Studies in History and Philosophy of Science 45 (2014) 97–102

Contents lists available at ScienceDirect

Studies in History and Philosophy of Science

journal homepage: www.elsevier.com/locate/shpsa

Prediction in context: On the comparative epistemic merit of predictive success

Martin Carrier

Department of Philosophy, Bielefeld University, P.O.B. 100 131, 33501 Bielefeld, Germany

ARTICLE INFO

Keywords:
Application-oriented research
Prediction and explanation
Expertise or expert judgment
Governance of research processes

ABSTRACT

The considerations set out in the paper are intended to suggest that in practical contexts predictive power does not play the outstanding roles sometimes accredited to it in an epistemic framework. Rather, predictive power is part of a network of other merits and achievements. Predictive power needs to be judged differently according to the specific conditions that apply. First, predictions need to be part of an explanatory framework if they are supposed to guide actions reliably. Second, in scientific expertise, the demand for accurate predictions is replaced with the objective of specifying a robust corridor of estimates. Finally, it is highly uncertain to predict the success of research projects. The overall purpose of the paper is to enlarge the debate about predictions by addressing specifically the roles of predictions in application-oriented research.

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When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

1. Predictive novelty vs. coherence as epistemic merits

Prediction stands out among the traditional criteria of epistemic merit in science. In anticipating novel effects theoreticians manage to look further than experimenters and observers and to tell the researchers out in the field what they would find if they turned their attention to some phenomenon. Predictive success is universally recognized as strong support of the account accomplishing this feat. Some philosophers of science even elevated theory-based anticipation of novel phenomena to the central epistemic distinction of science. Imre Lakatos is known for placing the prediction of novel facts among the paramount achievement of a progressive research program (Lakatos, 1978, pp. 31–34). A theory is confirmed by the successful prediction of effects that are improbable or forbidden in light of previously accepted beliefs. In this vein, Heather Douglas has recently demanded to supplement the emphasis on explanation with an equal emphasis on prediction. Her claim is that ceteris paribus prediction is epistemically superior to accommodation, the reason being that the successful anticipation of new evidence shows that the underlying theory had not been adjusted so as to match the relevant phenomenon. The epistemic role of predictions is to provide demanding test instances for theories; that is, predictions serve to test explanations. Predictions in a temporal sense are not necessary for this purpose. Rather, it is sufficient that a prediction is new with respect to the epistemic state of the person who makes the prediction. Retrodictions count as predictions as well if this condition is fulfilled (Douglas, 2009, pp. 454–460).

However, the superior epistemic value of predictions may be contested. In particular, a conflict emerges between the requirement that a theory produce novel predictions and the demand that it match the extant state of knowledge. Novel predictions refer to phenomena that were unknown at the time the prediction was made and not to be expected given the state of the pertinent science. Accordingly, novel predictions anticipate surprising phenomena that go beyond the system of knowledge and tend to be inconsistent with it. Thus predictive power and coherence with the background knowledge are conditions that tend to point in opposite directions. It is true that both conditions can be squared if they are taken to refer to the epistemic state of a researcher. This is what William Whewell's consilience of inductions is supposed to accomplish: a supposition adjusted to a certain class of phenomena turns out to account for a remote and seemingly unconnected class of phenomena (Whewell, 1968, p. 151). The pieces that are tied together unexpectedly form a new unity in the mind of a scientist; their connection may thus pass as a novel prediction from the epistemic perspective of the individual. Yet all these pieces may have

E-mail address: martin.carrier@uni-bielefeld.de

0039-3681/$ - see front matter © 2014 Published by Elsevier Ltd.
http://dx.doi.org/10.1016/j.shpsa.2013.10.003

been well known to science before, as it is apparent from Whewell's own example of Isaac Newton's unforeseen unification of the totality of Kepler's laws and a host of other previously acknowledged features of planetary motion (ibid.).

I adopt a stronger notion of prediction that refers to the epistemic state of science rather than scientists. A prediction is something new to science, regardless of when it happened. A surprising retrodiction is a prediction in this sense. The law of multiple proportions, as derived by John Dalton from his atomic theory, led to the expectation that fixed, integral ratios should obtain among the weights of the constituents of various compounds of the same chemical substances. Dalton famously confirmed this novel prediction by using earlier data produced by other chemists (who had not realized the relevant pattern in their records). With this understanding, requiring predictive novelty is not easy to harmonize with demanding the preservation of past knowledge. Many philosophers of science adopt a conservative stance and presuppose that the knowledge gained in the past is trustworthy (Quine & Ullian, 1978, pp. 66–80). Actually, it might appear at first sight that the requirement of coherence of new theories with background knowledge is purely empirical since it involves no more than a commitment to recognizing past experience. Earlier scientists proceeded on the basis of the facts available to them, and consequently their results need to be respected.

While the emphasis on coherence grows out of the respect for the scientific knowledge gained in the past, the prominence of novel predictions often goes along with a less than sanguine evaluation of what has been achieved before. Denying the authority of the established body of knowledge emphasizes its incompleteness or unreliability. Placing successful predictions at the fulcrum of methodological assessment is tantamount to stressing theoretical progress. For instance, the pioneers of the Scientific Revolution in the seventeenth century and feminist philosophers of science today refuse to recognize the system of knowledge as dependable on the ground that the relevant scientists worked under conditions that make them untrustworthy. In the first case their attempts were spoiled by the sway Medieval Aristotelianism held over them, while in the second it is the alleged dominance of androcentric conceptions that skews their research endeavors (Longino, 1995, pp. 386–387). As a result, it was the enhancement of the system of knowledge rather than its preservation and smooth expansion that was regarded as the primary objective of scientific research. I mentioned Lakatos's stress on the anticipation of novel effects. In striking agreement with the general cultural climate of the late 1960s that he was explicitly at odds with, Lakatos placed heavy emphasis on change in science and on the substitution of time-honored theories with burgeoning approaches. In light of this position, excellent theories need to contradict the background knowledge at the time in which they are conceived and pursued.

The upshot is that the appreciation of novel predictions places emphasis on scientific progress and exhibits a tension with regarding the existing background knowledge as a sort of binding frame. Maintaining past success and boosting future accomplishments constitute contrasting epistemic standards that do not mesh easily with one another. One may either pursue the preservation of what has been achieved before as a primary goal or chiefly emphasize venturing into new ground. Put more generally, novel predictions tend to distort the coherence of the body of knowledge, and coherence of this sort is highly valued by philosophers of science as well (Park, 2010). Consider the geometrization of gravitation involved in general relativity theory. This feature created a rupture with the Newtonian view on gravity and shifted gravity away from the other natural forces. Gravitation adopted a unique position as a geometrized interaction. There was a loss of coherence but a gain in empirical adequacy and predictive power. It is true, letting predictive power direct theory-choice turned out to be justified in this case. Yet speaking more generally, the tension between coherence and progress, or the existence of competing standards all of which enjoy some plausibility suggests that the emphasis on prediction shouldn't be exaggerated.

I will elaborate this claim in what follows by turning to three fields where science is faced with the challenges of practice. This is what I mean by "prediction in context." What is the merit of predictions if science leaves the controlled conditions of the laboratory and struggles with the intricacy of the world? A case in point is the relation between science and technology. Relevant episodes suggest that predicting successfully the result of technological intervention may not be a good guide for judging the appropriateness of explanations. Another pertinent field is expert knowledge. I deny that appropriate expert opinions need to be able to buttress accurate predictions. Rather, expert recommendations ought to strive for other virtues like robustness. Finally, I address the odds of predicting research outcomes. Research policies today are often based on the understanding that results in a certain problem area can be produced on demand. This confidence stands in striking contrast to the attitude prevalent until around 1980, namely, that the pathways of research are essentially unpredictable and that the best way to stimulate the solution of practical problems is to fund fundamental research on a broad range of topics.

2. Prediction in application-oriented research endeavors

In application-oriented research, prediction is assumed to play a key role. Targeted intervention in natural processes requires the ability to anticipate the results of one's action. Science in the context of practice quite naturally places heavy emphasis on foreseeing the outcome of endeavors to bring about certain products rather than epistemic virtues like causal explanation or theoretical unification. In research on medical drugs, causal efficacy and, accordingly, the reliable prediction of future effects is a significant distinction. Accounting for this efficacy is of secondary importance. After all, aspirin had been curing headaches for almost a century without any reliable explanation of how the agent substance performed this feat precisely.

In a similar vein, Johannes Lenhard takes computer simulations to promote a "culture of prediction" in which obtaining predictions from a model counts as a central virtue. The understanding is that computer simulations typically fail to model the processes that are assumed to underlie the phenomena in question. Rather, they employ deliberately artificial constructions that are adjusted to overcoming difficulties posed by the digital mode of computer operation. Computers cannot solve differential equations; they rather need to deal with proxies. As the case may be, the solutions to the original equations may not coincide with the solutions to their proxies, and this lack of agreement needs to be compensated for by makeshift software adjustments. The price paid is the loss of the realist fit with the causal processes that actually bring about the phenomena in question (Lenhard, 2007; see Winsberg, 2003).

This leaves us with ambivalent views on the proper role of prediction in science. Douglas demands bringing prediction back into philosophical accounts of explanation and highlights the use of predictions for the test of explanations. By contrast, Lenhard underscores the dominance of prediction in many areas of science. It goes without saying that the two claims are compatible. But they point in different directions. On the one hand, the neglect of prediction is criticized, while, on the other hand, the pivotal role of predictions is underscored. Put more bluntly, it is lamented, on the one hand, that explanation has eclipsed prediction, whereas it is contended, on the other, that prediction has eclipsed explanation.

I wish to explore this issue by turning to an example from biotechnology from the 1990s. Research in the context of practice aims primarily at foreseeing the outcome of human actions. Since

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