Forum

Historical science, experimental science, and the scientific method: Comment and Reply

COMMENT

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Carol E. Cleland tackles an important issue in her analysis of the alleged inferiority of historical science compared to experimental science. Yet, her method of analysis and the conclusions she draws deserve comment.

First, the asymmetry between past and future is intriguing, but what significance does it have in this context? Cleland claims this asymmetry not only explains, but actually demands, that historical science looks for confirmation while experimental science looks for refutation. Yet, whether an event is historical or the outcome of a controlled experiment, its effects appear afterward. Both historical events and the controlled experiments have a context before, during, and afterward. It is true that in experimental science the context is under an experimenter's control, while nature supplies it for a historical event; but what matters in either case is that the evidence gathered is sufficient to specify context and resolve among alternative hypotheses. This involves matters such as experiment design, verifying the conditions surrounding an experiment, and quantifying the uncertainty of its results; a collection of matters I think of as metrology.

Second, the problem of auxiliary hypotheses constitutes no more of a deep flaw in logical positivism than it does in confirmation. When scientists recognize that confounding factors exist they should apply the same sense of skepticism toward their own results that they do toward targeted hypotheses, and they should modify their experiment design. Well-designed experiments account for auxiliary hypotheses or make them irrelevant. Such experiments become classics and people write entire books about them (e.g., Trigg, 1975; Shamos, 1987). Cleland circles around this issue of experiment design and its continual refinement without discussing it directly.

Experimental results without supporting design and measurement analysis are meaningless. Therefore, ''protecting a hypothesis from misleading disconfirmation'' is justified when a result has no such support. As Cleland rightly points out, this protective behavior is not unscientific, but it hasn't anything to do specifically with refutation either. Evidence supported by solid experiment design and thorough analysis of uncertainties leads generally to behavior that we think of as scientific. It may renew interest in a topic and prod numerous researchers to replicate and improve their experiments. Eventually these activities settle a controversy, at least for the time being.

Cleland's examples of scientists not following the scientific method in the case of Uranus's orbit, or her hypothetical nonexpansion of copper are too simplistic to illustrate the continual refining of experimental design to reach a conclusion. Her example about a meteor impact extinguishing the dinosaurs is apparently not entirely settled (Flannery, 2001). The search for the fifth force (Franklin, 1993) provides a better example because the entire story is available for review.

The idea of a fifth force grew out of competing theories of gravitation, one of which postulated an exchange of heavy particles. This predicted an intermediate-range force akin to gravitation. The first attempts to measure (I would say refute) the fifth force involved a reanalysis of old torsion balance data, reanalysis of gravimetry in mines and boreholes, and new gravimetry on towers. Initial measurements suggested, just barely, the possibility of a fifth force. However, these analyses suffered from inhomogeneity of the earth and terrain effects, and depended on highly accurate continuation of the gravity field. Auxiliary hypotheses confounded the gravimetry. Several physicists designed experiments to eliminate these problems by examining material-dependent effects. Yet their initial results were inconclusive, which is to say that no evidence could refute a fifth force at that time. However, over the next couple of years the geophysicists improved their gravimetry and the physicists improved their methods. By the time experiments could support the resolution required to find a fifth force of the predicted size ($\sim$1% that of normal gravity), there was no evidence of a fifth force. The evidence now refuted it. The issue was closed in this instance because a clear prediction from a hypothesis was proved not true, and there were adequately precise measurements to support this result.

The most apparent difference between historical and experimental sciences concerns voluntary outlook toward prediction, experiment design, and measurement. We see this in the language that each group employs. Physicists, chemists, and biologists speak of theories having passed experimental tests—geologists speak of interpretation and confirming evidence. Geologists simply do not often emphasize the predictions of their hypotheses or the experiment design and precision required to test them. Cleland's reliance on an 1897 citation pretty much proves this. Yet there is no particular reason why they cannot coax predictions from their hypotheses, and then, rather than search for confirmation, decide where to look for refuting data. In doing so, they might have to go through a series of different and successively refined approaches, like the fifth force example, but they would also act more like experimentalists.

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