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REVIEW

Barry Gower, *Scientific Method: An Historical and Philosophical Introduction*, (London: Routledge, 1996), pp. 276, £12.99.

Two things seem to make science different from other human activities: the existence of a special method and the claim that this method produces objective knowledge of the world. Yet, as Barry Gower's impressive book shows, after centuries of philosophical reflection on scientific method, there is considerable disagreement as to what *exactly* this method is. What is more interesting is that all attempts to characterise scientific method, from Galileo and Descartes up until the present, suffer from an internal tension: whatever the method of science be in its details, it should satisfy two general desiderata which, at least prima facie, pull in contrary directions. On the one hand, it should be *ampliative*: it should be able to move from the finite data and observations available at any given time to hypotheses and theories which go far beyond these data, either by generalising them over unexamined (or even unexaminable) domains or by introducing unobserved and unobservable causes which bring the phenomena about. This 'content-increasing' aspect of scientific method is indispensable, if science is seen as an activity which purports to extend our knowledge beyond what is immediately observed by means of the senses. On the other hand, the method of science should be *epistemically probative*: it should be able to convey epistemic warrant to its conclusions (hypotheses and theories). Otherwise, its claim to extending our knowledge of the world beyond what is actually observed is dubious. The tension arises because ampliative methods don't carry their epistemically probative character on their sleeves. Since the conclusion of an ampliative method can be false, even though all of its premises are true, the following question arises: what makes it the case that the method conveys whatever epistemic warrant the premises enjoy to the intended conclusion rather than to its negation?

Sceptics point out that the defender of the rationality of scientific methodology should rely on some *substantive and contingent assumptions* (e.g., that the world has a natural-kind structure, or that the world is governed by universal regularities, or that observable phenomena have unobservable causes, etc.) in order to show that, although scientific methodology is fallible, it is nonetheless reliable and does confer epistemic warrant to its outcomes. But, the sceptic goes on, what else, other than ampliative reasoning itself, can possibly establish that these substantive and contingent assumptions are true of the world? Arguing in a circle, the sceptic notes, is inevitable and this simply means, he concludes, that the alleged defence of scientific method carries no rational compulsion with it.

Gower does not systematically engage with the sceptical challenge. Instead, he devotes the chapters of his book to discuss in great detail how the various philosophical accounts of scientific method have tried to characterise the shape that scientific method should take in order to minimise the tension between its ampliative nature and the need to be epistemically probative. I think this is the right approach. What needs to be done is to identify as precisely as possible the substantive assumptions that need to be made in an attempt to strike a balance between the two desiderata of scientific method. Once the assumptions are identified, they can and should be scrutinised. In any case, what one should show is not that scientific methodology can reliably operate in all possible worlds, but rather how it can operate reliably to those possible worlds which have the same nomological structure as the actual world. It is also worth adding, however, that the sceptical challenge is far from intuitive. It itself relies on a substantive *epistemic* assumption: that any defence of ampliative but epistemically probative methods should depend on no substantive and contingent assumptions whose truth cannot be established by independent means. Hence, the sceptical challenge is itself subject to criticism. For instance, if the sceptical assumption is accepted, no ampliative reasoning can ever be epistemically probative. (Unless, of course one admits to the existence of a synthetic a priori justification of ampliative reasoning.) But equally, if the sceptical assumption is accepted, even the possibility of epistemically probative demonstrative reasoning becomes dubious. For truth-transmission, even if it is guaranteed by demonstrative reasoning, requires some truths to start with. Yet, the truth of any substantive claims that feature in the premises of a demonstrative argument can only be established by ampliative reasoning, and hence it is equally open to the sceptical challenge. The sceptical challenge is not incoherent. But if its central assumption is taken seriously, then what is endangered is the very possibility of any kind of learning from experience.

Put in a different way, the problem faced by a philosophical investigation into scientific method is to show how an abstract model of scientific method can be constructed which is both austere enough to extrapolate only what is warranted in the light of evidence *and* strong enough to aspire to provide knowledge of causes (cf. Gower, p. 73). There are two extremes that will not do, but whose identification will help us figure out what we should look for. One is to endorse as the method of science a crude 'method of hypothesis', where a hypothesis is accepted as probably true on the basis of the fact that it entails all available relevant evidence. The *prima facie* attraction of this method is that it is content-increasing in a, so to speak, 'vertical way': it allows the generation of hypotheses about the, typically unobservable, causes of the phenomena. But what we gain in strength we lose in austerity. For, a crude 'method of hypothesis' is epistemically too permissive since there are, typically, more than one (mutually incompatible) hypothesis which entail the very same evidence. If a crude 'method of hypothesis' were to license any of them as probably true, it would also have to license all of them as probably true, which is absurd. The crude 'method of hypothesis' simply lacks the discriminatory power that scientific method ought to have. The other extreme is to endorse simple enumerative induction, or the 'more-of-the-same' rule, as the method of science. The *prima facie* advantage of this option is that it is content-increasing in a, so to speak, 'horizontal way': it allows the generation of generalisations based on observed evidence in a way that stays close to what has been actually observed. But what we gain in austerity we lose in strength. Enumerative induction is too restrictive. For, even if some substantive assumptions about universal and projectible regularities were in place, enumerative induction could not possibly yield any hypothesis about the causes of the phenomena. Conclusions which state generalisations are necessarily couched in the vocabulary of the premises. Hence, they cannot legitimately introduce causes whose descriptions go beyond the expressive power of the premises (e.g. by reference to unobservable entities).

Consequently, the correct account of scientific method should lie somewhere between these two extremes. But can there be such an account? Gower offers a detailed and broad survey of the several attempts to create such an account: from the Newtonian "deduction from the phenomena" and the Millian methods of "agreement and difference" (which lie more towards the austerity side) to Whewell's "consilience of inductions" and the Peircean "abduction" (which lie more towards the strength side). But the attempted reconciliation is a far from trivial issue as the Mill-Whewell debate makes clear (chapter 6). The basic idea is that scientific method should move from effects to causes by a process which guarantees that all but one potential causal explanations of the effects are eliminated, while it conveys epistemic warrant to the sole survivor. Proposing the method of "direct induction", Mill thought that scientific method should employ some substantive assumptions which, together with the phenomena to be explained, entail the correct causal explanation. So, said Mill, given a) that effects have causes, b) that the cause is necessary and sufficient for its effect, and c) that we have a complete list of potential causes of an effect, we can use his "method of difference" in order to deduce *the* cause of a certain effect from (a) to (c) above and the fact that one and only one of the potential causes is present when the effect is present and absent when the effect is absent. As Gower correctly points out (p. 123), the problem with this approach is not that it relies on substantive assumptions, for Mill did not want to dispel "philosophical doubts" about scientific method, but rather the "practical doubt" that scientific method cannot deal with the existence of more than one potential causal explanations of the phenomena (let's call that 'the multiple potential explanations problem'). But, as Gower also notes, Mill's target was not just the crude version of the method of hypothesis. He wanted to attack the legitimacy of the rival substantive assumption which featured in Whewell's more sophisticated view, viz., that elimination of rival hypotheses can and should be based on *explanatory considerations.*

Whewell thought that the key to ranking a hypothesis ahead of extant competitors and, eventually, the key to the rational acceptance of a hypothesis lies in the explanatory power of the hypothesis, as this is marked by the capacity to yield novel predictions and to unify hitherto unrelated domains of the phenomena. This is, in essence, what Whewell called the "consilience of inductions". Suppose that a hypothesis unifies a set of known phenomena, and that it also predicts new types of phenomena, which did not belong to its original scope and hence could not be part of the reasons for advancing this hypothesis. Taking this hypothesis to be false would not be contradictory, but it would be unreasonable.

For faced with the choice between a coincidence-based explanation ('It is just a coincidence that the hypothesis unifies the phenomena and entails the novel predictions, although it is essentially false.') and the fully worked out causal-explanatory story told by the hypothesis, it would be unreasonable to accept the former. So, Whewell's point against Mill was that a suitably sophisticated 'method of hypothesis', where a hypothesis is accepted as probably true on the basis of its unifying power and its capacity to entail novel predictions, can and should be epistemically probative. Interestingly enough, Mill rejected Whewell's sophisticated version of the method of hypothesis on the grounds that it is *possible* that there can be another, hitherto unknown, hypothesis which explains the evidence equally well. But if sound, Mill's reason undermines his own method no less. For it is also *possible* that we are mistaken in thinking that we have exhausted all the possible potential causes when we apply Mill's method of difference. The difference between Mill and Whewell was precisely over the role of substantive explanatory considerations in scientific method. A suitably amended method of hypothesis seems able to deal with the problem of 'multiple potential explanations'. But is it austere enough? Mill thought (mistakenly, I think) that it isn't, whereas Whewell thought that it is. But the debate still goes on.

Perhaps the most distinctive feature of Gower's book is his extensive discussion of the role of probability in scientific method. What is particularly worth noting is Gower's admirably clear presentation of Thomas Bayes' contribution (chapter 5) and of the Keynes-Ramsey debate (chapter 9). Ever since Leibniz, the Bernoullis and Bayes, a central thesis has been that an appeal to the probability calculus can make scientific method issue in both strong and epistemically warranted beliefs. For hypotheses, be they about unobservable causes or about universal regularities, can enjoy evidential support in the sense that their probabilities of being true can be raised by the evidence. Add to this the thesis that the probability of a hypothesis given the evidence should reflect the degree of certainty (or confidence) with which a reasonable person should believe the hypothesis, and you get reasonable degrees of belief in strong hypotheses. The problem, however, is that, as Gower repeatedly emphasises, in order for the evidence to influence the probability of a hypothesis, the hypothesis should be given some initial or prior probability of being true, that is, a certain probability prior to the evidence being taken into account.

What exactly determines the initial probability of a hypothesis? One thing is certain: if the prior probability of a hypothesis is either zero or one, then no further evidence whatsoever can influence it. Hence, in order for the probabilistic approach to get off the ground, the initial probability distribution should *not* be dogmatic. This gives us a nice way to identify both the sceptic and the fool. The sceptic is someone who gives all hypotheses about generalisations or causes zero prior probabilities, while the fool is someone who gives all hypotheses about generalisations or causes prior probability one. But even if their attitudes are dismissed as unreasonable, since, by default they forbid learning from experience, we are still left with the problem of how to specify non-extreme prior probabilities. What judgements should guide the assignment of non-extreme priors? This question is far from trivial because, as standardly understood, the rationality of scientific method goes hand in hand with its objectivity. If there is no objective way to assign prior probabilities to hypotheses, then the rationality of scientific method is in danger. For the degree of support of a hypothesis which entails the evidence is a function of its prior probability (as Bayes's theorem makes plain). Hence, if there is no objective way to specify the priors, there is no objective way to solve the 'multiple explanations problem'. If prior probabilities merely express the individual scientist's personal degrees of belief, then two mutually incompatible hypotheses may end up being both well-supported by the same evidence simply because their respective proponents started off with high subjective prior degrees of beliefs. In chapters 9 to 11, Gower discusses thoroughly the notorious difficulties faced by all attempts to think of prior probabilities as rational degrees of belief (be they logical, as in Carnap's and Keynes's cases, or factual-frequentist, as in Peirce's and Reichenbach's cases).

But the alternative, subjective Bayesian, approach is no less troublesome. To be sure, subjective Bayesians change the agenda of the philosophical discussions about scientific method. They think of the rationality of scientific method as a purely *structural* concept. What matters, they argue, is not whether the beliefs issued by scientific method are likely to be true, but rather how the degrees of these beliefs—whatever their content be—hang together at a certain time and how they get updated over time. As is well-known, subjective Bayesians think of rationality as *probabilistic coherence*: a set of degrees of belief is rational if and only if they satisfy the axioms of the probability calculus. Which leaves us with a problem: for some of us belief in, for instance, 'creationist biology' is less rational than belief in evolutionary biology. But from a Bayesian perspective, insofar as each of two mutually inconsistent sets of degrees of belief hangs together in the appropriate way, there is no further fact of the matter as to which set is more rational than the other. Gower ends his own qualified defence of Bayesianism in chapter 11 with the note that if we reject Bayesianism we are left with no other account "which has so many of Bayesianism's advantages and so few of its disadvantages" (p. 233). I beg to differ. I think that a sound account of scientific methodology should be able to show how the evidence can render some beliefs (or hypotheses) objectively more warranted than others. Hence, I think that methodologists should still look for an account of scientific method which does not minimise the original tension between its ampliative character and the need to be epistemically probative by compromising the objectivity of scientific method. A line that, perhaps, needs to be further explored relates to ways in which the explanatory considerations that Whewell favoured in the Mill-Whewell debate should determine rational prior probabilities. Be that as it may, Gower's book is thought-provoking and very well argued. No-one seriously interested in the philosophical debates about scientific method should miss it.

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