Do you chafe under the burden of proof?
Try Dr. Kuhn's soothing salve

Paul E. Meehl

This is an edited version of an informal talk to students and faculty members of the University of Minnesota's Center for Research in Learning, Perception, and Cognition, given on September 23, 1989, at Wilder Forest, Marine on St. Croix, Minnesota. —Leslie J. Yonce-Meehl

How many of you know who the Dr. Kuhn is in my title? And how many have read the book? All right, by the show of hands, it looks like I'm not talking about something that's totally outside your ken or irrelevant. The book is Thomas Kuhn's, *The Structure of Scientific Revolutions*, 1962. I normally don't give polemical speeches. People who know me will say I'm a reasonably permissive fellow who believes in letting everybody go to the devil in their own way. In fact, the late Kenneth MacCorquodale accused me of being too open-minded. He didn't like the fact that I believed there might be something to telepathy, or that I thought the theory of evolution was not in such great shape as it's always said to be, and he thought I was too open to some of Freud's stranger ideas.[1]

But this is a speech against Thomas Kuhn—or perhaps I should say against some members of our profession who quote Thomas Kuhn for what I considered nefarious purposes. There are half a dozen theses in Kuhn's book, which he claims is about the empirical history of science (he says it is not about *philosophy* of science—although, as our philosopher colleague Wade Savage points out, it really is), and from them he draws certain conclusions about how science works. Whether he intends to say that's how it *should* work is not clear, and when critics have asked, Kuhn never seems to make clear what he thinks about that.

I'm only going to discuss one of the several theses of the book and I don't prejudge the truth or falsity of any of the others. I'm concerned here with Kuhn's thesis that *all observations in science are theory-laden*. That means that there are, therefore, no theory-neutral observations. That means, as some read it, that there are no clear, plain, objective

[1] For instance, Freud said that for males (it doesn't work for female patients) there is a deep thematic connection between the urinary function, ambition as a drive, triumph and shame as emotions, and interest in fire. Based on my experience with patients, I think Freud had something there.

facts. Which means that theories are incommensurable with respect to facts (incommensurable is his favorite word, but he should not have used it this way; it has a technical meaning in mathematics and is not helpful in this connection). And that in turn suggests that scientific change is not rational, it is not forced upon the scientific community by its facts. But if change is not rationally forced by the facts, how the heck does it happen? It allegedly happens as some matter of the scientist's psychology and the social forces in the scientific community. That's the thesis, which I want to say is a mistake.

I think Kuhn's thesis here is incorrect both historically and epistemologically. I think it has an undesirable influence in the social sciences (I'm not competent to say anything about physics or chemistry). And I think that in my field—a soft field, clinical psychology—if it becomes widely accepted it will have an absolutely cancerous influence.

Now, there is an authority problem here. I am not a historian of science. I think I do have some competence as a *philosopher* of science: I've published in their technical journals, I have a title in that department, and Herbert Feigl once told me that anytime I want to become a full-time philosopher he would write me a letter of recommendation. But I claim no competence as a *historian* of science, although I read a great deal of scientific history. So there we have to make a little bit of an *argumentum ad verecundiam*,[2] which is the best you can do if you're not an expert. A number of historians of science think that Kuhn's book is vastly overrated, and some of them think it's a punk book. For instance, Roger Stuewer, historian of physics in the program in History of Science and Technology here, says it is considered a great book, but he's not sure it's even a good book. As I was preparing this talk, I told him I think it's a punk book. Stuewer, being a gentleman and a scholar and not a mean fellow, paused a bit, but then admitted, "Well, Paul, it's had a lot of influence, it… ah… I think it's a punk book too!"

My concern here is not primarily with Kuhn himself but rather with the psychologists who are currently misusing Kuhn's book. If you think I'm talking about something we

[2] *Argumentum ad Verecundiam* fallacy (argument from inappropriate authority): an appeal to the testimony of an authority outside the authority's special field of expertise.

don't have to worry about when I say there's a malignant influence, let me give you some examples. If you read the methodological papers in the *American Psychologist*, you certainly know the frequency with which Thomas Kuhn's ideas about observations being theory-laden are being quoted. You can hardly find a paper on methodology in that journal or in other scientific journals in the soft fields that does not at least make a nod to Thomas Kuhn, and sometimes the whole thesis of the article is founded upon his ideas.

I recently refereed a manuscript by a psychologist whose whole thesis was that there is no such thing as schizophrenia. Since my current research activity is partly involved with schizophrenia, that's very discouraging. I'm apparently studying something that doesn't exist; although, as Seymour Ketty says, "if there ain't any such thing, it sure is inheritable." This manuscript said schizophrenia is nothing but a nasty label put on disadvantaged persons by wicked members of the power elite. It's all right if this ideologue wants to have this crazy idea, but how does he avoid mentioning any of the facts? He doesn't tell us what answer he would give to the twin studies, the foster child studies, the soft neurology research, the psychophysiological research of people who study schizophrenics, the psychometrics even (e.g., the Chinese know the Multiphasic in Hong Kong gives you the same kind of pattern that you get in Milwaukee), and so forth. He doesn't address any of that. He doesn't even mention the research and say the findings are wrong—he just doesn't even mention it. Instead he tells you that "there are people who think that schizophrenia is a disease entity on the basis of certain alleged facts, but due to Thomas Kuhn we know that facts are in the eye of the observer and, of course, I can see the facts as I want." Which in turn means that he doesn't pay any attention to the facts, his attitude being, "They're *your* facts, they go with *your* theory…." As referee I urged rejection of the article.

A much more respectable example is in an excellent book that I'm reading by psychoanalyst Marshall Edelson, far more sophisticated in epistemology than psychoanalysts usually are. The book is *Psychoanalysis: A Theory in Crisis*. Edelson is a worried analyst, a smart, thoughtful man, and I think this book is great. But when he gets to the criticism by some experimental psychologists or behavior modifiers about the psychoanalyst's observations being poisoned by his theory, Edelson proceeds to tell us that everybody knows that all observations are theory-laden. He doesn't even cite Kuhn, and that's worse, because it means a doctrine has become so accepted that authors don't even mention the inventor of it anymore. Edelson is taking it for granted.

Even I—reared in positivism, trained at the University of Minnesota by tough teachers like Paterson and Skinner and Hathaway, and being anti-Kuhnian—can fall into this trap. Here is a quote from a paper I published in 1970, on the difficulties of psychoanalytic research[3] and why I don't do it. I practice psychoanalysis with some patients. I started to do research on it around 1953 and spent a lot of money and time with Kenneth MacCorquodale on analysis of protocols. I concluded that I hadn't proved anything, MacCorquodale agreed, we decided that we weren't smart enough to research the psychoanalytic hour and neither was anybody else, and fortunately I haven't spent any more time on researching it since. Discussing why it is difficult, I say:

> ...We have today the advantage which he [Freud] regrets not having, that recording an analysand's verbal behavior is a simple and inexpensive process. Skinner points out that what makes the science of behavior difficult is not—contrary to the usual view in psychoanalytic writing—problems of observation, because compared with the phenomena of most other sciences behavior is relatively macroscopic and slow. [That's a quote from Fred Skinner] The difficult problems arise in slicing the pie, that is, in classifying intervals of the behavior flux and in subjecting them to powerful conceptual analysis and appropriate statistical treatment. [Now here's the sentence I shouldn't have written.] Whatever one may think of Popper's view that theory subtly infects even so-called observation statements in physics, this is pretty obviously true in psychology because of the trivial fact that an interval of the behavior flux can be sliced up or categorized in different ways. Even in the animal case the problems of response class and stimulus equivalence arise, although less acutely. A patient in an analytic session says, "I suppose you are thinking that this is really about my father, but you're mistaken, because it's not." We can readily conceive of a variety of rubrics under which this chunk of verbal behavior could be plausibly subsumed. We might classify it syntactically, as a complex-compound sentence, or as a negative sentence; or as resistance, since it rejects a possible interpretation; or as negative transference, because it is an attribution of error to the analyst; or, in case the analyst hasn't been having any such

[3] Meehl, P. E. (1970). Some methodological reflections on the difficulties of psychoanalytic research. In M. Radner & S. Winokur (Eds.), *Minnesota studies in the philosophy of science: Vol. IV. Analyses of theories and methods of physics and psychology* (pp. 403-416). Minneapolis: University of Minnesota Press. Available online: meehl.umn.edu

> associations as he listens, we can classify it as an instance of projection; or as an instance of "father theme"; or we might classify it as self-referential, because its subject matter is mainly the patient's thoughts rather than the thoughts or actions of some third party; and so on and on. The problem here is not mainly one of "reliability" in categorization, although goodness knows that's a tough one too. Thorough training to achieve perfect interjudge scoring agreement per rubric would still leave us with the problem I am raising.

Well, the problem I'm raising in that paragraph is a valid problem. The examples are good clinical examples. But it is misleading for me to say that the theory infected observations. The theory suggests to me how I would *subsume* a protocol, but *the protocol is what the patient said*. Somebody might say there is a theoretical problem about classifying words. Professor Viemeister[4] might raise some of those difficulties; I say that's Viemeister's bailiwick, but not Freud's. What we have is the sentence, "You think this is about my father, but you're mistaken because it isn't." As long as we can classify that sentence, the observation is there. It is not theory-infecting of the observation with Freudian theory or with any of that list of possible classifications that I mentioned. By the linguistic level I have in mind, I mean such things as (to use Bertrand Russell's example about what is a word) hearing somebody recently coming from Germany say of canine behavior, "De dok vaks hiss tail ven pleasst,"[5] and I know that the dog wags his tail when pleased. That involves some complicated analysis of how you produce speech, but not a Freudian analysis, right? It is psycholinguistics and psychoacoustics and things like that, which are not the focus of Freudian theory.

"Kuhnian" philosopher Larry Laudan (who relies on Kuhn, but I think a content analysis would show he disagrees with Kuhn over 50% of the time) makes the bow that philosophers are required to make to Kuhn's book these days. But Larry Laudan wrote a much better book than Kuhn, a *great* book![6] He points out that these days young philosophers of science get a lot of mileage by positivist bashing. They can get three or

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[4] Psychology Department colleague, Neal Viemeister, researcher in auditory psychophysics and perception.
[5] Mimicking a German who is speaking English. The German pronounces the word *dog* as *dok*; an English speaker might hear *dock*; but in this context the word is recognized as *dog*. Bertrand Russell, *An Inquiry into Meaning and Truth*, NY: Norton, 1940, p. 27 in Chapter 1 "What is a Word?"
[6] The reference is probably Larry Laudan's *Progress and its Problems: Towards a Theory of Scientific Growth* (1977), which was on Meehl's suggested readings list for his Philosophical Psychology seminar. But other possibilities are Laudan's *Science and Hypothesis: Historical Essays on Scientific Methodology* (1981); *Science and Values: The Aims of Science and Their Role in Scientific Debate* (1984); A. Donovan, L. Laudan, & R. Laudan (Eds.), *Scrutinizing Science: Empirical Studies of Scientific Change* (1988).

four paragraphs of their articles started by beating up on the logical positivists, including saying things about them that are false. (That's partly what gets me polemic about this, because I admired those men.) Let me give you a little history about the logical positivists.

The logical positivists started as a group of intellectuals meeting irregularly in coffee houses in Vienna. They first formally organized in 1924, having weekly meetings at the suggestion of Herbert Feigl (subsequently my teacher and founder of the Philosophy of Science Center at the University of Minnesota—the first such center in the world). The leader of the Vienna Circle was Moritz Schlick, Ph.D. in physics, who held the Mach distinguished chair of philosophy and physics at the University of Vienna. To give you an idea of the range of expertise in the group, some other early members included Hans Hahn, mathematician; Otto Neurath, economist and sociologist (the only card-carrying Marxist in the bunch, the rest of them were all Democratic Socialists); Felix Kaufmann, jurisprudence; Kurt Reidemeister, mathematics; Rudolph Carnap (who joined the group a little later, he wasn't there in 1924), physics, math, symbolic logic, a pupil of the great logician Gotlob Frege; Herbert Feigl, chemistry, philosophy; and some other people came irregularly as visitors.

You commonly hear it said that Karl Popper was a member of the Vienna Circle, but he was not. He was in Vienna, and he came to some of the meetings. But Popper was against the positivist core idea from the beginning. It is an example of the carelessness of some of the Kuhnians that they lump Popper together with the logical positivists. I've seen articles by psychologists saying, "Karl Popper, Reichenbach, and the Vienna Circle…."—that's careless scholarship. Popper thought of the positivists as his enemies. And it's sloppy to lump the Berlin group with the positivists. The Berlin group was headed by Hans Reichenbach (physics, philosophy, mathematics) and included Carl Hempel (philosophy, and known as Peter to his friends) and Richard von Mises (mathematical statistics, and subsequently professor of aeronautical physics at M.I.T.). People routinely, the Kuhnians regularly, called these fellows positivists. You can call Reichenbach a logical *empiricist*, but in Reichenbach's 1938 *Experience and prediction*—still worth reading after a half century, a terrific book that I recently reread—the positivists are routinely his opponents. The first chapter of the book beats up on the verifiability criterion in the form that the positivists used it. Why do I bring it up those historical niceties?—It adds to my thesis that the post-Kuhnians are frequently careless in their scholarship, the psychologists who quote Kuhn especially so.

A related movement in the United States, completely independent in its origin, was the Operationalist movement[7] founded by the great experimental physicist and Nobel laureate Percy W. Bridgman of Harvard. Most philosophers of science did not subscribe to Bridgman's form of operationalism, and most physicists in fact did not, which resulted in his amending it a good deal in a 1936 book (*The Nature of Physical Theory*) that is much less simplistic than his 1927 book (*The Logic of Modern Physics*). American psychologists influenced by the logical positivists—it's arguable how much they were influenced, there was a good deal of autonomist development of the spirit of positivism in the United States—began to hear about the positivist group in Vienna and started referencing them in publications, sometimes for window dressing. People who made a good deal of use of the logical positivist doctrine in their writings, especially for beating up the opposition, included Clark Hull, S. Smith Stevens, E. G. Boring, and E. C. Tolman. B. F. Skinner didn't do much quoting of philosophers of science; as you know, Fred Skinner tends to do not much quoting of anybody. But you will find in the references of Skinner's classic 1938 *Behavior of Organisms* one of the few people cited, other than Skinner himself, is Rudolph Carnap, *Unity of Science*; also there—greatly influencing Skinner's thinking apparently—is Ernst Mach's, *The Analysis of Sensations*.

Let's be clear that I am not going to defend the Vienna Circle in their original position. They made a number of mistakes—none of us is infallible. The worst mistake they made was a definition by Schlick himself, which is just terrible and a good example of how a very smart person can say a dumb thing once in a while. Schlick said, somewhere in the late 1920s, the meaning of a sentence is the method of its verification. That is an awful mistake, as it stands and if taken literally. You don't need sophisticated examples to see why that's not good. Let's take the sentence "Caesar crossed the Rubicon." Now, I believe that statement. A Roman centurion who rode across on horseback along with Julius Caesar on that occasion also believed that statement. The centurion's grounds for believing it were that he was there, he knew Julius personally,

[7] https://plato.stanford.edu/entries/operationalism/

and so on. My grounds of evidence, if pressed, epistemologically stop around the ninth century. The oldest palimpsest we have of the *Gallic War* (located in the Vatican Library) is from the ninth century, plus a few Roman coins that have been dug up, right? That means there is a completely disjoint situation here. My evidentiary base and the centurion's evidentiary base for the sentence "Caesar crossed the Rubicon" are disjoint; if you take Schlick's statement literally, that means we don't mean at all the same thing, which is absurd. Now, we don't mean *exactly* the same thing, because there are semantics, [temporals?], and connotations, but *denotatively*—a guy Caesar, an act of crossing the river Rubicon (which I gather is still around), he crossed it—we mean pretty much the same thing in essence, so that if you take Schlick literally you have a nonsense. Defining meaning turned out to be a little more complicated than the logical positivists had thought.

Secondly, to analyze scientific concepts turned out to be a little more complicated than the logical positivists had thought. I don't suppose with a room full of psychologists I need to make a case for that. For one thing, the *verifiability principle* had some paradoxes in it, which I won't go into here. It became recognized that the verifiability principle could not be a thesis because you couldn't prove it; rather, it is in the nature of a proposal. In addition, their *concept of probability was not adequate*. They held, at that time, a complete frequency view of probability, and it is still not clear whether you can define probability for both epistemic and statistical purposes the same way. That is still in dispute. I think most philosophers of science agree with Carnap that there are really two kinds of probability: one is epistemic or logical—probability$_1$; the other is frequency or physical—probability$_2$. Of course the two kinds had better have some relationship to each other, but they are not the same conceptually. That's a terribly difficult problem, still unsolved.

So the positivists thought it was going to be easier than it turned out to be. They had two aims, one negative and one positive. The negative aim was to liquidate metaphysics and theology, to show that their statements are not false but meaningless, devoid of cognitive meaning. The positive aim was the rational reconstruction of scientific knowledge. When Feigl gave a talk at the University of Minnesota[8] in 1939, he said you can formulate logical positivism in a single sentence: "There are no synthetic a priori propositions." Everything else is supposed to come from that. After the talk, Albrey Castell from the philosophy department (who is the Augustine Castle in Fred Skinner's 1948 novel, *Walden Two*) got up and said, "Professor Feigl, you say there are no synthetic a priori propositions. What kind of a proposition is that one?" Well, Feigl had never heard that particular question before. He handled it rather well, I thought, under the circumstances; but it's damn hard to answer. There are a few who argue that you could do this by a logical analysis of language alone. Carnap in his great book of 1934, *Logical Syntax of Language* (which I do not recommend to you; I think there are only three or four people who have ever read it throughout), explicitly says (and later he said this was a mistake) that the methodology of science or the philosophy of science is nothing more or less than the logic of science, and the logic of science is nothing more than the syntax of scientific language, and the syntax of scientific language is nothing more or less than the combinatorics of certain geometrical forms, meaning the symbols. Well, that would be handy if it were so, but it won't quite wash and the positivists all agreed that was too strong.

The positivists themselves—though more broadly the logical empiricists, so we can include the Berlin group—amended these mistakes from within. Almost all of the developments in the movement came from within rather than from outside critics, mostly because the outside critics were philosophical muddleheads who didn't put the criticism very well. There were exceptions. Brand Blanshard at Yale was no muddleheaded, and he wrote a very good book against the movement.[9] But by and large, metaphysicians and theologians are not the most enlightenedly clear cerebraters that we have, and so most of the objections that they made just washed off. But the positivists came to realize the deficiencies of their own formulations and progressively amended them. By the time I

[8] He was at Iowa and being considered for a position at Minnesota. I was an undergraduate at Minnesota and went to hear him talk.

[9] Probably Blanshard's *Reason and Analysis*, 1962, La Salle, IL: Open Court. Quoting an online review: "With impeccable clarity and elegance, Blanshard presents a fair-minded yet withering characterization of the schools of philosophy dominant in the Anglophone world from the 1920s to the 1960s: logical atomism, logical positivism, and 'ordinary language' philosophy. Most of the objectionable features which Blanshard attacked have now disappeared from analytic philosophy, in part due to the influence of this book. Yet his acute observations retain their freshness and relevance."

first met Herbert Feigl (in 1940, when he came to the University of Minnesota from Iowa and I was a senior) he was starting to call himself a logical empiricist rather than a logical positivist; and by 1950 when he wrote an important paper called "Existential Hypotheses" (partly inspired by MacCorquodale and Meehl, 1948[10]) he was calling himself, under the influence of Wilfred Sellers, an empirical realist. So he had moved quite a bit and he had become persuaded that you couldn't analyze all of the great problems in philosophy of the past as either pseudo-problems (as the Vienna Circle had originally said) or, if not pseudo-problems, that they could be adequately clarified by linguistic methods alone. That is, he came to realize that there are important empirical components. Even for the old mind-body problem—epiphenomenalism versus identity theory versus double-aspect theory, and so on—Feigl came to the conclusion it's not just a matter of language, but there is something there (in the world-knot, as Schopenhauer called the mind-body problem) such that when you pose the mind-body problem, you end up talking about all the big problems of metaphysics—causality, intentionality, meaning, inference, etc.

Well, one of the ways that Kuhnians get by with their positivist bashing is really due to some undergraduate errors in logic. They pick out some statement that the positivists made that was false, such as Schlick's awful blooper; they move from that to saying logical positivism is incorrect, meaning the whole class of ideas the positivists held; and they move from that to saying since logical positivism is false, therefore Kuhn is proved. Well, Kuhn is *contrary* to logical positivism, but *not contradictory* to it in the logicians' sense. And you don't prove that Kuhn is right by proving that the positivists were wrong. Somebody else may be right that doesn't hold either one of those sets of views.

There was a graduate student in psychology in the late 1960s who was, in sheer Miller Analogies score, one of the brightest people we ever had here; he also had a somewhat abrasive style, and I was one of the few faculty who was happy to talk with him; but I did and I knew he was very, very bright. Animal learning Professor Milton Trapole was his adviser and I was his co-adviser, and Herbert Feigl was on the final oral

[10] Feigl, H. (1950). Existential hypotheses: Realistic versus phenomenalistic interpretations. *Philosophy of Science* 17, 35-62; MacCorquodale, K., & Meehl, P. E. (1948). On a distinction between hypothetical constructs and intervening variables. *Psychological Review*, 55, 95-107.

committee. The candidate had written one of the few dissertations allowed by the Department that was a think-piece, requiring no independent research for it (although he had done other independent research); the thesis was a theoretical paper grinding Kuhn's axe and beating up on positivism. The committee expected that. But in the oral, Feigl said, "Mr. Glotz, Meehl here does research in latent learning. He writes down a protocol 'On the test night, Rat 13 turned right.' We know what a rat looks like; and we know this one is number 13 because of how his ears are punched; and the rat scurries through the maze, turning right and going into the goal box. Now, Mr. Glotz, what is theory-laden about that?" Well, the candidate couldn't say anything, of course, because there isn't anything theoretical or theory-laden about that. So what did he say? "Well," he says, "there's the implicit commonsense theory of the genidentity of macro objects."—meaning this rat is the same rat that we all saw yesterday! That shows how hard-up people can get if they hold Kuhn's position. In some deep sense of the epistemologists, as Professor Savage pointed out before my talk, that is an interesting question. It's a very interesting question over in the Philosophy department; it's an irrelevant question in Psychology or Child Development. The genidentity of physical macro objects is part of the common lore of our language as, Quine would say, and it's shared by Tolmanites, Hullians, Skinnerians, Freudians; practically everybody but mystics and schizophrenes believe in the genidentity of macro objects! But the candidate had to shore up his "theory-laden" stance and this was the best thing he could dredge up. Afterwards Feigl said, "Well, he's very bright, but we should have flunked him for chutzpa!"

One of the examples that the Kuhnian's love is "The sun rises in the east in the morning." Holy smoke, what an issue to make of that! If you're Copernicus, you know that it's the earth that's moving; whereas if you're Ptolemy it's the sun that's moving; and therefore, according to the Kuhnians, the word 'sun' does not mean the same thing to both these people. Furthermore, if I'm a disciple of Homer I think it's Apollo's chariot. If I'm a disciple of Anaxamander (ancient Greek, he knew about the solar system, made some good measurements!) the sun is a big hot iron ball, not a bad hypothesis given his data. Professor Nye in astrophysics thinks it's a big globe of hot gas, mostly hydrogen. The sun! As to whether it rises!… Now, how do you deal with a question like that?

Well, I had all this clear in mind as a junior, from taking Feigl's course. When you find that people have an overlay of meanings and causal connections about a word or macro object that people can see without apparatus, what do you do? You sterilize the language. You use Korzybskian subscripts. What the scientist does is dredge up some word from a dead language like Greek or Latin, right? And so, we say, "Now look, you keep saying 'Apollo', and that's okay, that's your theory. I'm not dogmatic against your theory. Then, that thing over there—are we all looking at that thing?—there it is. Okay. Kind of warm? And when a cloud moves in front of it, it gets kind of chilly? And it's bright? Circular? There it is. Okay, we're going to call that 'Sol'. And 'Sol' is going to mean whatever that thing is that the three of us are now looking at. Is that agreed? Yes, we agree on that. And now we say, "Now, this business about 'it arises in the East in the morning.' Ptolemy thinks that the earth is the center and the sun is moving. And Kepler thinks that the earth is moving." (A little problem there for Kuhn, because Kepler's data were from Tycho Brahe who was a Ptolemaic and a believer…, but I won't go into that here.) Now, we're going to say that Sol appears in the morning. About the morning: when is the morning? Well, the cock crows, I feel hungry, the dog wants its breakfast. And, the East? Well, my cabin faces out toward the sea, and the back of the cabin is to the mountains. When I get up in the morning the cement is hard, it hasn't moved in the middle of the night. When I say it's morning, we can define 'morning' with a sand glass if you want, or we can have a watch. In the morning Sol appears in the East as a disk. We see the top of the disk, and then we see the whole disk, and then the distance between the bottom of the desk and the horizon increases. We'll make up a verb for rises, since that 'arises' has the connotation the *disk* is moving, whereas the Galileo & Co. think *we* are moving. We'll invent a word called 'rises' to mean the disk appears at an increasing distance above the horizon. "In the morning Sol rises in the east."[11]

We now have a disinfected sentence, which I submit is theory-neutral. Now you can begin to raise all the interesting questions. You can ask, is it the same thing every day? or does it get created afresh? and if so we'd better start putting Korzybskian subscripts in the form of dates: Sol_1, Sol_2, Sol_{1947} …. We'll keep working at it until we make the sentence theory-neutral enough so that we can start the argument. We can't get an

[11] See Carl R. Kordig who analyzed all this from hell to breakfast back in 1971 (*Justification of Scientific Change*), and none of the Kuhnians ever mention his book, as far as I can tell. Nobody has ever answered his analysis, and it's an excellent book by a philosopher of science.

argument going until we know what we're talking about—that thing over there, whatever it is—and whether it or we are moving. When we get that cleared up, then we can start arguing whether is it Apollo, or a big hot cannonball, or gas, and so forth.

Some of you, perhaps most, may be thinking about now, what's the matter with Meehl, is he getting potty in his old age? This stuff is obvious, this is trivial, this is boring. And if that's what you're thinking, *great*! That's exactly the message I'm trying to get across. It is obvious and trivial and boring, and it takes a Kuhnian to muddle things up so we [have to ask] what we're talking about. The matter of clarifying the semantics to be neutral was worked out long before Thomas Kuhn or any of the contemporary group of philosophers. You might say, well surely though, there must be something to it, or it wouldn't be believed by so many different people. Well, I don't accept that proposition. I can tell you all sorts of things, in our field especially, that have been bought by all sorts of people, things that have absolutely nothing to them. *There is such a thing as fads* in the life of the mind, and in the soft areas, like clinical and social psychology, it's mostly fads. Their theories have half-life of seven to 10 years, and it wouldn't be surprising at all if this Kuhnian fad dries up and blows away. I'm confident that the later Wittgenstein is a fad that will dry up blow away. I haven't learned one single useful thing as a psychologist from reading the later Wittgenstein. I did learn a few interesting ideas from the *Tractatus*, but nothing he wrote after 1920.

Well, what other than fad? I don't want to be unreasonable about this. There are some things other than fad about how theory affects observations. The first one—not very exciting, but it should be mentioned—*a theory may be the thing that gets you into the lab* at all. It can motivate you to do experiments you wouldn't do if you didn't have a theory in mind. Kenneth Spence said to me once that he didn't care about whether Hull's theory of learning was true, it just predicted the behavior of the rat; and I asked him, "Kenneth, why would a grown man want to predict the behavior of a rat?" The fact of the matter is that you don't have any need to predict and control the rat's behavior unless you are in the exterminator business. It's the theory that motivates doing those sorts of learning experiments. It's not the theory as a means to the fact, it's the fact as a means to examining the truth of the theory.

Secondly, *what to pay attention to*: There Kuhn is of course right. But he didn't invent that, did he. Do you think the logical positivists didn't know that the *theory directs our attention selectively*? A layman knows that if he thinks about it for five minutes. There is of course the problem of playing fair when you do that. That aspect of Kuhnian thesis is not constructive but malignant. Namely, suppose you and I have an argument about drive and reinforcement and I've been reading Gallistel[12]—about the wiring in the brain and the super-uppers and the rewarders, etc.—and I have a theory that as the drive goes up it should have a heck of a big effect on amplitude of responses, and that's what I am interested in. Whereas your theory about the wiring is that increased drive mainly affects the strength of probability of responding. We've put rats into the Skinner box, and I go into the lab and do an experiment, and I come out and show you a graph of how the rat, having been made hungrier and hungrier with longer deprivation times, hit the lever harder and harder. You, with your rate theory, say, "What about the rate?" And I say, "Oh, I didn't measure the rate." And you say, "You didn't measure the rate? We were having a debate about these two measures." And I say, "Well, I've read Thomas Kuhn, and so I don't have to measure the rate because everything is incommensurable. My theory says what I should look at, and so I do...." You wouldn't give me the Warren Medal[13] as a post-Kuhnian scientist. That's crummy science! We were debating about these two measures, and I deliberately avoided measuring the thing that you were talking about. That isn't Kuhnian, that's just poor science, right? (I'm not saying that Kuhn actually says you should do that, I don't know what he says. I can't tell what he says in this respect.)

How I measure observations: Suppose I am studying the theory of the genetics of schizophrenia and use the MMPI (as many highly respectable researchers have done) as a measuring device valid in psychology. This is a case in which one part of the theory of schizophrenia (its effects on the cognitive functions of language and semantic understanding) has a real spill-over into the measurement process. That does not distort the protocol, does it. The protocol is what the patient did when he saw "I have a tight band around my head" and put that card into the "False" pile. That is the protocol. When

[12] See, e.g., http://www.pnas.org/content/101/36/13121.full for information on the research of psychologist Charles R. Gallistel.

[13] Society of Experimental Psychologists award, instituted in 1936, for outstanding achievement in Experimental Psychology in the United States and Canada.

pressed, you get back to that which people who are not psychotic or feebleminded or drunk, and who have 20-20 corrected vision can agree on. If we're treating the MMPI as a trustworthy instrument, that's part of psychological theory. It could be just part of psychometric theory. In the case of the schizophrene, it's part of schizophrenia theory, because it involves such things as the way patients understand language and the care with which they attend to the task. No quarrel with Kuhn there at all. There is a question how to interpret it, and everybody agrees on that.

A big one, *whether to include an observation in the corpus of your beliefs*: As an empiricist, you want the facts to control the theory, but in the short run, the history of science shows (Kuhn is absolutely right saying that, but no positivist has ever denied it either) that it's sometimes very good practice to exclude a candidate protocol from the corpus of your belief system, because you have a well confirmed theory that forbids it. We don't have to look to the history of science; that's part of common life, isn't it. Suppose I tell you on January 3rd, in the middle of a Minnesota winter, that it's my birthday and I had such a nice little birthday surprise, I looked out of the window in the morning and there was a squirrel looking in at me, and eating a nut; you might think, well, good for Meehl, he had a nice birthday. Suppose I tell you I looked out of the window and there was a robin; you might wonder, let's see, is Meehl a social psychologist doing an experiment on credibility, or is he getting a little senile, or does he live near an aviary, or is it possible that a robin failed to migrate and survives up in Minnesota? Suppose I tell you I looked out of the window and I saw a leprechaun, we had quite a long conversation. You will think all right, now we know—either he's flipped or it *is* an experiment in credibility, because I do not believe there are any leprechauns, and any protocol uttered by anybody to the effect that he saw one, I will not admit into the corpus of my belief system. All very sound, no quarrel about that. There are deep mathematical and epistemological problems there—very, very tough problems: Under what conditions is the convergence of the whole class of protocols on a theory T so strong that when T precludes observation O_1 we are entitled as scientists to refuse to admit it into the corpus?

The history of science is full of such examples. Mendeleev was told that his periodic table didn't work because the atomic weights of gold and tellurium didn't fit, and he said, "They're wrong," which as it turned out they were. Mendeleev realized everything else fit to such an extent that his organizing scheme could not be, what Wesley Salmon calls, a damn strange coincidence. It had to be right. So the atomic weights must be wrong.

There's the famous case of Einstein and physicist Dayton C. Miller. Miller did a lot of work in the theory of musical sounds and was well known to be a very careful experimenter, he was no dullard, and he was President of the American Physical Society. He did an experiment and recorded ether drift—which of course Einstein's theory forbids. Everybody had accepted since the Michelson–Morley experiment that there wasn't any ether drift. Somebody told Einstein, "You know, Dayton C. Miller is no dope in the lab, and he's getting this ether drift down there in Cleveland." Einstein didn't bat an eye; he said "Must be a thermal artifact." It took 25 years before people went back to reanalyze Miller's protocol with improved computer programming for a big complicated Fourier analysis and showed the correlation with the ambient temperature.

It is well known if you're a historian of science or philosopher of science that there is such a thing as excluding an alleged fact because a theory is so well corroborated that it would be irrational to put the fact into the corpus. Among physicists it is called *occult effects*. Spooky effects. What do you do with occult effects? You let them sit on the shelf, hoping that someday somebody will explain them away.

Concerning the problem about the theory of the instrument used, the Kuhnians don't even make clear whether they are talking about the theory of the instrument and whether it belongs to the same science you're testing or some other science. When I'm doing some research in psychology, I take for granted instruments which have been carefully developed and calibrated by chemists or physicists. Naturally, you have to be careful. David Lykken did early polygraph research in the 1950s. Lykken had spent two years as a student of engineering. When asked as a boy what his father did for a living, he'd write "inventor." Given the influence of his father's thinking and work habits, plus his education in engineering, David Lykken thinks about instrumentation more than some psychologists. He thought that one of the reasons for the failure to replicate certain studies in psychogalvanometric research was that people didn't pay enough attention to the instrumentation. They focused on the verbal stimuli and personality aspects, and they weren't worrying about such things as whether you should shave the skin in the region before attaching electrodes; or whether you used copper or silver electrodes; or whether there might be some interaction with more anxious subjects getting more rapid oxidation of the surface of the electrodes. He was able to explain some of the research paradoxes in terms of instrumentation. Thus, I am not making a careless suggestion that you take all instruments absolutely for granted. Suppose I did a study on psychopathic personality, and you had trouble accepting the protocol. You ask, "By the way, how did you measure this?" I say, "Well, I used a psychogalvanometer." "Well, what actually were you looking at?" "I was looking at the reading on the ammeter, pointer readings, they're pretty objective." "Where'd you get this ammeter?" "I picked it up in the local Junk Shop." If I told you that, you would be reluctant to accept the protocol, which is in observation language about a reading. But, of course, the inference is that it's a statement about the amount of coulombs that are flowing, and that is not equivalent to the pointer reading. There's a causal connection there and a theory about the instrument.

One way of formulating this business of excluding a protocol from the corpus on the grounds of the theory is this: If you're an empiricist, you say that *the protocols collectively control the theory in the long run; but the theory, in the short run, controls individual protocols*. This is the best way I can say it and nobody has yet worked out the details of the structure here, Bayesian or some other form. I don't think science is different from common life or courts of law in this respect. That is not illogical, and it is not going against empiricism, it's just that the logicians haven't cleaned it up for us as to its exact logical structure.

One way the position that all observations are theory-infected gets a certain appearance of truth is the examples chosen. Professor Kuhn has a Ph.D. in physics, and his examples are from areas like astronomy, and Einstein's relativity theory versus Newton. Examples used by others, such as Norwood Russell Hanson, are from nuclear physics. Those are very special domains, as you know. For instance, in the case of relativity theory all of the objects, distances, and kinds that we are concerned with are part of the theory of relativity, which deals with them mathematically in a different way from the classical mechanics of Newton. I saw an article in the last issue of *Philosophy of Science* on the matter of the shrinkage of the distances in the Michelson–Morley experiment as a result of the motion. I thought that was all since settled and done. That is a complicated case, and since I don't understand relativity theory I'm not going to say any more about it. In the case of nuclear physics the instruments used to produce the effects we want are frequently themselves elementary particles in motion, with a certain charge and mass and spin, and so forth. And the instruments of observation are frequently other particles. You want to find out whether there is such a thing as the neutrino and you do a big famous experiment that costs so much it'll never be done over again, and it wonks some particle into a cadmium nucleus, and something else comes out, and you rely on the conservation laws, and it turns out the mathematics of the statistics is right for the neutrino. So both the objects used to produce the observations and the things that are coming out that we can observe are themselves all part of nuclear physics. That makes it somewhat more plausible for the Kuhnians, so that they may have a strong point there.

I can't think of any comparable examples in psychology where that's the case. Of course, you may say there are instances in which the theory is necessary to enable a person to observe something. I think of Professor Irving Biederman's chick-sexing article, how he could properly point out that he was able to do that research because of his geon theory,[14] and I would not dispute that. But he didn't have to explain about geons to the naïve students and faculty members to teach them to sex the chicks. Biederman had to use his theory to find the relevant geons for the task, and he could then tell people what to look for in less than a minute. But you can't explain Biederman's theory in less than a minute, for sure. So examples have to be looked at in detail to see whether they are genuinely cases of being theory-laden or only look like it. And if they are genuine, which theory—a theory from another discipline? A theory of a measuring instrument from physics or chemistry that the psychologist relies on? A theory from your own discipline? If it's from your own discipline, have you subjected it to relatively independent tests? Is it inextricably connected with the part of the theory that you are planning to test? Sometimes yes (as in the Multiphasic example I gave), sometimes no. You have to parse it.

[14] An explanation of visual perception based on simple geometrical components (geons): Biederman, I. (1987). Recognition-by-components: A theory of human image understanding. *Psychological Review*, *94*, 115-147). A test of the usefulness of the theory is described in: Biederman, I., & Shiffrar, M. M. (1987). Sexing day-old chicks: A case study and expert systems analysis of a difficult perceptual-learning task. *Journal of Experimental Psychology*: *Learning*, *Memory*, *and Cognition*, *13*, 640-645.

Now, the positivists themselves could parse it in the way I just did. Take my word for that! This is old stuff to me, from 1940. It takes the Kuhnians to muddle it all together so you have to re-parse it. They put it under the heading "theory-laden" and they don't tell you whether that means of an instrument, or part of the whole theoretical structure, or laden in the sense that calls your interest to something, and so forth.

I don't dispute the existence of expert observer skills. Nobody would dispute that; there are lots of examples around. There was a famous mystery when I was an undergraduate around 1938. Epidemiology was not in an advanced state then and somebody had plotted the incidence of cancer geographically and shown how it dropped off with distance from the Twin Cities, becoming less farther out in Minnesota, in North Dakota still less, and in Montana still less, and likewise less towards Michigan. It seemed very mysterious. I remember it raised speculations about all the Swedes and Germans drinking too much boiling hot coffee. And then there was the milk consumption factor, which had some reality (Professor Bittner).[15] Finally somebody disaggregated the data and found it was almost wholly cancer of the subdivision leukemia, more specifically, childhood leukemia (although some in adults), and that gave them the explanation. It was an artifact of skilled observers. Hal Downey, Ph.D., and Elexious T. Bell, M.D., head of the Department of Pathology, had a special interest in the diagnosis of leukemia. Pathologists trained at the University of Minnesota spotted leukemia cells (especially at the early stages) better than other pathologists did. Even general practitioners showed the trend with distance, because in those days a lot of GPs had their own little pathology in their office. These doctors were just better at spotting it, and there was a statistical distribution as to how far away you would move from the Twin Cities after getting your M.D. or doing your pathology residency at Minnesota.

I believe in clinical intuition, as do all clinical practitioners (except maybe a few behavior modifiers), and I believe I'm pretty good at it. One of Hathaway's students did a Ph.D. thesis in which all the faculty in psychology and psychiatry and all the graduate students and all the interns and residents took an intuition test; of the 135 people, I got the top score. So I have some objective evidence that I have clinical talent. Or, another one, I had a bet with a secretary in the hospital Psychiatric unit years ago, because I

[15] For pathology names: https://www.pathology.umn.edu/about/history

commented, "Look at that psychopath walking down the hall." The secretary said "Dr. Meehl, you know that you can't diagnose people by looking at them. I learned that from Extension General Psych." I said, "Well, maybe you can't diagnose IQ, and maybe not in the normal range, and maybe some people can't, but I can, because Hathaway taught me how." So we made a five dollar bet that in a year's time I would do better than chance at spotting psychopaths at sight—no interview, no tests, once in a while a little verbal behavior (such as one patient saying to another, "I'll see you at the coffee break," or whatever). During the course of a year (it was a small service, 35 beds) I spotted 13 people that I said were psychopaths, and using the Multiphasic as the criterion I missed one. Now that's quite good, so when I say I have radar for psychopaths…. You've seen one, you've seen 'em all.

But now the question is, how much theory is involved in that? Hathaway was against theory (he didn't believe theory should exist, I think, except for psychometric theory) and so he taught his students to look at people. He said, "You are so verbal, you intellectuals, you're so verbal and conceptual, you don't look at people. A house detective looks at people; he looks at how they sit, looks at their fingernails, looks at their hairdo, and that's what you should do." My response was "Okay, Dr. Hathaway, fair enough, I'll do that." I think I had a little natural talent for it. I think there are some people who can't learn to see anything clinical. There are students who go through the program who seem to have a blind eye and a tin ear, and they'll never learn to pay any attention to anything except the words the patient speaks. But if you have some talent for such things, you can be trained to be a better observer. What's the role of theory in that kind of observation? I think very little, but I am not prepared to say none.

One of the places where I have an interest in part of the Kuhnian thesis is clinical intuition. I have a notion—but this is a conjecture, I have no data to support this, and if you know the research on experts versus non-experts in this field, you'll know that it's quite inconsistent and not all that terrific. Suppose I tell you, "Well, yes, psychopaths have a certain kind of animal grace in their movements; and yes, they don't have that worried look that the neurotic and psychotic patients have; and if they're persons of the opposite sex that are not objectively all that statuesque and beautiful, yet they're attractive to you (that's subjective—that's Murray's 'recipathy')." And suppose in addition to those kinds of behavioral aspects that you can verbalize, I tell you things like, "Well, the psychopath à la Karpman, Philip Greenacre, David Lykken, McCord and McCord, and others, basically has a parameter defect in his anxiety condition; and that plus a little metamorphic toughness is the genetic condition to develop the psychopathic syndrome; and due to that defective anxiety parameter, the psychopath has a lack of what we call normal social fear; and he has (if you like Schachter's theory of emotion) a weakness of the affiliative drive, so that his object relations are tenuous; but due to the lack of social fear, those fragile object relations look warm to the unskilled person." Now, my conjecture would be that if I tell you all that stuff (and I could add a lot of other things if I had time), that kind of network of conceptions perhaps sensitizes you to things to look at when you haven't been told in so many words (as in the Biederman study) what to look at. I don't assert that, but I'm open to that possibility. Those things have to be investigated empirically, and I think that in a domain of primates interacting with other primates is one of the places there might be a certain amount of truth in it. But notice, if somebody called it into question, you would have to validate it against something external that is not entirely based upon the theory. It's because we had that Multiphasic sitting there, and that was based upon how the patient sorted the cards, and I expected to see 49' [psychopathic deviate] profiles, and sure enough, 12 out of the 13 were like that.

You have to also remember that a theory can even infect the physical observation. We tend to think physical things are easier to observe then psychic and social things. That's not at all obvious, and I'm sure there are many exceptions. The classic case is in Goring's English study of guards and convicts.[16] Guards (plus the assistant warden, the prison doctor, etc.) estimated the intelligence of the prisoners, and there was a sizable correlation between that and their estimates of heights of convicts' foreheads. Then actual forehead heights were measured, and the correlation with the estimates of intelligence dropped to zero. That means that the theory of the guards—that high foreheads went with smarts—infected their ability to perceive how high somebody's forehead was, because they could make, apparently, a fairly reliable judgment they could agree on with regard to intelligence. It would be nice if IQ tests had been available then, but this was done back in 1895 or so.

[16] Goring, Charles (1913). *The English convict: a statistical study*. London: HMSO.

If you are interested in observability for psychology, I recommend a really beautiful paper by Clark and Paivio (1989)[17] in which they had psychologists rate various things on a seven-point scale as to closeness to observation, amount of theory, what they thought would be the degree of interjudge agreement, and things like that. The factor loadings are huge, and you get things like the observability or factual objectivity of "ego" is 1.44, whereas that of "heart rate" is 7.00. It is a nice little list, it's a cute study.

Now, you might wonder why I get my blood pressure up over this. Why does this bother me? As I say, normally if somebody says something foolish, I figure it's a free country, let a hundred flowers bloom including the dumb ones. But I can introspect three reasons why it bothers me. The first is I don't like sloppy scholarship when you have an audience that can't correct you. When a psychologist writes an article for the *American Psychologist*, knowing that the readership are not historians of science and are not philosophers of science—they're an innocent, helpless audience—and he says not "I agree with Kuhn's thesis, although I am aware that many historians of science disagree"; but instead he says, "Since Thomas Kuhn's book, it is generally agreed that..."—that's dishonest scholarship. He's got this helpless audience, and he's brainwashing them, he's telling them that this is the gospel truth and every historian or philosopher knows that it is. Whereas a whole bunch of philosophers and historians strongly disagree with Kuhn, and some accept some of his theses and some not. Kuhn's views on observation sentences is one that gets them particularly irritated. That's also my main irk.[18] I just think it's careless, like the careless lumping together of Popper and Reichenbach with the positivists. That is terribly careless and, in fact, tells me they haven't read Popper. A Kuhnian who had read Popper could never, never, never make such a mistake as that.

Secondly, in Sir David Ross's book on ethics[19] one of the seven primary moral obligations is the duty of gratitude. I have deep feelings of gratitude to the logical positivists. If you look at my writings in psychology, they are spread (some would say much too widely) in animal learning, psychometrics, personality assessment, forensic

[17] Clark, J. M., & Paivio, A. (1989). Observational and theoretical terms in psychology: A cognitive perspective on scientific language. *American Psychologist, 44*(3), 500-512.

[18] I really don't care whether Kuhn is right about the dichotomy between revolutions and normal science. That thesis has been questioned, too, incidentally. That one to me has a certain plausibility, and I don't have any stake as a psychologist in that one.

[19] W. D. Ross (1930) *The Right and the Good*. Oxford: Oxford University Press. A helpful online resource is https://plato.stanford.edu/entries/william-david-ross/#Bib

psychology, behavior genetics, and recently my work is in taxometrics, I even co-authored a book on Lutheran theology and psychology once. Everything that I have done empirically and theoretically has been helped, not an itty-bit but greatly by what I learned from the positivists. A generation of philosophers of science and even worse psychologists treating them as if they were a bunch of dummies irritates me; it offends me, my sense of historical justice. I knew several of these men well, personally, and others I knew at least for 50 or 60 hours of conversation, and for all their mistakes, and certain naïveté they had about how easy it should be (and maybe a little naïveté about socialism and so forth), not a single member of that bunch was dumb. Any view of the logical positivist movement requiring you to hold that the positivists were dumb is itself dumb and does a historical injustice.

The third reason this gets my dander up is its malignant effect as I see it in the soft fields, which I claim I can discern now as a referee. Much of my work as a clinical psychologist has been devoted to trying to make clinical psychology a little more objective and scientific, which God knows it needs. It is never going to be a science. Like any practice—like medicine, or practicing law, or being an accountant, for that matter—it is an art. But at the least we should try to make it an art that is a little better based upon science. If you're raised in a scientific environment and believe in a theory that you cannot prove—e.g., Jung's, Freud's (I myself am a 40% Freudian and know that I believe a lot of things about Freud that I cannot prove scientifically)—you have cognitive dissonance and scientific guilt. And if the message that we give the young clinicians who hold such theories is that if they have any unpleasant feelings about the burden of proof, in order to prove their ideas they should go read Thomas Kuhn. Tell Father Kuhn "I have this awful burden of guilt that I believe in Jung's theory and I can't prove it statistically." And Kuhn says, "Oh. *Ego te absolvo*…." That message we give the student is a fraud!

It is not a problem about having over-belief (as William James called it). Having over-beliefs doesn't bother me at all. Everybody in this room is full of over-beliefs. That's not a sin. If you decided to come to Minnesota for your Ph.D. rather than to go to Michigan or Stanford or Illinois, you did it on an over-belief. Everybody here who got married or is about to do so suffers from over-belief. You can't buy a car without an over-belief. If you voted in the last election, you have an over-belief. You cannot live daily life, including making some earth-shaking personal decisions, unless you have over-beliefs. The distinction is between having an over-belief in daily life and knowing it, and having one in science which you are pretending is scientifically based. And that is where the sin is. You can even have an over-belief as a scientist, as Lakatos points out in his article on "Degenerating Research Programs." He says if somebody is a lucky maverick, or thinks he's a genius, or likes taking risks, he may say "this sure does look like a degenerating research program; but I'm going to be out of step with all the rest of them, and I'm going to turn out like Mendeleev to be right." Lakatos says, "it's a free country, be my guest, if that's what you want to do." What is sinful is to pretend that the program is not degenerating. Look at the record, as Al Smith said.[20] The requirement of the honest scientist is to know when you're working with a degenerating program.

Take Prout's hypothesis, for example, that everything is built out of hydrogen. It was thought to be dead for about 60 years. It wasn't until isotopes were discovered that people realized Prout was right after all. But there were still a few believers in Prout between the Civil War and Henry Moseley, and they turned out to have been right. Nobody is trying to forbid people to have over-beliefs. But if you have a rationalization in a scientific context claiming that an over-belief is really scientifically based (because, after all, "all facts are in the eye of the beholder, and theories are incommensurable, and therefore in a sense it's open season on beliefs, and I can believe anything I please"), that means that you should really be in some other building on the campus. There are places for that, but not in science classrooms and laboratories.

A cultural remark and I'll stop. Conjecture: There are some historians of ideas who believe that there are cycles in culture between what has been called the Dionysian and the Apollonian. The Apollonian person wants light, clarity, rigor, precision, objectivity, and so forth. The Dionysian person wants darkness, or at least just a few glowing things here and there (preferably rather spooky), and wallows in the subjective. You could also call it the Classical versus the Romantic temperament or tradition. I tend to agree, being an Apollonian, with our psychiatrist colleague Leonard Heston, who says the worst thing that ever happened to Western culture was the Romantic Movement. I tend to believe

[20] Alfred E. Smith (1873-1944), New York State politician, ran for president in 1928, became known for saying in his campaign, "Let's look at the record."

that. You and I can disagree about Romanticism, but the point is, science is supposed to be more or less Apollonian. And that's the two basic questions of the positivists, which, as Feigl once said, don't require any reference to epistemology as such, nothing from symbolic logic, just: "What do you mean? How do you know?" And if you stop feeling responsible to answer those two questions—if somebody asks "what do you mean?" or "how do you know?" and you say, "I read Thomas Kuhn. It's a free country…."—why, then, as I say, you're not engaging in the scientific enterprise anymore.

Now one more thing so you won't think I'm unfairly censoring Kuhn's book. It is not on the reading list I give my Philosophical Psychology class because that list is not for a history of science course or a philosophy course. It's for a psychology course, and what's on that list are the writings of philosophers and historians that have helped me as a psychologist. Since Kuhn hasn't helped me one bit, he's not there for the same reason that Wittgenstein isn't there. But that doesn't mean I'm saying you shouldn't read Kuhn or Wittgenstein. Okay, now I'll stop.

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