The Classics of Western Philosophy

A Reader's Guide

Edited by

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Karl Popper, *The Logic of Scientific Discovery* (1934)

Not Logic but Decision Procedure

Mariam Thalos

In his *magnum opus*, *The Logic of Scientific Discovery* (first published in German in 1934, English translation, 1959), Karl Popper (1902–1994) makes two fundamental philosophical moves. First, he relocates the center of gravity of the philosophical treatment of science around what he calls the *problem of demarcation*. This is the problem of distinguishing between science, on the one hand, and everything else on the other. (By contrast, his contemporaries of the Vienna Circle, whose positivism would prove the most influential brand of empiricism of the day, located the center of gravity around the problem of linguistic meaning, and used a criterion according to which a statement is meaningful to the extent that one can identify verification conditions for it.) Popper excludes from science such things as logic, metaphysics, Freudian psychoanalysis, and Marx’s theory of history.

Second, Popper propounds the doctrine of falsificationism, which handles the problem of demarcation, as well as answers David Hume’s shattering attack on science as the premier form of knowledge centuries before. The arguments he mounts for falsificationism would function also as an attack on any account of the scientific enterprise that, like positivism, adheres to the idea that science progresses logically from instances (given in observation or experience) to the high-order generalizations characteristic of mature scientific theory.

In this small space I shall undertake neither to illuminate the nuances of Popper’s position, nor to trace the (numerous) lines of criticism that have accumulated against it some seventy years later. I shall busy myself instead with tracing a trajectory of thought on the subject of scientific *reasoning* and its relation to individual *decision-making*, reflecting on Popper’s contribution and on how his legacy might be further enlarged.

**Reasoning**

Hume, like many of his contemporaries and predecessors, divided reasoning into two kinds: theoretical reasoning and practical reasoning. Practical reasoning has the func-
ion of controlling action or decision: its point is to figure out what to do. This is contrasted with theoretical reasoning, whose work is to figure out how things stand in the world, rather than what to do about them. Hume and his contemporaries held, moreover, that theoretical and practical reasoning operate independently of one another. This (if true) preserves the impartiality of theoretical reasoning, by way of ensuring that the opinions we hold as to how things stand in the world are not influenced by how we might wish things stood. And this, in turn, guarantees that science is never compromised by wishful thinking.

There is agreement between Hume’s empiricist camp and Immanuel Kant’s counter-empiricist camp on the subject of having to divide, and subsequently to separate in their operations, the functions of theoretical and practical reasoning. But there is disagreement between them about how each form of reasoning proceeds. For instance, Kant and his followers believed that practical reasoning itself has to be subdivided further, into moral reasoning (which has the office of figuring out what matters - what is worth wanting) and the instrumental reasoning that figures out how to achieve it, whilst Hume’s followers held that practical reasoning is means-ends reasoning through and through. To this day Humans maintain that there is no place in practical reasoning for figuring out what is worth wanting, and no point anyway, since one is in no position to decide to want; one either does or doesn’t, and deciding simply can’t swing things one way rather than another.

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Science and Decision

In no era of Western philosophy before the twentieth century have empiricists tolerated in scientific reasoning (or any academic subject) a garden-variety decision pro-
cess, such as is occasioned when someone or some group is deciding where to go for lunch. Positivists are no exception: they sought a logic for the advancement of knowledge, not a decision procedure. Their thought was that science — as the premier form of knowledge — would grow exactly in proportion to the growth in the number, variety, and precision of empirical observations. Popper was to be the antidote to their brand of optimism.

First, Popper argued that the positivists’ division between meaningful statements (among which are the scientific ones) and everything else (which of course would then deserve being called nonsense) is just arbitrary, resting as it does on an arbitrary — and thus itself unsound — decision about what to regard as meaningful, namely their verificationist criterion. Popper then argued that observation alone could not advance a true scientific hypothesis ahead of its competitors, since any given body of observations will be consistent with numerous and mutually incompatible bodies of theory. What is needed is an account of how an idea, however initially outrageous, can grow into something that takes a rightful place amongst the ranks of dignified scientific theories, to the point of overwhelming older and initially better-regarded theories. Such a thing can occur only if there exists a (unique) scientific method for testing one theory against another. That method, says Popper, is the method of falsification, which also marks the difference between science and other things. A theory is scientific to the extent that it excludes or prohibits certain possibilities that are in principle observable (and is ipso facto falsifiable). And it is corroborated to the extent that it survives tests aimed at falsifying it.

This position would appear to commit Popper to a certain doctrine, also espoused by the positivists: to wit, the doctrine that observation is in some sense primary or fundamental — at least that it is independently distinguishable from scientific theory as such. But Popper does not seem happy with such a commitment. Like those in Kant’s camp (for example N. R. Hanson and the later Ludwig Wittgenstein), Popper repudiates the view that observation is either infallible or foundational. He argues that they are not mere reports of sensations passively registered, but are instead descriptions of what is observed as interpreted in the light of a theoretical framework. And this is what he means when he says that perception is an active process, in which the mind assimilates data against a backdrop of theory — that observation is therefore theory-laden. Accordingly, he asserts that statements of observation are open-ended hypotheses: they are not a function of experience alone, nor can they be verified by experience as such. But if this is true, then how can a scientific theory be falsified? How can it even be testable, as it must be if it is even to qualify as scientific? For if it is a matter of judgment, and not of simple fact, whether an observation sentence is true, where is the objectivity in the test? Here now is Popper’s twist.

Popper states that acceptance of observation statements is “prompted” by experience, but not determined by it. Therefore observation statements “are accepted as the result of a decision or agreement, and to that extent they are conventions. The decisions are reached in accordance with a procedure governed by rules” (Popper, 1959: 106). All the same, they are free decisions; we have a genuine choice:

From a logical point of view, the testing of a theory depends upon basic statements whose acceptance or rejection, in its turn, depends upon our decisions. Thus it is decisions which
deciding where to go for advancement of knowledge – as the premier form of knowledge – and meaningful, namely their meaningful purpose being the antidote to their own meaning. Statements of course would only be on an arbitrary – and meaningful, namely the assumption of universal statements but that, on the contrary, it enters into our acceptance of the singular statements – that is, the basic statements. (1959: 108–9)

Popper is very aware of the move he is making; he is saying that scientific reasoning is decision-making all the way down. Even the method of testing, while distinguishing science from non-science, still involves a form of decision-making. Now does this view obviate the distinction between practical and theoretical decision-making? And where, in the end, is there room for the sort of open-ended criticism, and the decisive, or at least genuine, testing of hypotheses against evidence, that Popper so venerates? To the end of his life Popper had no satisfactory answer.

Some of Popper’s successors hold that once the potential for genuine, bona fide falsifiability is gone, there is no longer any room for justification of any scientific methodology, in the strict objective sense insisted upon by Popper. For example, Thomas Kuhn and his followers too speak of maintaining and overthrowing scientific theories (Kuhn, 1962). But Kuhnians hold that, when it comes to the question of deciding between competing scientific hypotheses, the affair is social, subject to sociological laws, and not the simple and dignified matter of checking (albeit piecemeal) against the gold standard of evidence. So evidence, in and of itself, can never play the role of absolute and final arbiter, as Popper would have liked. The scientific enterprise, on this view, becomes indistinguishable from the putatively pseudo- or non-scientific. The only standards there can be are the inconstant, shifting standards of (as they might put it) “professional practice.” There are no now-and-forever methodologies by means of which science can expect to grow by simple accretion.

This is the doctrine of naturalized epistemology – that there are no now-and-forever, sure-fire methodologies for increasing or refining knowledge of the world. It is nowadays pervasive, and Popper, much as he should have fought it, seemed incapable even of resisting it. Indeed he called himself a naturalist, and was content to acquiesce in some of the naturalist’s favorite axioms. For example, he acquiesced in the idea that we should learn about scientific methodology from the history of science. He was fond of using episodes in the history of science himself to reinforce his falsificationist proposal, and that way blurred the distinction between the context of discovery and the context of justification.

**Epistemology: the Individual and the Citizenry**

Sometimes a certain contrast is drawn between Popper and Kuhn. Popper, some people have said, is a staunch defender of scientific Reason with a capital R, whilst Kuhn is not. This assessment is too simplistic, as Kuhn himself points out (in Lakatos and Musgrave, 1970: 1–23). After all, it is not at all clear how precisely Popper shall sustain a place for the critical objectivity he holds in such high regard, once one
appreciates his position on the nature of evidence and observation. Both Kuhn and Popper acknowledge the role of decision throughout scientific practice, and both ascribe a methodology, or at least a pattern, to scientific activity. To be sure they do not see entirely eye to eye on what the pattern of scientific change looks like in the real-life history of science, or on whether there is genuine advancement in science. (The rather minor differences here are two: (1) Popper does not acknowledge a difference between what Kuhn terms “normal science” and “scientific revolution,” whereas Kuhn himself makes much of that difference; and (2) Popper is more on the side of genuine advancement than Kuhn.) Even so, there is a substantive difference between Popper’s position and Kuhn’s.

That difference lies in their starting positions on the subject of the relation between the history of science and the status of proper scientific methodology. Popper’s position is more enduring, and it holds the possibility for a host of developments that Popper himself was not in a position to envision. He can (though he did not himself, possibly for political reasons that I shall not go into here) defend rather well against criticism alleging that he has no logical space for open-ended criticism and objective testing of theory by evidence.

Kuhn begins by examining various episodes in the history of science, and concludes that science does not progress by accretion. Theories, he tells us, are overthrown rather than built upon. He makes no antecedent separation between the subject matters of discovery and justification, and is content, when he fails to find a pattern of growth in the transmitted body of theory, or a pattern to the succession of superseded bodies of theory, to dismiss the idea of now-and-forever methodology entirely. It is not at all clear what sorts of conclusions about matters of methodology he is entitled to draw from the historical and sociological data he amasses. Popper, by contrast, begins (like his positivistic predecessors) with a crisp distinction between the question of methodology and the question of discovery. And there is a path that leads from here to a defense against criticism that alleges no room in his system for objectivity.

Let us call the view that a decision as to what theory to accept (or recommend, or place in high regard, or what have you) is just another practical matter, pragmatism. It is the view that the imperatives of science fall under the imperatives of practical life, as a special category. Pragmatism does not distinguish between the decisions of a collective and the decisions of individuals. Now Popper, by acquiescing in the idea that experimentation and observation in science are just more decision-making, adheres to or at least leans in the direction of pragmatism as we have just defined it. And thus he becomes vulnerable to criticism that his methodology is not objective.

But it need not have gone this way. Popper could have acquiesced in the idea that observation by individuals is an ordinary matter of individual decision, and thus subject to theoretical bias. But he could have remained firm in the idea that decisions vis-à-vis a body of evidence, assembled and preserved by citizens acting, not with personal aims for personal gain, but rather as faithful public officers, are not like everyday personal matters of decision. He could have insisted that decisions of that sort are not similarly subject to such bias because they are subject instead to canons of collective decision, whose standards transcend those of personal decision. Thus Popper could have made effective use of a distinction between individual and collective reasoning, in a way that would have set him apart from all his predecessors. And this move could
have rehabilitated and indeed sharpened, rather than blurred, the difference between justifying one theory in preference to another, in relation to a body of evidence, and merely overthrowing the latter in favor of the former.

Now, of course, such a move, if someone were to make it, would require giving a normative account of collective decision, and decision *vis-à-vis* theory acceptance in particular. No such account, or anything remotely like it, exists at this time. But surely the time is now ripe. Perhaps someone will insist that this move is *ad hoc*. And why in particular should the falsificationist be entitled to it? For surely it is available even to the inductivist. There are very good reasons for viewing this move as especially suited to the Popperian position, and far from being *ad hoc*. These reasons are rooted in what we can anticipate a normative account of collective reasoning to look like. In particular we have very good reasons for thinking that an account of collective reasoning *vis-à-vis* theory acceptance (or anything else) could not take the form of a logic, but instead must take the shape of a decision procedure. Hence such an account would fit quite naturally into a Popperian system consisting of decision-making all the way down. Moreover, that decision procedure will by necessity look very much like the criticism-friendly one suggested by Popper. Among many-membered decision-making bodies, the most functional, and certainly the most democratic (in a broad sense), govern themselves by something like Robert's Rules of Order. These rules come quite close to the procedure of critical evaluation Popper esteems. And nothing even remotely like a logic (in the inference-drawing sense) exists for governing the proceedings of any group. This would suggest, though of course it does not prove decisively, that any decision procedure worthy of the name of science shall incorporate just the sort of critical examination of hypotheses and evidence that Popper might have sought.

**Bringing it up to Date**

There is a revival in the inductivist church that has come with the rise of naturalized epistemology. Bayesianism – made possible by certain advances in the application of probability theory – is a marriage between contemporary decision theory, and Bishop Butler's much-admired dictum (endorsed by nearly everybody) that probability is the very guide of life. Very roughly, Bayesians acknowledge that the question for a scientific methodology to address is not "which theory shall we accept (or attach high probabilities to)?" but instead "how must we proceed – in as mechanical a way as possible – from one set of (given) opinions to another, with each new observation, as responsible empiricists ought to do?" And they leave the arrival at a theory for the initial application of their principles as a matter of personal taste. The algorithms they propose for massaging probabilities are perhaps as much in the way of a *logic* as one can hope for. Still, their view accommodates quite well Popper's original idea that everything rests on a bedrock of decision.

The neo-Popperian, who embraces the distinction between the individual and the citizenry that I have sketched, will of necessity tangle with the Bayesian. Their struggle (in the twenty-first century) will be over the role of evidence in the *formation* of scientific theory – with the Popperians having the cloud of history's weightiest opinions on their side.
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