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The roots of predictivism

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ABSTRACT

In *The Paradox of Predictivism* (2008, Cambridge University Press) I tried to demonstrate that there is an intimate relationship between predictivism (the thesis that novel predictions sometimes carry more weight than accommodations) and epistemic pluralism (the thesis that one important form of evidence in science is the judgments of other scientists). Here I respond to various published criticisms of some of the key points from *Paradox* from David Harker, Jarret Leplin, and Clark Glymour. Foci include my account of predictive novelty (endorsement novelty), the claim that predictivism has two roots, the prediction per se and predictive success, and my account of why Mendeleev's predictions carried special weight in confirming the Periodic Law of the Elements.

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1. Introduction

In *The Paradox of Predictivism* (2008, Cambridge University Press) I tried to demonstrate that there is an intimate relationship between predictivism (the thesis that novel predictions sometimes carry more weight than accommodations) and epistemic pluralism (the thesis that one important form of evidence in science is the judgments of other scientists). The thesis of epistemic pluralism is at one level a mundane truth: working scientists make important use of the judgments of other scientists in assessing scientific theories. We can verify this claim by picking up any scientific article and noting the multiple citations of other scientific authors' judgments offered in support of various scientific claims. In *Paradox* I argue for the pervasiveness of epistemic pluralism in theory assessment. But I also argue that epistemic pluralism contains the key to a proper understanding of predictivism. Here I respond to various published criticisms of some of the key points from *Paradox*.

It is easy to point to an intuitive relationship between predictivism and epistemic pluralism. Imagine that you are in search of a financial counselor to help you invest a recently acquired inheritance. You consider two candidates, each of whom possesses some body of theoretical belief which, they each claim, can explain how stock prices have fluctuated over the last several years. However there is this critical difference: one candidate expounded his theoretical belief *prior* to the last several years and thus successfully *predicted* the recent price fluctuations. The other expounded his theoretical belief *after* the fluctuations occurred and thus apparently *accommodated* the fluctuation data. There is a palpable intuition that one should prefer the counsel of the predictor over the accommodator. For the successful predictions confirm the credibility of the predictor—to make a long string of stock price predictions successfully is quite difficult, and one who does this demonstrates considerable expertise. But the mere act of endorsing some theory that explains some data after the data is already known carries no similar implication of credibility. Thus we can envision an agent-centered theory of predictivism.

It may be objected at this point that while the agent-centered approach has some plausibility, there is surely another, deeper story which eliminates reference to agents per se. This story will focus instead on the specific theories of the agents, the data they consider, and how strongly that data supports their theories given their respective bodies of background beliefs. Many would hold that the real confirmation theoretic story should be spelled out in strictly logical terms at this level. While there is something to this objection, I think it should not annul our interest in agent-centered theories of confirmation. For there are many cases in which a theory must be evaluated by one who lacks full information about the underlying beliefs, data, or theories of predictors and/or accommodators. Consider your predicament in the previous para-

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graph: assuming you are not yourself a financial expert, you must rely on the advice of those who appear to be genuine experts. Predictive success can function, for the non-expert, as an important form of evidence of expertise, and for this reason, a form of evidence for the theory endorsed by the predictor. Thus predictivism is spelled out in the context of an assumption of epistemic pluralism. But my position is that this is not merely true for the non-expert theory evaluator—it is true for the expert theory evaluator as well. This is because even experts must rely on the judgments of other experts even within their own fields of expertise, a claim I document at some length in Ch. 2 of *Paradox*.

2. What is novelty?

One way to appreciate the predictivism/pluralism connection is by way of considering the very nature of a novel prediction. Perhaps the most popular conception of prediction is the famous heuristic conception, according to which a consequence N of theory T is a novel prediction of T just in case N was not "used" in the construction of T. Heuristically novel confirmations have seemed to many philosophers to confirm theories more strongly than evidence a theory was "built to fit" (i.e. accommodated). Maher's theory of predictivism (1988, 1993). e.g., could be applied to explicate this appearance: a novel prediction, unlike an accommodation, testifies to the claim that the predictor "had a reliable method of making predictions." Of course predictivism (understood in terms of the heuristic conception of novelty) has been denounced because it makes facts about theory assessment curiously dependent on facts about particular agents, specifically about what evidence an agent who constructs a theory used in constructing it. Many philosophers have maintained that actual theory assessment should not and could not depend on such historical curiosities about particular agents. My position is rather that the heuristic conception misses what is epistemically important about prediction vis a vis predicting agents. To see this, consider the following example: suppose some competent scientist Connie constructs a new theory T that entails some (otherwise very unlikely) consequence N1 that she assures us will turn out true and is subsequently shown to be true[1]—she does not use N1 in constructing T. An evaluator—a person who is assessing the probability of T—is impressed and is prepared to give high marks to T on the basis of something like Maher's reasoning (Connie apparently has a reliable method of making predictions). This would simply seem to be an example of the intuition that motivates the heuristic-based thesis of predictivism. But suppose at this point Connie insists that while she did construct T, she by no means endorses T—in fact, she is certain that T is false. She claims that if T is tested again, now by testing for another (also very unlikely) consequence N2, T will be shown false. She is obviously sincere. Now at this point I would suggest that the success of N1 would testify to Connie's credibility as a predictor for this evaluator—for such credibility seems required to explain her predictive success. If indeed we judge she is a reliable predictor based on her prediction of N1, we should judge that her prediction of N2 will probably be confirmed. However, for this very reason, her successful prediction would no longer constitute strong evidence for T despite its being a use-novel confirmation of T. This is because Connie did not endorse T upon constructing it. Now let us suppose that another competent scientist, Endora, appears and claims that Connie is wrong. Endora endorses T and claims that it is probably true—and that N2 will be shown true when tested. Now suppose N2 is shown true. The truth of N2 could testify to the credibility of Endora rather than Connie—despite the fact that she did not construct T. This suggests that the epistemically relevant act is not construction but endorsement.[2]

David Harker admits that he does not share my intuition about this case. He asks, regarding Connie's refusal to endorse T despite her accurate prediction that N1, "why not suppose that the theory is veridical in some respects but not in others?" (2011, p. 221). This might be the case, but this possibility does nothing to undermine the point I am trying to make: Endora, because she endorses the theory and thereby successfully predicts N2, thereby gains credibility which reflects off her and on to T (though parts of T might yet be false)—suggesting that whatever skepticism was based on Connie's non-endorsement was misplaced. I am arguing simply that it is endorsement, not construction, which is epistemically relevant.

What does it mean to endorse a theory? To endorse a theory, I claim, is in some way to affirm the theory in the presence of some evaluator(s) (other than the endorser himself, who is obviously also an evaluator of T).[3] This would consist of posting a probability for the theory that is either high or at least not so low as to constitute a primarily skeptical or noncommittal attitude toward the theory. (I should note that I am using the term "probability" here in its subjective sense, as simply a synonym for "degree of belief." I do not assume that an agent's probabilities are necessarily coherent.) I claim that N (a known consequence of T) counts as a novel confirmation of T relative to agent X insofar as X posts an endorsement-level probability for T that is based on a body of evidence that does not include observation-based evidence for N. That is, X does not base her endorsement of T on any observation of N's truth. Confirmations that are novel in this sense I deem "endorsement-novel." To say that N is a novel prediction of T is thus not merely to say that N is a heretofore unestablished consequence of T—I rather construe a prediction to be a human act of endorsing a theory that carries such an implication. Predictivism now amounts to the claim that when true evidence N confirms T, endorsed by X, T is more strongly confirmed for some evaluator when N is endorsement-novel relative to X than when it is not.[4] One advantage of endorsement-novelty over use-

[1] Although I often use the phrase "some empirical consequence N is a logical consequence of theory T" it should be emphasized that typically T will entail N only in conjunction with certain auxiliary hypotheses which are, in the context, regarded as unproblematic propositions of background belief.

[2] SHPS Referee 1 comments that the claim that an endorser's predictive success confirms her credibility is unjustified in the absence of information about the base rate of her success in theory generation. If an endorser's base rate of success is low enough, it may be more probable that she has merely gotten lucky in making a successful prediction than that she had some truth-conducive basis for endorsing the theory. True enough, but I preempt this objection by assuming that my endorsers (Connie and Endora) are known to be "competent," and competence could be understood in terms of a not too low base rate for theory generation. But my point is that predictive success is further evidence of competence, which is to say of a high base rate.

[3] Unless otherwise noted, in *Paradox* I take an endorser of T to be one who affirms the truth (or approximate truth) of T. (In Chapter 4 I allow that an endorser could be one who merely affirms the empirical adequacy of theory.)

[4] Referee 2 offers the following objection: "...suppose we include all possible agents (i.e. not just actual ones) into our evaluation, so as not to arbitrarily eliminate any opinion. Then there will always be at least one agent who endorses T without relying on observations regarding N...But this collapses the author's view into the view any true consequence of a theory strongly confirms the theory." Notice, first of all, that this objection could easily be applied to refute the heuristic conception as well—insofar as T was built to fit N by some actual scientist, there is always a possible scientist who built T on some other basis, thus N should always (by the referee's logic) be counted a novel consequence of T. However, it is a mistake to think that appeal to the judgments of merely possible scientists carries epistemic import in our present context. To appeal to such judgments would undermine any and all ordinary appeals to the epistemic import of the judgments of actual scientists: e.g., it is generally considered that the existence of a consensus among scientists that T is true is good evidence for T. But even given such a consensus there is a population of possible scientists who would deny T—if we counted this population as epistemically significant then consensus would lose any epistemic force.

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