Madness in the method

The biologist Peter Medawar didn't believe that most scientists have a clear conception of the scientific method. Ask one what he thinks it is, he once commented, and "he will adopt an expression that is at once solemn and shifty-eyed: solemn because he feels he ought to declare an opinion; shifty-eyed because he is wondering how to conceal the fact that he has no opinion to declare."

I've always enjoyed this comment, even though I find it hard to believe. Most scientists, I suspect, even without shifty eyes, would readily offer up their take on the scientific method, at least of their own practice and how it contributes to making legitimate scientific knowledge. The scientific method, most would say, depends on a commitment to contact with empirical reality, either through controlled experiments or careful observations. Most would also insist that wide-ranging speculative theory is also necessary, as we can only find the right ideas by generating many, and discarding those found wanting.

In this regard, many scientists might also mention Karl Popper's well-known concept of falsification — that science proceeds not by demonstrating any theory to be true, only by showing some to be untrue; we move towards truth, paradoxically, only by demonstrating untruth. Popper's ideas don't seem to be very popular with philosophers of science, even if they make sense to practising scientists.

But to give the philosophers their due, they have shown beyond any doubt that none of this is as tidy as it seems. It's naive, for example, to suppose that theories get rejected when they meet damning empirical evidence. More often, scientists reject the evidence, or the theory mutates so as to fit the new data. And there's some sense in this. After all, experiments can be as incorrect as theories.

Also, as the philosopher Michael Polanyi once argued, there has to be some kind of theoretical inertia in the system, which maintains conceptual consistency in ongoing work:

"There must be at all times a predominant accepted scientific view of the nature of things… A strong presumption… must prevail… that any evidence which contradicts this view is invalid. Such evidence has to be disregarded, even if it cannot be accounted for, in the hope that it will turn out to be false or irrelevant."

Is there a scientific method, some simple, verbal way to wrap up the recipe of scientific success?

So is there a scientific method, some simple, verbal way to wrap up the recipe of scientific success? Vigorous arguments over this topic continue, with little sign of any emerging consensus. But I wonder if part of the problem isn't that some expect too much from a scientific method, with emphasis on the word 'method'.

In particular, the philosophical attitude generally looks for a scientific method that would be akin to an algorithm, and aims to prove that this algorithm will always lead to knowledge, rather than something else. This way of thinking comes out of the tradition of mathematics, as a hangover from Euclid and Plato and a desire to achieve certainty by building up a world from pure logic. Our philosophy hasn't yet become sufficiently biological, tolerant of uncertainty and disorder and fallibility, and of methods that can't be wholly defined but work anyway, at least much of the time.

In this spirit, physicist Byron Jennings, of the TRIUMF laboratory for nuclear and particle physics in Canada, recently suggested the following pragmatic definition:

"Science is the construction of parsimonious, internally consistent models that can reliably predict future observations."

Scientific method, it follows, is almost anything, even the exploitation of the most opportunistic mistake, that leads to the creation of such models, which I think makes good sense. Jennings also argues that Popper had it wrong: observations don't prove or disprove models, but give relevant information that lets us establish a ranking between them. New models displace older models, or at least have a tendency to do so, if they predict observations more effectively.

Of course, we also like simpler, rather than more complex models. Why? Is this only a matter of aesthetics, as Dirac might have had it? Jennings suggests a much less mysterious explanation: it is simply that science aims to work with models that can be tested. We simplify models because we seek to remove all elements that aren't actually necessary to making it work. We remove untestable components, which has the additional bonus of making models more transparent, easier to use — and more instructive.

This may sound a little like logical positivism, which early thinkers in the twentieth century, carried away by the apparent philosophical weirdness of quantum physics, took to absurd extremes. But one needn't be immediately ruthless about what seems unobservable, and science often proceeds slowly for good reason. As the American physicist David Bohm argued quite convincingly, temporarily unobservable concepts often have a very useful role in a theory because they help scientists develop its content, act as inspiration for further hypotheses, and therefore increase its ability to predict — and may even lead science to eventual evidence favouring the existence of those entities themselves. Quarks, invented initially for purely mathematical reasons, offer a salient example.

None of this will satisfy the philosophers, even if it sounds entirely sensible to most physicists. The ultimate question, of course, is whether this way of seeing science as pragmatic, efficient model building to predict observations can help us distinguish good science from bad science, pseudoscience or superstition? The answer, Jennings suggests, isn't really surprising — it simply has to do with prediction and making successful contact with empirical reality. Successful predictions always mark out science, whether predictions of future events from past, or of relationships among unobserved data on the basis of what has already been observed.

What Medawar once said about scientists and the scientific method may not be true, but he was right that scientists don't just follow some algorithm. Science is a messy business. To my mind, the method of science is less likely to resemble a mathematical algorithm, which can be proven sound, than the working process of a biological organism — complex, mired in history, cobbled together as a patchwork of mechanisms, which together make errors continually, but nevertheless do an extraordinary job of coping in an uncertain world. ❒

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