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Introduction: Novel Predictions

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This special issue is the culmination of a sequence of events that began with organising a conference on novel predictions at the University of Düsseldorf back in February 2011. Upon the conference's conclusion, it was decided that there was enough new material to produce an edited volume. This decision was reinforced by the fact that, despite the popularity of the subject matter both inside and outside the philosophy of science community, no comparable publication existed—something that also holds true today. After a long and painstaking refereeing and editing process we ended up with eight essays. These essays represent a rather broad spectrum of views. Some are straightforward attempts to support (Eric Barnes, Samuel Schindler, John Worrall) or to defeat (Ioannis Votsis) predictivism, i.e. the view, simply put, that predictions carry special confirmational weight or even that they are the sole carriers of confirmational weight. Others take a more intricate route, attempting to reveal what is right and what wrong about it (Deborah Mayo, Gerhard Schurz). Finally, two essays examine the value of predictions in specific domains, one in practical domains (Martin Carrier) and the other in the domain of the scientific realism debate (Cornelis Menke).

Let us start with those authors who support predictivism. Barnes' contribution continues the work he carried out in *The Paradox of Predictivism* (2008, Cambridge University Press), the only monograph in existence entirely devoted to the subject matter. There he argued for a kind of predictivism he calls "endorsement novelty." According to this view, a theory endorsed by a scientist *A* is more strongly confirmed for a scientist *B* when the true evidence used to confirm it is endorsement-novel relative to *A*, that is to say, when *A* endorses the theory without relying on observations that have any effect on the truth of that evidence. In his latest work, Barnes takes up the task of responding to a number of critics. He begins with a clarification of his view, which includes a list of some its presumed advantages over the use-novelty view, an elaboration of why his view is an instance of what he calls "virtuous predictivism," and a defence against the accusation that his view does not deserve to be classified as a form of predictivism. Barnes concludes his essay with two thought experiments and a case study from the history of science, namely Mendeleev's predictions of the existence of gallium, scandium and germanium, which aim to demonstrate the superiority of his view over existing accounts.

The philosopher perhaps most synonymous with predictivism is Worrall. In recent years, he has developed an idiosyncratic version of use-novelty predictivism that distinguishes between two kinds of confirmation. The first concerns general theories by which he means theories that possess a number of free parameters. The second concerns specific theories, that is, theories that result from fixing those free parameters. Roughly speaking, genuine confirmation for either general or specific theories can be garnered only through data that is predicted by a specific theory and that is independent of the data used in the fixing of free parameters. Worrall begins his essay by elaborating aspects of this view to ward off misconceptions. He then turns to two figures in the predictivism versus accommodationism debate, namely Patrick Maher and Marc Lange. Regarding the former, Worrall argues that although at first glance Maher appears to support the temporal-novelty view, when the lessons of his coin-tossing example are properly construed and applied to scientific examples Maher's view reduces to an approximate version of his own use-novelty version of predictivism. Regarding the latter, Worrall argues that Lange's variation of Maher's original coin-tossing example is insightful in that it reveals that what matters is not the temporal order of evidence and theory but rather whether or not confirmation spreads within a theory. This insight, Worrall continues, is best explained by his own two-type account of confirmation and not by Lange's view that spreading occurs when the theory in question is not an "arbitrary conjunction".

A critical appraisal of Worrall's views can be found in Schindler's essay. Schindler examines what he judges to be two separate accounts of use-novelty present in Worrall's work, the use-novelty account proper and the parameter-fixing account, and finds them both wanting. He then proposes an alternative form of predictivism which he calls "local-symptomatic" predictivism. The central idea here is that predictions count more than accommodations but only in some contexts, namely when they reveal that the theory under consideration has correctly identified a coherence of facts. Schindler also discusses "contrapredictions,"

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i.e. those predictions made by theories that bring about corrections to hitherto accepted empirical results. He maintains that contra-predictions are a particular form of temporally novel predictions. He then proceeds to argue that temporally novel predictions played an important role in the acceptance of the periodic table of elements by the scientific community. Mendeleev's successful predictions of three new chemical elements (those we cited earlier), Schindler claims, provided additional support for the periodic table only insofar as they demonstrated the approximate truth of the coherence of facts about chemical elements. This coherence is manifested in Mendeleev's idea that all chemical elements are ordered via the sole criterion of atomic weight.

Votsis' contribution aims to supply foundations for an objective theory of confirmation. He begins the discussion with a challenge some confirmation theorists take seriously: To discover what conditions are needed, other than inferential–semantical ones, for a complete theory of confirmation. Such a theory ought to be able to answer the question whether or not, and if so to what extent, hypotheses constructed post hoc can be confirmed. Votsis claims that predictivism seeks to meet this challenge by making contingent factors, e.g. the temporal order of evidence and hypotheses, confirmationally relevant. He then goes on to construct a general counter-example to predictivism which purports to show that appeal to contingent factors results in the issuing of conflicting judgments. The upshot is that such factors must be forbidden from playing a role in confirmation. This constitutes the first of four principles presented as foundations for an objective theory of confirmation. All four are motivated by the need to avoid certain alleged failures of predictivism. The last one returns to the issue of post hoc-ness. Votsis reasons that there is nothing wrong with the confirmation of such hypotheses. It's not the manner of construction that makes a confirmational difference but how the content parts of a hypothesis are related. Support stops spreading from one part to another, he argues, when the parts stand in a monstrous relation—a notion he defines formally as measuring, roughly, the absence of unity in their content. Votsis ends his essay by replacing the original challenge with a version that takes his four principles for granted.

Perhaps more conciliatory toward predictivists, Mayo seeks to uncover several surprising facts about the debate over "double-counting," i.e. using data both to construct and support a theory. She argues that use-novelty theorists are wrong to unreservedly dismiss double-counting. What matters, in her view, is not novelty but how well the data (in conjunction with background information) rule out erroneous inferences to the hypothesis in question; incidentally Mayo lists this as one of the aforementioned surprising facts. Under what conditions and why "double-counting" should be avoided is best accounted for, she claims, by her notion of the probative-ness or severity of a test. Tailored for the admissibility of use-construction rules, the test reads roughly as follows: A piece of evidence e that is employed by a rule R to construct a hypothesis H counts as 'good' for H provided that (i) e fits H and (ii) were H false, it would be at least very improbable for R to produce as good a fit with e. Among other things, Mayo goes on to list kinds of use-construction rules, e.g. rules for constructing anomaly-overcoming auxiliaries that may or may not be legitimately applied depending on whether or not they have passed the severity test. She brings her essay to a close by urging philosophers of science to help classify the kinds of inferential errors that must be ruled out in order for the severity test to be correctly applied.

Schurz's essay starts with what he considers to be a major failure of the Bayesian concept of comparative confirmation, namely its inability to prohibit the empirical confirmation of arbitrary ex post facto explanations like rationalised versions of creationism. A view that does offer such prohibitive measures is the use-novelty account of confirmation as articulated by Worrall. Since Worrall's view suffers from a number of problems of its own, Schurz modifies this account in two respects: First, unlike Worrall, Schurz does not take the inferential relations between theory and evidence to be merely deductive but demands that they must also include inductive ones. Second, Schurz argues for a restriction of Worrall's view that parameter-fixing evidence cannot support a general theory, a restriction which focuses on the probabilistic argument that if a general theory H can be made to fit all the possible outcomes $E_1, \ldots, E_n$ of a certain experiment, then no such outcome can confirm the general theory (in the usual Bayesian sense). Based on this argument, Schurz proposes a new theory of confirmation which holds that genuine confirmation occurs when a piece of evidence Bayes-confirms at least some content parts of a hypothesis that are not logically contained in the evidence. So the confirmation of H by E (in the Bayesian sense) must spread to E-transcending content parts of H. To demonstrate the power of his theory, Schurz shows how it can be applied to various domains, e.g. to hypotheses with latent parameters and in curve fitting, but also to confirmation puzzles such as the tacking paradox or Goodman's paradox.

Carrier's contribution attempts to expand the debate about predictions by addressing the roles predictions play in practical contexts. He builds a case for three main claims. First, he argues that in application-oriented research, predictive power should not be considered the sole most important epistemic virtue of theories. Instead it must be looked at as part of a framework where other virtues, e.g. explanatory power, play significant roles. Second, he examines the role predictions play in the context of scientific expertise and policy advice. Carrier argues that often there is no demand for highly precise predictions in this context. Rather it suffices if the predictions remain stable over an appropriate range of changes in the relevant causal factors or factual conditions. Finally, he discusses whether it is possible to predict the success of planned research. Carrier surmises that the business of making such predictions is highly uncertain. He supports this view by pointing out that in some cases such as the Manhattan Project, planned research proved to be successful, while in others, such as Nixon's "war on cancer," it didn't. The overall message of his essay is that in the context of applied research predictive power plays a less prominent role.

Finally, Menke's contribution focuses in on the use of predictivism in the scientific realism debate and, particularly, the "no miracles argument." This argument is meant to lend credence to realism by holding that the best—some say the only—explanation for the predictive success of scientific theories is their truth or approximate truth. To deny that, the argument goes, would be to make the predictive success of theories miraculous. A popular way to formulate the argument is in probabilistic terms. That is to say, it is highly probable that a theory enjoying considerable predictive success is at least approximately true. Realists endorsing this formulation have been accused of committing the base-rate fallacy. That is, they have been accused of assuming favourable base-rates, i.e. the relative frequency of true or approximately true theories within the class of all theories, although, the critics argue, no information about such rates is readily available. In his essay, Menke tries to save the probabilistic version of the no miracles argument. He argues that the base-rate fallacy applies only when the argument aims to explain the success of individual theories. It does not, he continues, apply when the argument is aimed at explaining the distribution of successful predictions among rival theories within a mature field of science. In the latter case, we can presumably estimate the base-rate by drawing on the distribution of successful predictions within that field. Provided that most of the successful predictions in a field are made by a single theory, Menke argues, chance can be ruled out as a likely explanation of that success. To illustrate this point, he utilises two case studies, one involving nineteenth century optics and the other theories of gravity. The essay concludes with an attempt to pre-empt the objection that in estimating the distribution of successful predictions within a mature scientific field we might be neglecting successful predictions made by theories other than the ones that ultimately triumphed.

Aside from its informational value, we hope that the introduction has made the prospect of reading the essays in this collection more enticing. What is more, we hope that the readers will learn as much from these essays as we have.