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Prediction and accommodation revisited

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ABSTRACT

The paper presents a further articulation and defence of the view on prediction and accommodation that I have proposed earlier. It operates by analysing two accounts of the issue—by Patrick Maher and by Marc Lange—that, at least at first sight, appear to be rivals to my own. Maher claims that the time-order of theory and evidence may be important in terms of degree of confirmation, while that claim is explicitly denied in my account. I argue, however, that when his account is analysed, Maher reveals no scientifically significant way in which the time-order counts, and that indeed his view is in the end best regarded as a less than optimally formulated version of my own. Lange has also responded to Maher by arguing that the apparent relevance of temporal considerations is merely apparent: what is really involved, according to Lange, is whether or not a hypothesis constitutes an "arbitrary conjunction." I argue that Lange's suggestion fails: the correct analysis of his and Maher's examples is that provided by my account.

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1. Introduction

In previous work (for example, Worrall, 1985, 2002, 2006), I articulated an account of theory confirmation that, so I argued, satisfactorily resolves the longstanding issue about what predictive success can do for a theory that "merely" accommodating a known result cannot. I have also previously defended that view against a number of rival approaches (most recently that of Deborah Mayo—see Worrall, 2010); so far, however, this has not included in detail the approach taken by Patrick Maher (1988, 1990). I did earlier, in a joint paper with Eric Scerri (Scerri & Worrall, 2001), give some detailed historical criticisms of Maher's account of Mendeleev and the alleged extra confirmational impact on the latter's periodic law of the prediction of the existence of hitherto unknown elements; but I have not considered Maher's general approach to the prediction/accommodation issue and the further discussion that Maher's approach has engendered. In this paper I make good that omission.

Maher has "argued that the predictivist thesis holds in typical scientific contexts" (1993, p. 329) where he takes that thesis to assert "that a given piece of evidence confirms a hypothesis better if it was predicted than if it was accommodated" (ibid). As his celebrated coin-tossing example illustrates, he has the straight *temporal* notion of prediction initially in mind.[1] The idea that the time-order of theory and evidence matters is emphatically denied in my account, as we shall see. Marc Lange later (2001) provided a "tweak" on the coin-tossing example that is central to Maher's analysis and argued that, when thus tweaked, this example carries a very different moral for the prediction/accommodation issue than the one argued for by Maher.

This paper begins with a rehearsal of my own account of the general issue and the central justification for that account (Section 2)—one that, I hope, clarifies a number of points that some have found puzzling. In Section 3, I present and criticise Maher's position. I argue that the way in which prediction counts for Maher means that his position should be regarded in the end as an anti-temporal predictivist view, despite his initial assertion. I claim that once systematically elucidated and made applicable to real scientific examples, Maher's account is ultimately best regarded as an approximation to my own. In Section 4, I outline Lange's account

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[1] As the discussion in Section 3 will show however, the way in which prediction counts for Maher means that his position arguably should be regarded in the end as an anti-temporal predictivist view. I claim that once systematically elucidated and made applicable to real scientific examples, Maher's account is ultimately best regarded as an approximation to my own.

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and show that its correct features are captured better by my account, which at the same time rejects those aspects of Lange's account that are incorrect.

2. Two types of confirmation: intra-programme and inter-programme confirmation

Why did scientists come to think by the 1820s that there was stronger evidence for the wave theory of light than for the rival corpuscular theory? Why did early 20th century scientists come to accept that there is more evidence for relativity theory than for classical physics? Why do current biologists hold that the fossil record supports Darwinian theory more strongly than the "theory" of Special Creation? Duhem, after all, taught us (and Kuhn reminded us) that all the evidence in these inter-theory debates can be accommodated within the intuitively less well-supported theory.

So the results of various interference experiments were, for example, predicted by the wave theory of light but corpuscularists (or some of them) attributed those effects to a "force of diffraction"—the details of which they set out to read off the experimental results. Although Special Relativity Theory predicts the null result of the Michelson–Morley experiment, that result can, as is well known, also be explained in classical physics courtesy of the Lorentz-Fitzgerald Contraction Hypothesis. Even more straightforwardly, Philip Gosse showed how easily the fossil record (and other signs of the great age of the Earth) could be accommodated within the theory that the Earth and its flora and fauna were created relatively recently: simply assume that God created the Earth with these funny scratchings in the rocks, these funny bone-like structures in the ground, various already partially decayed samples of radioactive elements and the rest—so that the Earth was created *appearing to be*, in parts, already very old.

Lakatos, I still believe, was essentially correct that the important distinction here is between "progressive" and "degenerating" theory-changes. The corpuscular theory or classical physics or the theory of special creation do not in this way catch up in terms of evidential support with their intuitively superior rivals. A research programme progresses if it makes predictions that turn out to be correct, while a programme degenerates if it merely accommodates data after the event by making special assumptions designed on the basis of that data. Accommodations count less than predictions and the history of theory-change (or rather change of theoretical framework) in science has been guided by a consistent preference for progressive theoretical frameworks (research programmes) over degenerating ones.

Note that the process of accommodating a piece of evidence, e, *ad hoc* can always be represented (even if sometimes artificially) as a theory being developed with a convenient free theoretical parameter whose value is then fixed on the basis of e exactly so that the adjusted theory (with fixed value of the initially free parameter) yields e. So, for example, the basic theory that the universe was created "essentially as it now is" around 4004BC gives its proponents in effect an indefinite series of free parameters (specifying how the world is) that can be filled in on the basis of observation. Corpuscularists assumed a large number of free parameters in their expression for the force of diffraction, whose values could be fixed in the attempt to match known diffraction data. The Lorentz-Fitzgerald Contraction Hypothesis (LFC) involves a free length-contraction parameter that can be adjusted to fit the result of the Michelson–Morley experiment within Classical Physics.[2]

Note also one major divergence from Lakatos's original account. Lakatos initially took an observational or experimental consequence of a theory to count as a prediction just in case the empirical result was not known to hold when the theory was formulated. On the contrary on my approach "prediction" is defined simply as the opposite of accommodation: a piece of evidence e that follows deductively from T (plus relevant auxiliaries) is predicted by T just in case it was not accommodated within T by fixing some initially free parameter on its basis. Hence a piece of evidence that was known (perhaps long known) at the time some theory was formulated may perfectly well be predicted by that theory in what I claim is the epistemically important sense. What matters is whether or not the evidence was used in the construction of the theory (or rather the particular version of the theoretical framework/programme that entails it). For a well-known example, the facts about the precession of Mercury's perihelion, although they are consequences of the General Theory of Relativity played no role in the development of that theory; hence the theory *predicts* those facts in the important sense and hence is fully supported by them on my view, just as fully as if those facts had come to light only after the formulation of the theory.

But how does this account square with the point that critics of what I dubbed the "UN [Use Novelty] Charter" were quick to make (see Worrall, 2002 for references), that amending a theory to fit the facts, even more specifically using empirical data to fix the value of a parameter left free by theory, far from being a hallmark of bad science, may instead be a perfectly respectable scientific process? Suppose, for example, that some general theory leaves a particular parameter free: as the wave theory of light leaves free the wavelength of light from any particular monochromatic source. It would be madness to indulge in trial and error by guessing a particular value of the wavelength of light from a certain source and then testing that theory against observation; instead the wavelength can be "measured" in the following way. The general theory with the wavelength from any particular source left as a free parameter—call it $T(\lambda)$—predicts that, at any rate to a very close approximation, the distance d between the central fringe of the interference pattern produced by a double slit and the centre of the first dark fringe is related to the observable distance between the slits, X, the observable distance, D, from the two slit screen to the observation screen, and the (theoretical) wavelength, λ, via

$$X/(X^2 + D^2)^{1/2} = \lambda/d.$$

And this can of course be solved for λ to give

$$\lambda = dX/(X^2 + D^2)^{1/2}.$$

All the quantities on the right hand side of this latter equation are measurable. So, by taking the values given by experiment using light from a particular monochromatic source (say a sodium arc) and by calculating the value of λ as λ_0 and feeding that value into $T(\lambda)$, the more specific theory $T(\lambda_0)$ is arrived at by "deduction from the phenomena." (Of course, as always, "deduction from the phenomena" is shorthand for "deduction from the phenomena *plus* other more general background principles"—in this case $T(\lambda)$.) It would be strange, to say the least, to hold that e (the result of the two slit experiment with sodium light) does not support, or

[2] As one referee pointed out, this is perhaps in fact best regarded as a slightly different kind of case. Pre-LFC Classical Physics was tacitly but firmly committed to a particular value of the "length-contraction parameter"—namely the value 1 representing no contraction of rigid rods as they move through the ether. This however yields an incorrect observational prediction about the outcome of the Michelson–Morley experiment. Lorentz and Fitzgerald can therefore be thought of as initially retreating to the logically weaker version of classical physics which takes length contraction as a free parameter, and then using the observed result of the experiment to fix the value of the parameter again (to of course a value—namely, $(1 - v^2/c^2)^{-1/2}$—different from the initial value). The confirmational message is, however, the same.

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