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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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

does not "fully" support $T(\lambda_0)$—it *establishes* $T(\lambda_0)$ (though it does so, of course, only *relative to* $T(\lambda)$). And yet this counterintuitive consequence of no (or little) support might seem to be entailed by the "accommodation counts less" account.

Suppose that we react to this by allowing that the UN critics are indeed correct that, in this example and others like it, e does fully support $T(\lambda_0)$. Where would that concession leave our earlier discussion? After all, corpuscularists were out to deduce from the phenomena (of various interference effects) the values of the parameters in their "diffraction force" law and hence more specific versions of their corpuscular theory of light; similarly Gosse in effect deduced (or "nearly" deduced) what we might call in his (dis)honour the "Gossefied version of Special Creation Theory" (that God created the earth with the "fossils" already in the rocks, *etc*) from the fossils and other phenomena that *apparently* indicated the great age of the Earth. It seems that, if, on the basis of the "good" accommodations, we took the line that accommodated evidence in general lends support to the theory that accommodated them, then in consistency we ought to say that those phenomena—the diffraction phenomena in the case of corpuscularism and the "fossils" *etc* in the case of Creationism—supported their theories just as they supported their wave and Darwinian rivals respectively. And this would mean, of course, that the Lakatosian rationale for regarding the wave and Darwinian theories as still ahead of their respective rivals in terms of empirical support is lost.

The clue to the—straightforward—resolution of this problem is, as already hinted, to concentrate on the *relative* nature of the support in (*all*) the parameter fixing cases. Let e report simply the measured distance between the central and first dark fringe in the two slit experiment using light from a sodium arc; e deductively entails $T(\lambda_0)$ given the general, free parameter version, $T(\lambda)$. Clearly then e maximally supports $T(\lambda_0)$ *once we take* $T(\lambda)$ *as "given."* However exactly because *any* outcome of that experimental measurement could have been added to $T(\lambda)$ to get some specific version of the general theory $T(\lambda)$, the actual fringe distance recorded in e constitutes no test of that general theory and hence supplies no evidential support for it. The support or confirmation provided by e of $T(\lambda_0)$ is *intra-programme* or *intra-general theory* support: e establishes $T(\lambda_0)$ within the framework of the general theory $T(\lambda)$ and in that sense could not support it any better; but e gives no support that is "passed on" to the general theory $T(\lambda)$. (Remember that I am taking e here to state simply the precise distance between the central and first dark fringe. The further facts that the central fringe is bright and that first and other dark fringes are symmetrically placed with respect to the central fringe are a different matter. These further facts do indeed provide "full" inter-programme support for the wave theory—exactly because those features are independent of the value of the wavelength.[3])

Similarly in the case of Gosse: since he in effect (quasi-)deduced his version of Creationism from the fossil and other "apparent age" phenomena, that version was supported maximally by those phenomena from which it was deduced *relative to* the general theory—that the Earth was created in roughly 4004BC essentially as it now is; but those phenomena give *no* support of the kind that "passes onto" the general theory. This is the kind of support that counts when assessing the relative empirical merits of two rival general theories or programmes: the corpuscular and wave theories of light or Darwinism and Creationism, for example.

In a moment of Lakatos-inspired weakness, I resorted in earlier work to subscripts, calling the intra-framework notion "confirmation$_1$" and the more powerful inter-framework notion, "confirmation$_2$." So while, for example, the fossils provide confirmation$_1$ for

Gossefied creationism, they supply no confirmation$_2$ and hence no evidential support for the central or general creationist thesis.

The difference between the scientifically acceptable accommodations (as in the wave theory case) and unscientific ones (like the "Gosse dodge") in fact has nothing to do with the accommodatory move itself. In both types of case the correct judgment about the accommodation and the accommodating theory is: maximal (or near maximal) intra-framework/general theory support for the accommodating theory, zero support for the framework/general theory itself. The difference is that in the "good" cases there are *other* pieces of evidence that do indeed give support to the general theory. So there are lots of pieces of evidence that supported the general wave theory of light (including, as noted, the symmetric nature of the two slit interference pattern and the fact that the central fringe is bright); but no evidence at all that supports the general Creationist hypothesis.

But a mystery seems to arise here: how, if the Duhem thesis is correct, can such support for the general theory (in of course positive cases) ever be garnered? Duhem claimed that no testable prediction is ever derived from a "single" scientific theory, so that general theories must always be supplemented by specific and/or auxiliary assumptions in order for directly testable consequences to follow. It looks as if all confirmation directly accrues to specific versions of general theories: how then does the general theory itself—the general theory that light consists of transverse waves in a medium, or the Darwinian theory of natural selection—obtain empirical support?

Two types of case are salient. In the *first*, some auxiliary assumption is indeed developed (or equivalently a parameter value is adjusted) so as to accommodate some known initially anomalous observation, but the overall system of general theory plus accommodating assumption proves to be *independently testable*. That is, the accommodating move not only deals with the initial anomaly but—together of course with the general theory—makes a further testable prediction that turns out to be correct. Perhaps the most often cited case of this kind is Adams' and Leverrier's prediction of the existence of Neptune: the initially accepted auxiliary assumption about the number of planets affecting the path of Uranus led to an incorrect prediction of the latter's orbit, and that assumption was modified specifically so as to account for Uranus's observed orbit; however, that modified assumption involved the consequence that a hitherto undiscovered planet exists with a particular orbit and particular characteristics and that consequence was of course not only testable but spectacularly confirmed by the observation of Neptune. Hence the data from Uranus, since it was used in "deriving" the new version of the theory complete with conjectured new planet, provided only confirmation$_1$ for that new version; the actual observation of Neptune, on the other hand, provided confirmation$_2$—initially accruing to that new version but also "spreading over" to give support to the general underlying Newtonian theory.

Another classic case of this type involves Fresnel's shift from the longitudinal to the transverse wave theory light. Fresnel's original assumption (along with all other early wave theorists) was that the luminiferous ether is a fluid (how else could the planets move through it so freely?) and this meant that light waves must be longitudinal since fluids transmit only such waves (ones in which the direction of the vibrations of the molecules constituting the light is the same as that of the overall transmission of the light). The fact, discovered by Fresnel and Arago, that the interference bands disappear when the two-slit experiment is performed with oppositely polarized light coming through the two slits was anomalous for this version of the theory. Fresnel switched to the theory that the

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[3] I am grateful to one of the referees for insisting that I clarify this point.

waves are transverse (that is, the ether-molecules vibrate at right angles to the transmission of the light) specifically so as to accommodate this experimental result. (He "read off" the transverse nature of the waves from the null result in the modified two-slit experiment.) Of course the modified theory now entailed that there would be no interference fringes in this experiment with oppositely polarized light beams—it was bound to do so "by construction"; however, Hamilton showed that Fresnel's transverse assumption together with the underlying general theory entails *further* testable consequences—particularly the existence of (internal and external) conical refraction: consequences that were again entirely unexpected and spectacularly confirmed (by Humphrey Lloyd). Hence while the null result of the modified two slit experiment provides only intra-general theory confirmation$_1$ of the general wave theory of light with the transverse wave assumption, supplying no extra support for the general theory itself, the existence of conical refraction is genuine confirmation$_2$ for that general theory: support in that case "spreads" from the specific (transverse) theory to the underlying general theory of light as a wave in an elastic medium.[4]

Cases of confirmation$_2$ of the *second* type can loosely be characterized as ones in which it looks at first sight as if the Duhem thesis fails to apply. A particularly good example is provided by planetary stations and retrogressions and the Copernican theory. It had been known for centuries before Copernicus that what distinguishes observationally the planets ("wandering stars") from the "fixed stars" was that as well as sharing the stars' diurnal westward rotation, the planets had their own additional motion through the fixed stars. This additional motion is generally eastward but occasionally the planet slows to a halt (remember a halt against the moving background of the fixed stars) and then briefly turns westward before resuming its normal eastward motion through the stars. The fact that the planets exhibit observable "stations and retrogressions" falls "naturally" out of Copernican theory: it looks like a direct consequence of the Copernican claim that we are on a moving observatory, aboard the Earth, and so will periodically overtake those planets moving more slowly than the Earth or be overtaken by those moving more quickly: the *appearance* of the planet's slowing, standing still, and retrogressing against the background of the fixed stars is a seemingly inevitable consequence of the fact that we are on a moving observatory, the Earth. In fact the Duhem thesis still applies as can be seen if you ask whether, had no stations and retrogressions been observed, this could have been made consistent with the theory of a moving earth. The answer is positive, but the extreme adhocness and unnaturalness of the auxiliary assumptions that would be necessary are an indication of how natural the account of the real phenomena are within Copernicus. So planetary stations and retrogressions confirm on my account

(confirm$_2$) not just the detailed Copernican models but the basic general Copernican theory—because they "drop naturally out" of that theory.[5]

3. Maher's defence of the "predictivist thesis"

On my account, then, the time order of theory and evidence *in itself* carries no epistemic significance—if a scientist didn't know some experimental result e ahead of formulating a theory then she could not have used that result to fix a parameter value within it, but she of course may have known the result but still not used it in that way. Patrick Maher, (seemingly!) on the other hand, 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 is exemplified by his examples, he is thinking of the straightforward temporal notion of prediction here, where a theory predicts some evidence that it entails just in case that evidence was discovered to hold only *after* the theory at issue had been articulated. It therefore seems as if Maher is advocating a version of the temporal view: the time-order of theory and evidence, at least sometimes, counts when it comes to the degree of confirmation lent to the theory by the evidence.

The example that Maher uses to develop his account has been subsequently much discussed and involves coin-tossing. Two investigators end up making assertions about the outcome of 100 tosses of a coin, but by different routes. The first investigator, I$_1$, articulates his hypothesis only after 99 tosses have occurred; he operates by "reading back" (Maher, 1988, p. 275) those 99 outcomes and then adds a prediction about the outcome of the 100th toss. The second investigator, I$_2$, formulates her claim about another sequence of 100 tosses *ahead of* any tosses, again 99 tosses have been made and her hypothesis has got all 99 correct.

In both cases, then, we await the 100th toss and in both cases we have a prediction of its outcome: let's assume with Maher that both predict a head on the 100th toss of their series. Maher is surely right that we would have a great deal more confidence in I$_2$'s prediction than we would in I$_1$'s. He reports that

> People I have talked to say that in [case 1] they would give a probability of around ½ to the 100th toss landing H, while in [case 2] they judge the probability to be near 1. Their rationale is that [I$_1$] is probably just making a random guess [about the 100th outcome], while [I$_2$'s] successful prediction of 99 tosses is strong evidence that [she] is not merely guessing, but rather has some reliable method for predicting the toss. (*op. cit.* pp. 275–276)

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[4] What, one referee effectively asked, if Fresnel had simply conjectured the transverse theory straight off? Wouldn't that theory then on my account have got full confirmation$_2$ from the result of the Fresnel–Arago experiment? But the bold Popperian-conjecturing imaginary Fresnel and the real Fresnel who worked his way to the transverse theory on the basis of experiment finish up with the *same* theory; and yet my account seems to have the absurd result that, because of the two different ways of arriving at it, the self same theory can have different empirical confirmation. Since "same theory, same confirmation" seems like a necessary condition for any remotely defensible account it would appear that my position is in dire straits. This is in fact a version of an objection raised as early as (1974) by Alan Musgrave. I have replied to the objection at length in earlier papers (see in particular my (2006, pp. 51–56). The essence of the reply is that whether or not a theoretical parameter is left free by general theoretical considerations is not an arbitrary matter: the basic theory that Fresnel was advocating was that light is some sort of wave in some sort of elastic medium; the impact of confirmation$_2$ in this case is to give us confidence in this general theory and—from a practical point of view—underpin the programme to develop that general theory to apply to areas aside from diffraction and interference; the general theory leaves open the issue of whether or not the waves are longitudinal or transverse (indeed there were other possibilities but let's not complicate matters): hence both outcomes of the Fresnel–Arago experiment—fringes remain, fringes disappear—are open; therefore some confirmational weight just is swallowed up by determining the value of the longitudinal/transverse parameter. The lucky imaginary Fresnel would not therefore have garnered any extra empirical support of the kind that is at issue—that is support that underpins the general underlying theory.

[5] One referee asked both for a general account of "naturalness" and for more detail on how the basic Copernican theory is compatible with no observable stations and retrogressions. I am, sadly but predictably, unable to meet the first request. Like all those philosophers of science (pretty well all of them!) who have invoked naturalness or its close relatives "unity" and "simplicity" I am unable to provide an adequate, non-circular, characterisation. All I can do is try to elicit the intuition by pointing to examples. On the second issue: clearly what would have to be invoked is some "compensating mechanism" that "just happens" to cancel out the expected effects of the overtakings: so the planet would have be supposed to be (somehow!) kicked into some fancy extra movement that it normally does not display just as either the Earth was overtaking it or it was overtaking the Earth; as I said, it is the extreme adhocness of this suggestion that gives the appearance that the Duhem problem does not arise. Put more positively, what you have to assume to get the "natural" prediction of stations and retrogressions out of Copernican theory is that *all* that happens is the overtaking (while the basic theory is committed only to there being an overtaking).

It seems undeniable that it is indeed reasonable to have more confidence in the assertion about the 100th toss made by I_2 who predicted the first 99 outcomes before they occurred than in the corresponding claim made by I_1 who saw the first 99 outcomes before producing his hypothesis. The only difference between the two cases seems to be the time order of theory and evidence—the evidence, that is, of the first 99 outcomes; so, at least in this case, (temporally) predicted evidence counts more. Or so things *seem*.

Marc Lange (2001) describes Maher's example as "a lovely example to motivate the intuition that a successful prediction has a kind of confirmatory significance that an accommodation lacks." In fact the intuitive/rhetorical force could perhaps have been made still greater by (a) having the two investigators pronounce about the *same* run of 100 tosses rather than different runs (whether with same coin and same tossing mechanism is not made explicit by Maher who simply describes them as two "cases" of tossing a coin 100 times); (b) having the two investigators make *different* predictions about the (now same) 100th toss—say I_1 predicts heads, while I_2 predicts tails; and (c) asking readers whether, if they were forced to bet on the 100th toss, they would bet heads or tails. In this slightly amended scenario we would surely all, unless being perverse, bet tails: again because we would feel I_1 is merely guessing the outcome of the 100th toss while I_2 is very (overwhelmingly) likely to have "some reliable method for predicting the toss."

So, to repeat, there is no denying in either the original or my amended example that the fact that I_2 predicted correctly the first 99 tosses ahead of time, while I_1 simply read off the results of those tosses has important impact on the reasonable degree of belief in the predictions made by the two of them about the 100th toss. But let's ask: what exactly is it that is (or is not) confirmed in Maher's initial two contrasting coin tossing cases? The only hypothesis on the table in each case is h—simply an ordered list of 100 toss outcomes. But Maher never speaks of the confirmation of h. Instead the emphasis is on the evidence giving us greater faith in *investigator* I_2 and hence in her prediction about the 100th toss—because her "successful prediction of 99 tosses is strong evidence that [she] is not merely guessing, but rather has *some reliable method for predicting the toss*" (*op.cit.*, emphasis supplied). And, in response to Howson and Franklin (1991), Maher emphasised the fact that "the introduction of the concept of a method is the main conceptual innovation in [his] account of the value of prediction" (1993, p. 335).

So what gets confirmed directly on Maher's story is not any hypothesis about the coin or the coin-tossing mechanism, but instead a meta-level hypothesis about the investigators involved: that I_2 has some method for reliably predicting tosses, whereas I_1 has probably simply noted the first 99 outcomes and added "a random guess" about the 100th—indeed the initial description of his example explicitly supposes that I_1 developed h in (large) part by "reading back" the results of the first 99 tosses.

Maher unquestionably has the psychology right: it is of course logically possible that a pure guess would get all the first 99 tosses correct and indeed this will happen on average once in every 2^{99} times that someone makes such a guess, but we would all take it that that number is so huge that it is overwhelmingly likely that I_2 knows more than she is telling and has some systematic and accurate way of predicting the outcomes; while we have no reason

whatsoever to think the same of I_1. So we all indeed end up with greater confidence in I_2's prediction of the final toss than we have in I_1's.

However, this in turn surely means (as Howson and Franklin already argued in their (1991)) that the example is entirely unrepresentative of the sort of case we are interested in from science. In such cases we want to assess the evidential support for hypotheses *about the real world, not* ones about the reliability of scientists. Moreover, (except in cases where unpleasant "external" factors become involved) science has a tradition of openness—we could perfectly well ask I_2 how she "did it": what "method" of toss prediction she had; and we would expect a reply. Suppose, by analogy, that Einstein had initially hidden the GTR but had, out of the blue and without showing that those predictions followed from any theory, made successful (temporal) predictions—of the gravitational bending of light, say. We would not be content with the judgment that the success of such a prediction gave us confidence that Einstein had a "reliable method" of making physical predictions! Instead we would say that he must have some underlying (and stunningly successful) *theory* that enabled him to make those predictions, and can we please have that theory on the table? Once on the table, then the questions become—exactly as they are on my account—about the relationships between the underlying theory, specific versions of it and the evidence. And then, as I have argued (and I think Maher would concede), purely temporal questions about the time order of theory and evidence become irrelevant.

Indeed it is difficult to see what a "reliable method of prediction" could be in science other than having a good general theory that may or may not contain free parameters that need to be fixed on the basis of observation.[6] The purely temporal predictive success is simply indicative of the (likely) existence of such a theory which itself is highly evidentially confirmed. Once we have eliminated the investigators (as we should) and have the general theory on the table along with its specific version and start to ask about confirmation, then (1) the time-order is of not the slightest relevance and (2) the considerations that *are* relevant are exactly the ones involved in my account. (In the end then it becomes unclear if Maher should be thought of as advocating "the" (temporal) predictivist thesis.)

In the case of Maher's coin predictors, what we all surely believe is that I_2 is more accurately described not as (very) likely to have a "method for predicting the tosses" but rather as having some direct insight into, or knowledge of, the coin-tossing set up. It is difficult even to *imagine* what might be going on in detail in this case because, of course, regular tossing of a coin, as normally performed, like other games of chance, such as roulette, is "pseudo-indeterministic." That is, while everything from the input of the flick of a certain force to a particular part of the coin to the outcome of the coin lying with a particular face up, given the physical characteristics of the coin and the elastic properties of the surface on which it lands, might be at root fully "metaphysically" or "ontologically" determinate, that outcome is so dependent on nuances of initial conditions that are in practice unknown and on a complex of interacting factors of which we have no detailed knowledge that there is no way in practice that that outcome can be successfully, reliably predicted. Indeed that is the whole point of coin tossing as a "game of chance."

Maher is therefore asking us to imagine a scenario that is in fact impossible in the ordinary run of events. In order for it to be

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[6] Maher is quite insistent that the two are different and Lange (op cit, p. 578) goes along with Maher that "a 'reliable method' is not the same as a true [somewhat true?] hypothesis." But while clearly the two are linguistically distinct, I find it difficult to see how they are anything other than two different ways of describing the same thing. Surely we can at least always reconstruct the development of any specific theory (the "method" by which that theory is produced) as the result of plugging in some data (or other "givens") into some more general theory (so that data plus general theory deductively entails the specific theory). Lange also has a long footnote (p. 579) where he expresses some related doubts about Howson's view that Maher's case is unrepresentative of real science because we typically know the "method" by which a hypothesis was arrived at. But it seems to me that, although in real cases there is always a penumbra of vagueness, if we cannot at least rationally reconstruct that "method" as in effect, as suggested above, a deduction from the phenomena—a case of plugging some empirical information into a general theory so as to generate a more specific version of that theory—then there are no clear cut confirmation judgments to be made.

possible that I_2 had this predictive success, the coin-tossing process cannot be pseudo-indeterministic. One suggestion might be that some well-controlled coin-tossing *machine* is involved, one that reliably produces Heads or Tails depending on some macroscopically discernible, but not immediately apparent, aspect (or aspects) of the initial state of the machine and of the coin. Perhaps the flipping mechanism guarantees that if the coin starts with Heads up it inevitably lands Tails up, and vice versa; and that there is part of the machine that detects the coin's face and places it initially either Head or Tail up according to some pre-programmed routine. (Or perhaps the coins are magnetized and the machine can produce, via some mechanism, two different magnetic fields such that when the magnetic field is F_1 a head is always produced, and when F_2 a tail. But let's stick to the first possibility for expository purposes, the lessons will surely be the same in all cases.)

In order to have correctly predicted in advance the first 99 outcomes, I_2, unlike those of us supposed by Maher to be innocently looking on, must presumably know how this machine works and could explain its working so as to show us that the outcomes as designated in (the no longer really hypothetical) h were, short of some malfunction of the tossing machine, inevitable. But this turns the example into one that is, for a second reason, unrepresentative of the issues from science that this whole debate focusses on: the issue is about the relative confirmational weight lent by some piece of evidence e to two rival *theories* each of which entails e. Relatedly this means that it seems inappropriate to judge that the first 99 coin toss outcomes support or confirm anything here—beyond the claim that the machine is working as advertised and not suffering from some malfunction.

Just to further emphasize the ultimate irrelevance of the time-order of theory and evidence at least when we bring the case nearer to the typical scientific one: consider again Maher's first investigator, I_1. The reason that we regard his prediction of the outcome of the 100th toss as having only a half chance of success is our *suspicion* that he had no insight into the coin-tossing mechanism and that, not only had he as a matter of fact observed the first 99 toss-outcomes before formulating his version of h about the whole run of 100 tosses, he *needed to* make those observations in order to produce a hypothesis that accorded with the first 99 outcomes. (Maher tells us that I_1 "read back" the first 99 outcomes, but of course we can't tell for sure that he read them back in the sense that this was all he *could do*: all that we can, as onlookers, *observe* is the time at which he produced h compared to the time at which the various tosses were made.) But suspicions like this play no role in theory-confirmation in science: we could of course ask I_1 (just as we asked I_2) how he had reasoned. If I_1 says that he too had knowledge of the coin-tossing mechanism and shows us how that mechanism works (implying of course that he knows how it works) and that he simply chose, for no particular reason, to articulate h only after the first 99 tosses, then the situation is entirely the same as it was with I_2: assuming that he does indeed have knowledge of the mechanism, then I_1 *could have* predicted the first 99 tosses in advance and the fact that he chose not to do so goes down as a quirk of no epistemic consequence.[7]

The role of the time-order of theory and evidence in Maher's example, therefore, is simply to indicate that there are further, deeper confirmational issues at stake in terms of the confirmation of some underlying more general theory. Those further issues themselves do not at all involve the time order and are best addressed in terms of my account. When properly analysed, Maher's account does not in fact endorse the "[temporal] predictivist thesis" and is best regarded as a less than optimally formulated version of my view.

4. Lange's "tweak" on Maher's coin tossing case

In a (2001) article, Marc Lange considers a "tweak" on Maher's coin tossing example. Lange writes (p. 580):

> Maher specifies that the first 99 outcomes form 'an apparently random sequence of heads and tails'. Suppose otherwise. For example, let the sequence be strictly alternating: tails, heads, tails, heads,…In one case, the subject proffers [h] prior to learning the outcome of any toss. In the other case, the subject arrives at [h] by accommodating the outcomes of the first 99 tosses. Although the two cases still differ only in that one involves prediction while the other involves accommodation, it seems to me that this difference no longer makes very much difference to us. Intuitively we judge it to be extremely likely that [h] accurately predicts the outcome of the 100th toss, regardless of whether [h]'s success in the first 99 tosses came by prediction or accommodation.

Lange seems to envisage one run of tosses ("*the* sequence…") which threatens to obscure the situation since a predictivist might say that we indeed have just as much faith in the accommodator's prediction about the 100th toss but exactly because the same outcome was predicted by the successful predictor of the first 99. It clarifies the situation if (like Maher, I believe) we think of two different sequence of tosses both of which however have—so far!—strictly alternated: tails, heads, tails, heads…So one investigator, I_1', looking at the first sequence, waits until after 99 tosses and then formulates h_1' which entails that the 100th toss in his sequence will be heads (as are the outcomes of all even numbered tosses in his sequence), while I_2', formulates h_2' about the second run of tosses ahead of any toss, h_2' gets the first 99 outcomes in this second sequence correct and it entails that the outcome of the 100th toss in this sequence will be also be heads (as all even numbered outcomes in her sequence have again been observed to be). In both cases we await the outcome of the 100th toss. Lange's claim is then that we would have just as much (or perhaps "almost as much") confidence that h_1' will be correct in its entailment as we have that h_2' will be correct in its—even though the evidence of the first 99 tosses in sequence 1 was accommodated within h_1', while h_2' predicted the outcomes of the first 99 tosses in sequence 2.

Lange, as we shall see, argues that our contrasting reactions to Maher's original and his "tweaked" case shows that the crucial question in judging confirmation or empirical support is not whether or not the evidence was predicted but rather whether or not we judge the hypothesis involved to amount to an "arbitrary conjunction."

Lange adopts and endorses Goodman's view that a conjunction is "arbitrary" just when "establishment of one component endows the whole statement with no credibility that is transmitted to other component statements" (Goodman, 1983, pp. 68–69).

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[7] One referee urged me to analyse some more realistic example where it is easier to see what the underlying hypotheses and general theories might be. The example s/he suggested was predicting earthquakes. Suppose two investigators produce hypotheses about a series of earthquakes—when and where they will occur and with what intensity; one, say, about earthquakes in Asia and one about earthquakes in America. Say that 10 such earthquakes are mentioned in each case (100 is overkill). The first investigator formulates her hypothesis before any of the relevant earthquakes has occurred—9 have been observed and all her predictions were correct. The other investigator just reads back a list of the relevant facts about 9 earthquakes that have already occurred and adds a prediction about a 10th. This is completely analogous to Maher's example of course. The right thing to say seems even clearer: namely that the predictive success of the first investigator gives us good reason to hold that she has a "reliable method" for predicting earthquakes and here that can *obviously* mean nothing more than a well-confirmed general theory of seismic activity in the area covered. I am grateful for this suggestion since it avoids the sort of fantasy needed to make the coin-tossing case transparent, but I have continued to discuss the coin-tossing in the text since analysis of Maher's view has been so closely tied to it.

Goodman famously gave the example: "8497 is a prime number and the other side of the moon is flat and Elizabeth the First was crowned on a Tuesday." Of course ascertaining that one of the conjuncts is true supports the whole conjunction—in the sense of "reducing the net undetermined claim"—but it is not support that "spreads" to the overall claim by making the remaining conjuncts any more likely to be true. Lange, again in Goodmanesque vein, extends this to universal generalisations that are accidental (that is, "believed to be coincidental, if true") such as "All the families on my block have two children": discovering that the family next door did indeed have two children would not, it is suggested, make it any more likely that the family two houses away did so. (It is not in fact clear to me that this example works—suppose that, as is often the case, all (or most) of the houses in any given neighbourhood are of a similar size—say 3 bedroom; then the fact that one household includes two children surely makes it at least somewhat more likely that others do. But let this pass—the general idea is clear even if Lange's example fails to instantiate it clearly.)

So, according to Lange's analysis, we judge the first 99 tosses observed by I_1 in the original Maher case to form an arbitrary conjunction and so I_1 has no more reason to append "heads" as the outcome of the 100th toss than he has to append "tails"; hence the fact that his hypothesis gets the 1st 99 cases right has no impact on the credibility of the prediction about the 100th—the establishment of the correctness of that hypothesis' consequences about the first 99 tosses does not lend confirmation that "spreads" to its consequence about the 100th. (Just as, in Goodman's example, ascertaining that Elizabeth I was indeed crowned on a Tuesday does nothing that should increase our confidence in the assertion that the other side of the moon is flat.) On the other hand, when we are told that Maher's second investigator I_2 predicted the first 99 outcomes of his run, even though the run looks "random," we "now believe it more likely that [I_2] was led to posit this particular sequence by way of something we have not noticed that ties [it] together"[8]; and hence that his hypothesis does *not* make the first 99 tosses an arbitrary conjunction. And this has the consequence that the hypothesis's accuracy with the first 99 is confirmation of the whole theory of the kind that increases the probability of its remaining conjunct—its prediction about the 100th toss.

In contrast, in the "tweaked" case where the outcomes have (so far!) alternated, we believe—again according to Lange—that we "see the pattern" and so (partly, presumably, because it is such a simple pattern) regard both of the two alternating sequences of 99 tosses as *not* forming arbitrary conjunctions. Hence, on Lange's analysis, there is no reason why confirmation should not 'spread' from the first 99 outcomes to the prediction about the 100th in both cases—so independently of whether or not the first 99 outcomes had been observed prior to the formulation of the hypothesis.

Let's initially grant that *so far as the prediction of the outcome of the 100th toss is concerned* there is no (or little) difference in our degrees of confidence in Lange's two cases. Because, let's suppose, we feel it "has to be" a case of alternation (in both series) and we feel that this is likely to continue in still to be observed cases, we are just as confident about h'_1's prediction of a tail as we are of h'_2's of a head.

But even so Maher's asymmetry, while modified, remains intact. We would feel confident in the tweaked cases, just as we were in the untweaked one, that "predictive" I'_2 has some insight into (or rather likely knowledge of) the coin tossing mechanism and that "accommodating" I'_1 has no such insight. The difference is that, while in Maher's case it seems likely that I_1 was simply adding a

50/50 guess on the 100th toss while "reading back" the results of the first 99, in Lange's case I'_1 is going on the gut feeling that such a simple pattern as the alternation could not have arisen by chance (and so [!] will continue). But I'_2 *knows why* it will continue in her case—again presumably because she knows (or has a very good theory of) how the coin tossing mechanism operates so as to have produced alternating outcomes and knows that this will continue. Just as in Maher's untweaked case, the fact that I'_2 was able to predict the first 99 outcomes in advance is good evidence that she has an underlying theory (or more likely, as discussed earlier, in this unrepresentative case, outright knowledge) of what is going on, while I'_1 does not have such a theory (knowledge). It happens that in Lange's tweaked case this fact, so long as we go along with the idea that the alternating pattern is made "manifest" by the first 99 outcomes, has no impact on our confidence about the entailments about the 100th toss made by the two lower-level claims about the outcomes. Nonetheless I'_2 remains in a stronger epistemic position overall (so can presumably again, and unlike I'_1, show us how the tossing mechanism operates so as to produce alternating outcomes) and the fact that she is in that stronger position is indicated by the fact that she was able to predict what all the outcomes would be without needing to observe any.

Indeed the point can be made more forcefully if we tweak Lange's tweak. Suppose that we go back to Lange's own set up in which there is just one sequence of tosses and again the outcomes have alternated for the first 99: tails, heads, etc. Again I_1 has waited until the first 99 tosses have been made before producing his hypothesis which records those 99 outcomes and adds the prediction of a head on the soon-to-be-performed 100th. The hypothesis that I_2 formulated ahead of all tosses and predicted correctly all 99 outcomes however predicts a *tail* on the 100th![9] Again we can ask how the sensible person would bet (if forced to do so). Well at the very least it is certainly no foregone conclusion that we should bet "heads" in accordance with the "pattern" that we might have thought jumped out at us from the first 99 tosses. Indeed I would suggest that the smart money would be on "tails"—the argument being that I_2's predictive success with the first 99 tosses really does strongly suggest that she has insight into/knowledge of the coin-tossing mechanism and must have seen a reason why the "strict alternating pattern" that has allegedly been "exhibited" hitherto is merely a coincidence: I_2 knows why (or has a very well confirmed theory to the effect) the "pattern" will be "broken" on the 100th toss.

This in turn underlines the fact that there are very good reasons *not* to grant Lange his distinction between arbitrary and non-arbitrary conjunctions. It may seem difficult intuitively not to be impressed by such a simple "pattern" as alternating heads and tails in a series of coin tosses, but ought we to be so impressed when we know that through any number of data points an indefinite number of curves can be drawn (all differing with respect to at least one so far unobserved datum) and hence that any "conjunction" can be considered to be non-arbitrary in infinitely many ways? This is not a mere "philosopher's quibble." Suppose, for example, that we were to record the position of, say, the planet Mars on successive midnights: what we get when we conjoin the data statements will look initially like an entirely "arbitrary conjunction"; but once we have Kepler's laws (or their later improvements) we see that each element in the conjunction is a necessary consequence of those laws (plus initial conditions). Contrary to what Lange's example seems to have seduced him into supposing, science (or at least its more successful parts) is not a "pattern recognition" exercise but rather an attempt to find underlying explanatory regularities that can,

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[8] Lange seems to be thinking about "pattern spotting" but surely the only (remotely!) realistic possibility if we are looking to tie this to real cases in science is that I_2 has some (very good, very well confirmed) theory that entails the outcomes.

[9] I am very grateful to one of the referees for this clever suggestion.

and often do, reveal hidden order in the most "irregular," apparently "arbitrary" conjunctions of data.

At the level of data, then, such as the outcomes of the tosses, a sequence is certainly not arbitrary *simpliciter*, instead its status depends on theories. I think that this cannot seriously be denied and, when its consequences are thought through, it spells disaster for Lange's account. To see this let's return to Maher's original totally untweaked case: where both the sequences of tosses that are concerning his investigators appear "random" as Maher says, or "arbitrary" in Lange's term. Suppose that—as earlier—the first investigator I_1 (the one under suspicion of merely accommodating the first 99 outcomes) insists that the conjunction of outcomes that his hypothesis entails is *not* arbitrary. Instead it is the restriction to the first 100 tosses of a general deterministic law that entails outcomes for all tosses of the coin using the particular mechanism at issue. However this law initially had a series of free parameters whose values became fixed only on the basis of the first 99 tosses—which is why he was unable to predict any toss outcomes before the 100th. But having filled in the values of all parameters on the basis of the 99 tosses, I_1 is now is in possession of a theory that entails outcomes, not only for the tosses already made, but for the 100th and indeed all subsequent tosses.

Since I_1 can show that each of the first 99 tosses are in accordance with his now parameter-free theory, that sequence is not arbitrary (any more than the sequence of Mars's apparent positions at midnight is arbitrary) and so Lange will, it seems, have to allow that the evidence confirms I_1's hypothesis just as the first 99 tosses that were predicted in advance by I_2 confirmed her hypothesis. Yet Lange wants (surely correctly) to endorse the confirmational asymmetry that Maher insisted on in this case. Hence, since it is surely impossible to deny that what counts as an arbitrary conjunction is theory-dependent, Lange's account cannot, in this case, allow him to underwrite the obvious confirmation asymmetry that we would all see.

My account, on the other hand, delivers exactly the intuitively correct judgment here. I_1 is now claiming that he began with some general theory $T(\alpha_1, \ldots, \alpha_m)$ with the α_i free; evidence e (the results of the first 99 tosses) was then used to create the more specific theory $T'(\boldsymbol{\alpha}_1, \ldots, \boldsymbol{\alpha}_m)$ where the $\boldsymbol{\alpha}_i$ are now fixed; $T'(\boldsymbol{\alpha}_1, \ldots, \boldsymbol{\alpha}_m)$ entails e but is only confirmed$_1$ by it on my account—that is, relative to the general theory T, the specific theory T' is actually maximally confirmed by e, but there is no confirmation$_2$—that is, no confirmation "spreads" to the general theory T; hence supposing that we have been given no other evidence for T, then we have (as yet and in contrast to the predictive I_2) no empirical reason to believe any of T''s further consequences, in particular about the outcome of the next (100th) toss. However, should T' get the 100th and the 101st and…tosses correct then that would of course supply confirmation$_2$ and hence some reason to accept the underlying general theory T. (Though of course I_2 remains in a stronger position from the point of view of confirmation since, unlike I_1, she was able to predict the outcomes of the first 99 tosses, as well as, we presume, the later ones.)

Of course, as we already noticed (and as is in any case obvious), Maher and Lange's coin-tossing examples are far from reflecting real scientific cases. Lange does attempt to extend his analysis to such cases. In particular he argues, that the reason why the null result of the Michelson–Morley experiment does not support Lorentz's general ("classical") theory is that once the Lorentz-Fitzgerald Contraction Hypothesis (LFC) is included that theory becomes an "arbitrary conjunction." The "empirical adequacy of the [LFC]," he writes (*op. cit.*, p. 583) "does not confirm any of the other components" of Lorentz's theory; but this is "not directly because Lorentz formulated the [LFC] to accommodate the…evidence rather than before the evidence had been found. Rather, the LFC together with the rest of [Lorentz's theory] form an arbitrary conjunction."

Lange makes the mistake of thinking that the LFC is an additional conjunct within the modified version of Lorentz's theory—"added" so as to account for the null result of the experiment. Although often made, the suggestion makes no logical sense: if T makes an incorrect prediction about some experimental result (there will be a shift in the fringes in the Michelson–Morley experiment) then so of course does T & C for any additional conjunct C! And in fact ahead of the null result of the experiment there was an assumption within Lorentz's theory about length contraction, namely that it does not occur. So one conjunct (in the initial theory) says the length contraction factor is 1, the replacement conjunct (in the modified theory) says length contraction factor is $(1 - v^2/c^2)^{-1/2}$. It is not a question of the modification making the theory an "arbitrary conjunction." Instead—just as my account suggests—it is a question of, in the usual story, the new value for the length contraction factor having, within Lorentz's theory, been read off the null result of the experiment. This means that Lorentz's modified theory gets only confirmation$_1$ from that result. (Unlike the Special Theory of Relativity which gets confirmation$_2$ from it.)

In sum, Lange and I start out from a shared intuition about some kinds of confirmation "spreading" within theories and some not—more particularly in my case spreading (or not) from the specific theory that actually entails the evidence to the general theory underlying it; and from the shared view that it is the distinction between confirmations that spread and those that don't that is really at issue in "prediction versus accommodation" cases rather than anything to do with the time order. Lange's claim is that such "spreading" occurs exactly when the theory that entails the evidence does not amount to an "arbitrary conjunction." But I have shown that this analysis is untenable both in his coin-tossing case and in the genuinely scientific case that he cites of the LFC. The intuition that we share can only properly be fleshed out using the account that I have endorsed and developed in Section 2.

Acknowledgements

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