

# Dr. Popper: Or How I Learned to Stop Worrying and Love Metaphysics

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## Introduction to Falsificationism

Although his reputation among philosophers was never quite as exalted as it was among non-philosophers, Karl Popper was a pre-eminent figure in 20<sup>th</sup> century philosophy. As a non-philosopher, I won't attempt to adjudicate which take on Popper is the more astute, but I think I can at least sympathize, if not fully agree, with philosophers who believe that Popper is overrated by non-philosophers. In an excellent blog post, [Phillipe Lemoine](#) gives a good explanation of why philosophers look askance at falsificationism, Popper's most important contribution to philosophy.

According to Popper, what distinguishes or demarcates a scientific statement from a non-scientific (metaphysical) statement is whether the statement can, or could be, disproved or refuted – falsified (in the sense of being shown to be false not in the sense of being forged, misrepresented or fraudulently changed) – by an actual or potential observation. Vulnerability to potentially contradictory empirical evidence, according to Popper, is what makes science special, allowing it to progress through a kind of dialectical process of conjecture (hypothesis) and refutation (empirical testing) leading to further conjecture and refutation and so on.

Theories purporting to explain anything and everything are thus non-scientific or metaphysical. Claiming to be able to explain too much is a vice, not a virtue, in science. Science advances by risk-taking, not by playing it safe. Trying to explain too much is actually playing it safe. If you're not willing to take the chance of putting your theory at risk, by saying that this and not that will happen — rather than saying that this or that will happen — you're playing it safe. This view of science, portrayed by Popper in modestly heroic terms, was not unappealing to scientists, and in part accounts for the positive reception of Popper's work among scientists.

But this heroic view of science, as Lemoine nicely explains, was just a bit oversimplified. Theories never exist in a vacuum, there is always implicit or explicit background knowledge that informs and provides context for the application of any theory from which a prediction is deduced. To deduce a prediction from any theory, background knowledge, including complementary theories that are presumed to be valid for purposes of making a prediction, is necessary. Any prediction relies not just on a single theory but on a system of related theories and auxiliary assumptions.

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observed. The one-to-one logical dependence between a theory and a prediction upon which Popper's heroic view of science depends doesn't exist. Because the heroic view of science is too simplified, Lemoine considers it false, at least in the naïve and heroic form in which it is often portrayed by its proponents.

But, as Lemoine himself acknowledges, Popper was not unaware of these issues and actually dealt with some if not all of them. Popper therefore dismissed those criticisms pointing to his various acknowledgments and even anticipations of and responses to the criticisms. Nevertheless, his rhetorical style was generally not to qualify his position but to present it in stark terms, thereby reinforcing the view of his critics that he actually did espouse the naïve version of falsificationism that, only under duress, would be toned down to meet the objections raised to the usual unqualified version of his argument. Popper after all believed in making bold conjectures and framing a theory in the strongest possible terms and characteristically adopted an argumentative and polemical stance in staking out his positions.

### **Toned-Down Falsificationism**

In his tone-downed version of falsificationism, Popper acknowledged that one can never know if a prediction fails because the underlying theory is false or because one of the auxiliary assumptions required to make the prediction is false, or even because of an error in measurement. But that acknowledgment, Popper insisted, does not refute falsificationism, because falsificationism is not a scientific theory about how scientists do science; it is a normative theory about how scientists ought to do science. The normative implication of falsificationism is that scientists should not try to shield their theories by making just-so adjustments in their theories through ad hoc auxiliary assumptions, e.g., *ceteris paribus* assumptions, to shield their theories from empirical disproof. Rather they should accept the falsification of their theories when confronted by observations that conflict with the implications of their theories and then formulate new and better theories to replace the old ones.

But a strict methodological rule against adjusting auxiliary assumptions or making further assumptions of an ad hoc nature would have ruled out many fruitful theoretical developments resulting from attempts to account for failed predictions. For example, the planet Neptune was discovered in 1846 by scientists who posited (ad hoc) the existence of another planet to explain why the planet Uranus did not follow its predicted path. Rather than conclude that the Newtonian theory was falsified by the failure of Uranus to follow the orbital path predicted by Newtonian theory, the French astronomer Urbain Le Verrier posited the existence of another planet that would account for the path actually followed by Uranus. Now in this case, it was possible to observe the predicted position of the new planet, and its discovery in the predicted location turned out to be a sensational confirmation of Newtonian theory.

Popper therefore admitted that making an ad hoc assumption in order to save a theory from refutation was permissible under his version of normative

then available, making the ad hoc assumption testable only in principle, but not in practice. Strictly adhering to Popper's methodological requirement of being able to test independently any ad hoc assumption would have meant accepting the refutation of the Newtonian theory rather than positing the untestable — but true — ad hoc other-planet hypothesis to account for the failed prediction of the orbital path of Uranus.

My point is not that ad hoc assumptions to save a theory from falsification are ok, but to point out that a strict methodological rules requiring rejection of any theory once it appears to be contradicted by empirical evidence and prohibiting the use of any ad hoc assumption to save the theory unless the ad hoc assumption is independently testable might well lead to the wrong conclusion given the nuances and special circumstances associated with every case in which a theory seems to be contradicted by observed evidence. Such contradictions are rarely so blatant that theory cannot be reconciled with the evidence. Indeed, as Popper himself recognized, all observations are themselves understood and interpreted in the light of theoretical presumptions. It is only in extreme cases that evidence cannot be interpreted in a way that more or less conforms to the theory under consideration. At first blush, the Copernican heliocentric view of the world seemed obviously contradicted by direct sensory observation that earth seems flat and the sun rise and sets. Empirical refutation could be avoided only by providing an alternative interpretation of the sensory data that could be reconciled with the apparent — and obvious — flatness and stationarity of the earth and the movement of the sun and moon in the heavens.

So the problem with falsificationism as a normative theory is that it's not obvious why a moderately good, but less than perfect, theory should be abandoned simply because it's not perfect and suffers from occasional predictive failures. To be sure, if a better theory than the one under consideration is available, predicting correctly whenever the one under consideration predicts correctly and predicting more accurately than the one under consideration when the latter fails to predict correctly, the alternative theory is surely preferable, but that simply underscores the point that evaluating any theory in isolation is not very important. After all, every theory, being a simplification, is an imperfect representation of reality. It is only when two or more theories are available that scientists must try to determine which of them is preferable.

### **Oakeshott and the Poverty of Falsificationism**

These problems with falsificationism were brought into clearer focus by Michael Oakeshott in his famous essay "[Rationalism in Politics](#)," which though not directed at Popper himself (whose colleague at the London School of Economics he was) can be read as a critique of Popper's attempt to prescribe methodological rules for scientists to follow in carrying out their research. Methodological rules of the kind propounded by Popper are precisely the sort of supposedly rational rules of practice intended to ensure the successful outcome of an undertaking that Oakeshott believed to be ill-advised and hopelessly naïve. The rationalist conceit in Oakeshott's view is that there are demonstrably correct answers to practical

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The entry on Michael Oakeshott in the *Stanford Encyclopedia of Philosophy* summarizes Oakeshott's position as follows:

The error of Rationalism is to think that making decisions simply requires skill in the technique of applying rules or calculating consequences. In an early essay on this theme, Oakeshott distinguishes between "technical" and "traditional" knowledge. Technical knowledge is of facts or rules that can be easily learned and applied, even by those who are without experience or lack the relevant skills. Traditional knowledge, in contrast, means "knowing how" rather than "knowing that" (Ryle 1949). It is acquired by engaging in an activity and involves judgment in handling facts or rules (RP 12–17). The point is not that rules cannot be "applied" but rather that using them skillfully or prudently means going beyond the instructions they provide.

The idea that a scientist's decision about when to abandon one theory and replace it with another can be reduced to the application of a Popperian falsificationist maxim ignores all the special circumstances and all the accumulated theoretical and practical knowledge that a truly expert scientist will bring to bear in studying and addressing such a problem. Here is how Oakeshott addresses the problem in his famous essay.

These two sorts of knowledge, then, distinguishable but inseparable, are the twin components of the knowledge involved in every human activity. In a practical art such as cookery, nobody supposes that the knowledge that belongs to the good cook is confined to what is or what may be written down in the cookery book: technique and what I have called practical knowledge combine to make skill in cookery wherever it exists. And the same is true of the fine arts, of painting, of music, of poetry: a high degree of technical knowledge, even where it is both subtle and ready, is one thing; the ability to create a work of art, the ability to compose something with real musical qualities, the ability to write a great sonnet, is another, and requires in addition to technique, this other sort of knowledge. Again these two sorts of knowledge are involved in any genuinely scientific activity. The natural scientist will certainly make use of observation and verification that belong to his technique, but these rules remain only one of the components of his knowledge; advances in scientific knowledge were never achieved merely by following the rules. . . .

Technical knowledge . . . is susceptible of formulation in rules, principles, directions, maxims – comprehensively, in propositions. It is possible to write down technical knowledge in a book. Consequently, it does not surprise us that when an artist writes about his art, he writes only about the technique of his art. This is so, not because he is ignorant of what may be called aesthetic element, or thinks it unimportant, but because what he has to say about *that* he has said

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the appearance of certainty: it appears to be possible to be certain about a technique. On the other hand, it is characteristic of practical knowledge that it is not susceptible of formulation of that kind. Its normal expression is in a customary or traditional way of doing things, or, simply, in practice. And this gives it the appearance of imprecision and consequently of uncertainty, of being a matter of opinion, of probability rather than truth. It is indeed knowledge that is expressed in taste or connoisseurship, lacking rigidity and ready for the impress of the mind of the learner. . . .

Technical knowledge, in short, can be both taught and learned in the simplest meanings of these words. On the other hand, practical knowledge can neither be taught nor learned, but only imparted and acquired. It exists only in practice, and the only way to acquire it is by apprenticeship to a master – not because the master can teach it (he cannot), but because it can be acquired only by continuous contact with one who is perpetually practicing it. In the arts and in natural science what normally happens is that the pupil, in being taught and in learning the technique from his master, discovers himself to have acquired also another sort of knowledge than merely technical knowledge, without it ever having been precisely imparted and often without being able to say precisely what it is. Thus a pianist acquires artistry as well as technique, a chess-player style and insight into the game as well as knowledge of the moves, and a scientist acquires (among other things) the sort of judgement which tells him when his technique is leading him astray and the connoisseurship which enables him to distinguish the profitable from the unprofitable directions to explore.

Now, as I understand it, Rationalism is the assertion that what I have called practical knowledge is not knowledge at all, the assertion that, properly speaking, there is no knowledge which is not technical knowledge. The Rationalist holds that the only element of *knowledge* involved in any human activity is technical knowledge and that what I have called practical knowledge is really only a sort of nescience which would be negligible if it were not positively mischievous. (*Rationalism in Politics and Other Essays*, pp. 12-16)

Almost three years ago, I attended the History of Economics Society meeting at Duke University at which Jeff Biddle of Michigan State University delivered his [Presidential Address](#), "Statistical Inference in Economics 1920-1965: Changes in Meaning and Practice, published in the June 2017 issue of the *Journal of the History of Economic Thought*. The paper is a remarkable survey of the differing attitudes towards using formal probability theory as the basis for making empirical inferences from the data. The underlying assumptions of probability theory about the nature of the data were widely viewed as being too extreme to make probability theory an acceptable basis for empirical inferences from the

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becoming more widely accepted, a great deal of empirical work, including by some of the leading empirical economists of the time, avoided using the techniques of statistical inference to assess empirical data using regression analysis. Only in the 1970s was there a rapid sea-change in professional opinion that made statistical inference based on explicit probabilistic assumptions about underlying data distributions the requisite technique for drawing empirical inferences from the analysis of economic data. In the final section of his paper, Biddle offers an explanation for this rapid change in professional attitude toward the use of probabilistic assumptions about data distributions as the required method of the empirical assessment of economic data.

By the 1970s, there was a broad consensus in the profession that inferential methods justified by probability theory—methods of producing estimates, of assessing the reliability of those estimates, and of testing hypotheses—were not only applicable to economic data, but were a necessary part of almost any attempt to generalize on the basis of economic data. . . .

This paper has been concerned with beliefs and practices of economists who wanted to use samples of statistical data as a basis for drawing conclusions about what was true, or probably true, in the world beyond the sample. In this setting, “mechanical objectivity” means employing a set of explicit and detailed rules and procedures to produce conclusions that are objective in the sense that if many different people took the same statistical information, and followed the same rules, they would come to exactly the same conclusions. The trustworthiness of the conclusion depends on the quality of the method. The classical theory of inference is a prime example of this sort of mechanical objectivity.

Porter [*Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*] contrasts mechanical objectivity with an objectivity based on the “expert judgment” of those who analyze data. Expertise is acquired through a sanctioned training process, enhanced by experience, and displayed through a record of work meeting the approval of other experts. One’s faith in the analyst’s conclusions depends on one’s assessment of the quality of his disciplinary expertise and his commitment to the ideal of scientific objectivity. Elmer Working’s method of determining whether measured correlations represented true cause-and-effect relationships involved a good amount of expert judgment. So, too, did Gregg Lewis’s adjustments of the various estimates of the union/non-union wage gap, in light of problems with the data and peculiarities of the times and markets from which they came. Keynes and Persons pushed for a definition of statistical inference that incorporated space for the exercise of expert judgment; what Arthur Goldberger and Lawrence Klein referred to as ‘statistical inference’ had no explicit place for

judgment in making statistical inferences. At the same time, mechanical objectivity was valued—there are many examples of economists of that period employing rule-oriented, replicable procedures for drawing conclusions from economic data. The rejection of the classical theory of inference during this period was simply a rejection of one particular means for achieving mechanical objectivity. By the 1970s, however, this one type of mechanical objectivity had become an almost required part of the process of drawing conclusions from economic data, and was taught to every economics graduate student.

Porter emphasizes the tension between the desire for mechanically objective methods and the belief in the importance of expert judgment in interpreting statistical evidence. This tension can certainly be seen in economists' writings on statistical inference throughout the twentieth century. However, it would be wrong to characterize what happened to statistical inference between the 1940s and the 1970s as a displacement of procedures requiring expert judgment by mechanically objective procedures. In the econometric textbooks published after 1960, explicit instruction on statistical inference was largely limited to instruction in the mechanically objective procedures of the classical theory of inference. It was understood, however, that expert judgment was still an important part of empirical economic analysis, particularly in the specification of the models to be estimated. But the disciplinary knowledge needed for this task was to be taught in other classes, using other textbooks.

And in practice, even after the statistical model had been chosen, the estimates and standard errors calculated, and the hypothesis tests conducted, there was still room to exercise a fair amount of judgment before drawing conclusions from the statistical results. Indeed, as Marcel Boumans (2015, pp. 84–85) emphasizes, no procedure for drawing conclusions from data, no matter how algorithmic or rule bound, can dispense entirely with the need for expert judgment. This fact, though largely unacknowledged in the post-1960s econometrics textbooks, would not be denied or decried by empirical economists of the 1970s or today.

This does not mean, however, that the widespread embrace of the classical theory of inference was simply a change in rhetoric. When application of classical inferential procedures became a necessary part of economists' analyses of statistical data, the results of applying those procedures came to act as constraints on the set of claims that a researcher could credibly make to his peers on the basis of that data. For example, if a regression analysis of sample data yielded a large and positive partial correlation, but the correlation was not "statistically significant," it would simply not be accepted as evidence



assumption required for the model to produce unbiased estimates, the evidence of a relationship would be heavily discounted.

So, as we consider the emergence of the post-1970s consensus on how to draw conclusions from samples of statistical data, there are arguably two things to be explained. First, how did it come about that using a mechanically objective procedure to generalize on the basis of statistical measures went from being a choice determined by the preferences of the analyst to a professional requirement, one that had real consequences for what economists would and would not assert on the basis of a body of statistical evidence? Second, why was it the classical theory of inference that became the required form of mechanical objectivity? . . .

Perhaps searching for an explanation that focuses on the classical theory of inference as a means of achieving mechanical objectivity emphasizes the wrong characteristic of that theory. In contrast to earlier forms of mechanical objectivity used by economists, such as standardized methods of time series decomposition employed since the 1920s, the classical theory of inference is derived from, and justified by, a body of formal mathematics with impeccable credentials: modern probability theory. During a period when the value placed on mathematical expression in economics was increasing, it may have been this feature of the classical theory of inference that increased its perceived value enough to overwhelm long-standing concerns that it was not applicable to economic data. In other words, maybe the chief causes of the profession's embrace of the classical theory of inference are those that drove the broader mathematization of economics, and one should simply look to the literature that explores possible explanations for that phenomenon rather than seeking a special explanation of the embrace of the classical theory of inference.

I would suggest one more factor that might have made the classical theory of inference more attractive to economists in the 1950s and 1960s: the changing needs of pedagogy in graduate economics programs. As I have just argued, since the 1920s, economists have employed both judgment based on expertise and mechanically objective data-processing procedures when generalizing from economic data. One important difference between these two modes of analysis is how they are taught and learned. The classical theory of inference as used by economists can be taught to many students simultaneously as a set of rules and procedures, recorded in a textbook and applicable to "data" in general. This is in contrast to the judgment-based reasoning that combines knowledge of statistical methods with knowledge of the circumstances under which the particular data being analyzed were generated. This form of reasoning



the supervision of an experienced empirical researcher.

During the 1950s and 1960s, the ratio of PhD candidates to senior faculty in PhD-granting programs was increasing rapidly. One consequence of this, I suspect, was that experienced empirical economists had less time to devote to providing each interested student with individualized feedback on his attempts to analyze data, so that relatively more of a student's training in empirical economics came in an econometrics classroom, using a book that taught statistical inference as the application of classical inference procedures. As training in empirical economics came more and more to be classroom training, competence in empirical economics came more and more to mean mastery of the mechanically objective techniques taught in the econometrics classroom, a competence displayed to others by application of those techniques. Less time in the training process being spent on judgment-based procedures for interpreting statistical results meant fewer researchers using such procedures, or looking for them when evaluating the work of others.

This process, if indeed it happened, would not explain why the classical theory of inference was the particular mechanically objective method that came to dominate classroom training in econometrics; for that, I would again point to the classical theory's link to a general and mathematically formalistic theory. But it does help to explain why the application of mechanically objective procedures came to be regarded as a necessary means of determining the reliability of a set of statistical measures and the extent to which they provided evidence for assertions about reality. This conjecture fits in with a larger possibility that I believe is worth further exploration: that is, that the changing nature of graduate education in economics might sometimes be a cause as well as a consequence of changing research practices in economics. (pp. 167-70)

The correspondence between Biddle's discussion of the change in the attitude of the economics profession about how inferences should be drawn from data about empirical relationships is strikingly similar to Oakeshott's discussion and depressing in its implications for the decline of expert judgment by economics, expert judgment having been replaced by mechanical and technical knowledge that can be objectively summarized in the form of rules or tests for statistical significance, itself an entirely arbitrary convention lacking any logical, or self-evident, justification.

But my point is not to condemn using rules derived from classical probability theory to assess the significance of relationships statistically estimated from historical data, but to challenge the methodological prohibition against the kinds of expert judgments that many statistically knowledgeable economists like Nobel Prize winners such as Simon Kuznets, Milton Friedman, Theodore Schultz and Garv Becker routinely used to make in their empirical studies. As Biddle notes:

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probability theory as well as anyone in the profession in the 1950s, and he used statistical theory to derive testable hypotheses from his economic model: hypotheses about the relationships between estimates of the marginal propensity to consume for different groups and from different types of data. But one will search his book almost in vain for applications of the classical methods of inference. Six years later, Friedman and Anna Schwartz published their *Monetary History of the United States*, a work packed with graphs and tables of statistical data, as well as numerous generalizations based on that data. But the book contains no classical hypothesis tests, no confidence intervals, no reports of statistical significance or insignificance, and only a handful of regressions. (p. 164)

Friedman's work on the *Monetary History* is still regarded as authoritative. My own view is that much of the *Monetary History* was either wrong or misleading. But my quarrel with the *Monetary History* mainly pertains to the era in which the US was on the gold standard, inasmuch as Friedman simply did not understand how the gold standard worked, either in theory or in practice, as McCloskey and Zecher showed in two important papers ([here](#) and [here](#)). Also see my posts about the empirical mistakes in the *Monetary History* ([here](#) and [here](#)). But Friedman's problem was bad monetary theory, not bad empirical technique.

Friedman's theoretical misunderstandings have no relationship to the misguided prohibition against doing quantitative empirical research without obeying the arbitrary methodological requirement that statistical be derived in a way that measures the statistical significance of the estimated relationships. These methodological requirements have been adopted to support a self-defeating pretense to scientific rigor, necessitating the use of relatively advanced mathematical techniques to perform quantitative empirical research. The methodological requirements for measuring statistical relationships were never actually shown to be generate more accurate or reliable statistical results than those derived from the less technically advanced, but in some respects more economically sophisticated, techniques that have almost totally been displaced. One more example of the fallacy that there is but one technique of research that ensures the discovery of truth, a mistake even Popper was never guilty of.

### **Methodological Prescriptions Go from Bad to Worse**

The methodological requirement for the use of formal tests of statistical significance before any quantitative statistical estimate could be credited was a prelude, though it would be a stretch to link them causally, to another and more insidious form of methodological tyrannizing: the insistence that any macroeconomic model be derived from explicit micro-foundations based on the solution of an intertemporal-optimization exercise. Of course, the idea that such a model was in any way micro-founded was a pretense, the solution being derived only through the fiction of a single representative agent, rendering the entire optimization exercise fundamentally illegitimate and the exact opposite of micro-founded model. Having already explained in previous posts why transforming

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of the danger of elevating technique over practice and substance.

### Popper's More Important Contribution

This post has largely concurred with the negative assessment of Popper's work registered by Lemoine. But I wish to end on a positive note, because I have learned a great deal from Popper, and even if he is overrated as a philosopher of science, he undoubtedly deserves great credit for suggesting falsifiability as the criterion by which to distinguish between science and metaphysics. Even if that criterion does not hold up, or holds up only when qualified to a greater extent than Popper admitted, Popper made a hugely important contribution by demolishing the startling claim of the Logical Positivists who in the 1920s and 1930s argued that only statements that can be empirically verified through direct or indirect observation have meaning, all other statements being meaningless or nonsensical. That position itself now seems to verge on the nonsensical. But at the time many of the world's leading philosophers, including Ludwig Wittgenstein, no less, seemed to accept that remarkable view.

Thus, Popper's demarcation between science and metaphysics had a two-fold significance. First, that it is not verifiability, but falsifiability, that distinguishes science from metaphysics. That's the contribution for which Popper is usually remembered now. But it was really the other aspect of his contribution that was more significant: that even metaphysical, non-scientific, statements can be meaningful. According to the Logical Positivists, unless you are talking about something that can be empirically verified, you are talking nonsense. In other words they were deliberately hoisting themselves on their petard, because their discussions about what is and what is not meaningful, being discussions about concepts, not empirically verifiable objects, were themselves – on the Positivists' own criterion of meaning — meaningless and nonsensical.

Popper made the world safe for metaphysics, and the world is a better place as a result. Science is a wonderful enterprise, rewarding for its own sake and because it contributes to the well-being of many millions of human beings, though like many other human endeavors, it can also have unintended and unfortunate consequences. But metaphysics, because it was used as a term of abuse by the Positivists, is still, too often, used as an epithet. It shouldn't be.

Certainly economists should aspire to tease out whatever empirical implications they can from their theories. But that doesn't mean that an economic theory with no falsifiable implications is useless, a judgment whereby Mark Blaug declared general equilibrium theory to be unscientific and useless, a judgment that I don't think has stood the test of time. And even if general equilibrium theory is simply metaphysical, my response would be: so what? It could still serve as a source of inspiration and insight to us in framing other theories that may have falsifiable implications. And even if, in its current form, a theory has no empirical content, there is always the possibility that, through further discussion, critical analysis and creative thought, empirically falsifiable implications may yet become apparent.

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