enough detail it is unique in the history of the universe....

Amicus acknowledges that the court of appeals below relied not on *Frye* but on FRE 702 and 703. Nonetheless, amicus finds both the result below and “principle articulated in this brief [which I am now discussing] consistent with the ‘*Frye* rule’.”

Needless to say, I accept amicus’ delineation of important differences between science and law which tend to support the *Frye* rule. But that should not blind us to the fact that there are also similarities, such as the one I cited Popper as noticing, that judges and scientists alike make time-driven decisions all the time. I only hold that the differences outweigh the similarities with respect to relevance to the *Frye-Daubert* dispute. In particular, it is not clear to me why the time-driven similarity Popper cites has any relevance to the dispute. For the judges’ and the scientists’ time-driven decisions are very different in kind. In particular, the judge is time-driven to give a final decision which will be the legal truth of the matter. But the scientist defers final decisions as to the scientific truth to the indefinite future. In contrast, the differences amicus

points out between science and law are important considerations clearly favoring *Frye*.

Indeed, amicus calls the *difference* on time-driven finality “critical” and says, “This concept of finality--essential to adjudication--is completely foreign to science.” Amicus concludes:

The stands for the reception of evidence by courts in the course of adjudication are thus not the same as the standards that scientists use in accepting or rejecting new data or theories....

[I]n adjudication the law establishes its own standards of precision and reliability, and mandates that the “truth” be determined from admissible evidence.

At the same time, the legal rules of admissibility of evidence will be inadequate if they contain:

none of the safeguards of review, replication and evaluation by the scientific community that are essential parts of the process of acceptable scientific inquiry. It is not enough, we submit, to rely on cross-examination of scientific experts before a lay jury to separate the valid from the bogus. It is doubtful whether a lay jury would understand the intricacies or subtleties of sophisticated analysis and criticism....

Amicus suggests that the chief thing is “the opportunity to show” widespread acceptance or criticism by the scientific community, “or that the theories employed or the conclusions reached have been rejected or falsified by other scientists.” To this end, publication and peer review are

helpful but not infallible guides.

Amicus concludes that the Ninth Circuit test using factors of publication, peer review, and replication “is soundly based upon and is in conformity with the requirements of Fed.R.Evid. 703.” Thus amicus affirms the *Daubert* rule as acceptable, though it seems to prefer the *Frye* rule, or at least to affirm the *Frye* rule as at least as acceptable as the *Daubert* rule.

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