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Howson on Novel Prediction

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ABSTRACT. Bayesian discussions of the value of novel predictions have become moribund since the early 1990's. The last major discussion occurs in Howson and Urbach (1993), and the Bayesian position on novel prediction presented there is largely negative. Howson and Urbach (1993) contains a defense of predictivism utilizing an argument found in Howson (1984; 1990). However, I show this defense to be unsatisfactory. Instead, by deploying a formalism found in Howson and Franklin (1991), and despite their disavowal of predictivism in that paper, I demonstrate in Bayesian fashion the value of novel predictions. The elegance of this formalism bypasses the interesting but rather complicated, Bayesian defense on predictivism found in Maher (1988; 1990).

KEYWORDS: Bayesian confirmation theory, Colin Howson, Allan Franklin, predictivism, probability theory, novel predictions, Mendeleyev.

Colin Howson, in various publications (1984; 1990; Howson and Franklin 1991; Howson and Urbach 1993), addresses the question whether a hypothesis is better supported when it makes a novel prediction than when it accommodates evidence already known. His conclusions are as follows: 1) where one deliberately constructs a hypothesis to capture a certain piece of evidence, it is false to claim that, necessarily, such evidence does not support the hypothesis (this claim Howson 1990 calls the 'Null-Support Thesis'); and 2) al-

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though in the usual case accommodated evidence supports a hypothesis just as well as predicted (synonymously, novel) evidence, there are nevertheless certain circumstances in which predicted evidence supports a hypothesis better. In this paper I am less interested in the accommodation thesis 1) than I am with the novel prediction thesis 2). In particular, I wish to claim that Howson's stated support for 2) is quite weak and has practically nothing to do with novel predictions; and that in criticizing Patrick Maher's (1988; 1990) support for the predictivist thesis, Howson along with Allan Franklin (1991) surprisingly creates an ideal formalism for defending 2), despite openly disavowing predictivism. Let me begin with the latter claim.

1. Howson and Franklin's (1991) formalism

In Howson and Franklin (1991), we are asked to compare two situations: first, a coin tossing case, and second, Mendeleyev's prediction of three new elements using his Periodic Table. Maher had first suggested this comparison in his (1988) and, though his formalism for handling these cases is interesting, I omit discussion of it here since I think Howson and Franklin (1991) offer a more approachable, straightforward formalism. We briefly review this formalism before turning to evaluating it.

In the first case, a subject predicts the outcomes of 100 tosses of a coin. e is the result of the first 99 tosses, h is the result of the 100^{th} toss plus e, and m is "the hypothesis that the subject has reliable advance information about the outcomes of the 100 flips of the coin" (Howson and Franklin 1991, p. 576), where such advance information might include such things as the precise bias of the coin, the subtle environmental conditions that could influence the flight of the coin, the relevant laws of physics that govern the coin's movement, and so on. By probability theory,

(1) $P(h/e) = P(h/e\&m)P(m/e) + P(h/e\&\text{-}m)P(\text{-}m/e)$.

The case is now sub-divided into two different scenarios: SCENARIO (A) the subject accurately predicts the outcomes of the first 99 flips without being informed about the results beforehand; SCENARIO (B) the subject is informed about the outcomes of the 99 flips beforehand.

SCENARIO (A): $P(h/e\&m) = 1$ (since if the subject has reliable advance information about the tosses, she will certainly know h). Thus,

(2) $P(h/e) = P(m/e) + P(h/e\&\text{-}m)P(\text{-}m/e)$.

Following Howson and Franklin, since $P(e/m) = 1$ and $P(e/\text{-}m)$ is very small, $P(m/e) \approx 1$, and so $P(h/e) \approx 1$; in all likelihood, *e* provides excellent support for *h*.

Analysis: Howson and Franklin here demonstrate that the subject's track record as an excellent predictor supplies confirmatory weight to the hypothesis that she correctly predicts the 100th toss. According to predictivism, this support should be better than the support provided for *h* in a case where we assume that the subject was told about the 99 tosses beforehand and was unable to make a successful prediction of such tosses otherwise. This is exactly what we find when we consider SCENARIO (B).

SCENARIO (B): Unfortunately, Howson and Franklin's discussion of this case is confused. They comment:

> the background information does not specify what the outcomes of the first 99 flips were, and so *m* does not entail *h* or *e* relative to that information (although *e&m* entails *h*). (1991, p. 576)

Yet, in their presentation of SCENARIO (A), there is no mention of what the background specifies as to the exact outcomes predicted by the subject – and still *m* is taken to entail *h* (and thus *e*). Indeed, given how Howson and Franklin define and use the symbols *m*, *h* and *e*, it does not matter whether the background information specifies what the outcomes are; *m* entails *h* (and so *e*), in any case, for given that the subject has reliable advance information about the outcomes of the 100 flips she will correctly predict *h*, whether in SCENARIO (A) or SCENARIO (B), that is, whether she was informed about the outcomes of the first 99 flips or not.

Thus, we have $P(h/e\&m) = 1$, and again (2), as above – an equation Howson and Franklin agree with (but I believe derive in a confusing way in their discussion of SCENARIO (B)). They then argue as follows:

> the probability of *e* conditional on *m* is now plausibly the same as its probability on *-m*. [Thus] [...] by Bayes' Theorem [...] $P(m/e) = P(m)$. (1991, p. 576)

But again given the meaning assigned to *m*, $P(e/m)$ is surely much larger than $P(e/\text{-}m)$. However, it is still the case than $P(m/e) = P(m)$ in the circumstances described in SCENARIO (B); for the fact that the subject correctly predicts *e* is irrelevant to the claim that she has advance reliable information about the outcomes of the 100 flips of the coin, given that she was informed about the outcomes of the first 99 flips beforehand. (And similarly for $-m$; accordingly, $P(-m/e) = P(-m)$). Thus,

(3) $P(h/e) = P(m) + P(h/e\&-m)P(-m)$

which is the same equation Howson and Franklin arrive at, derived in more obvious fashion, I believe.

Given (3), the value of $P(h/e)$ will vary depending on $P(m)$. If $P(m) = 1$, $P(-m) = 0$ and so $P(h/e) = 1$. And if $P(-m) = 1$, $P(m) = 0$, and since $P(h/e\&-m) \approx .5$ (that is, the subject has a 50/50 chance of correctly predicting the result of the 100^{th} toss, given a complete lack of advance information about the tosses), $P(h/e) \approx .5$ as well. In brief, if $P(m)$ is high, so is $P(h/e)$; if $P(m)$ is low, $P(h/e)$ is about .5.

Analysis: It follows that the probative significance of evidence e in the 'accommodation' case (where the subject is informed about the outcome of the 99 flips beforehand) varies with the value of $P(m)$. Where $P(m)$ is low, $P(h/e) \approx .5$ in the accommodation case whereas $P(h/e) \approx 1$ in the prediction case. In other words, where $P(m)$ is low, predicted e better supports h than accommodated e. Conversely, where $P(m) \approx 1$, $P(h/e) \approx 1$ in both the prediction and the accommodation case. In other words, where $P(m) = 1$, there is no special confirmatory benefit in predicting rather than accommodating e.

What sense can we give to these results? It turns out that when the subject, to begin with, is unlikely to have advance knowledge about the coin tosses ($P(m)$ is low), she acquires significant confirmation for this claim by successfully predicting the first 99 tosses ($P(m/e) \approx 1$ with prediction). Conversely, she acquires no extra confirmation for the claim of advance knowledge if she is preinformed about the results about the tosses ($P(m/e) = P(m)$). In this way, the hypothesis that she correctly picks the outcome of the 100^{th} toss receives better confirmation in the prediction case than in the accommodation case. (If $P(m/e) \approx 1$, $P(h/e) \approx 1$; if $P(m/e)$ is low, $P(h/e) \approx .5$.) On the other hand, if the hypothesis of advance knowledge is maximally probable from the start ($P(m) = 1$), no extra confirmatory benefit is endowed on the hypothesis that the subject correctly discerns the 100^{th} toss when the subject is successful at predicting e as opposed to just accommodating e (for in either case, $P(h/e) = 1$).

My belief is that we have derived in a fairly straightforward Bayesian fashion the essential issue in adjudicating the benefit of prediction over accommodation. And Howson and Franklin apparently concur with this assessment in the coin-tossing case. Yet they claim that the scientific case, as exemplified by

Mendeleyev's prediction of three new chemical elements using his Periodic Table, is not amenable to the same sort of analysis. I believe their reasoning on this issue is flawed, a flaw connected to the confusion I cited above regarding their analysis of SCENARIO (B).

2. The Mendeleyev case

On the basis of his Periodic Table, Mendeleyev famously predicted the existence of three elements, scandium, gallium and germanium. The question is, does Mendeleyev's hypothesis that the third element, germanium, exists receive better support given that he correctly predicted the first two elements than if he had known about the two elements previously and simply accommodated their existence into the Periodic Table? If this case is analogous to the coin-tossing case above, then the answer here should be yes. However, Howson and Franklin do not think the cases to be analogous. They make the following comments: where *e* is the observation of the first two elements, and *h* is *e* plus the hypothesis that the third element is germanium,

> the Mendeleyev and the coin-tossing examples are not isomorphic precisely because there is no asymmetry in the Mendeleyev example in the relation between *m* and *e* in the predictive and accommodative cases. For what is *m* after all in the Mendeleyev example? It is the hypothesis that Mendeleyev's 'method' of prediction, that is, his theory of the Periodic Table, is true. *But m entails the truth of h and e independently of whether or not Mendeleyev had arrived at his theory before learning e*. Thus [...] the support of *h* by *e* should be independent of whether or not Mendeleyev had known about *e* before advancing his theory. (1991, p. 577; their italics)

It should be clear where Howson and Franklin go wrong in this quote. In their confused rendering of SCENARIO (B), discussed above, they claim that *m* does not entail *h* or *e*, and attribute the diminished support *e* provides for *h* in the accommodation case to this lack of entailment. However, I have argued that the asymmetry in the prediction and accommodation cases holds, *given that m entails h and e in both the prediction and accommodation cases*. Thus, the coin-tossing and 'science' cases are analogous, or at any rate Howson and Franklin have not shown how they are disanalogous.[1]

[1] As such, their final statement in their 1991, that the moral of their paper is that "science really is not coin-flipping" (p. 584), is gratuitous.

Now whether or not in some case prediction has value over accommodation depends, as I have noted, on $P(m)$, that is, on how likely the subject has reliable advance knowledge. For instance, in the Mendeleyev case, if from the perspective of the scientific community wherein Mendeleyev works the theory of the Periodic Table is thought to have a low probability of being true, it follows that Mendeleyev's hypothesis that the third element is germanium is better confirmed if he can predict the existence of the first two elements, scandium and gallium, than if he learns about them beforehand and accommodates them into his theory. On the other hand, if Mendeleyev's theory of the Periodic Table is considered plausible and likely to be true, there is no special benefit to prediction. These conclusions follow from the Bayesian analysis given above. However, Howson and Franklin provide exactly the opposite assessment. In response to Maher who provides a Bayesian defense of predictivism, they speculate on why Maher thinks prediction carries extra weight in the Mendeleyev case; "the answer", they submit,

> [is] very simple, and carries no comfort for Maher's view. It is that there was at the time no chemically plausible theory of the elements which explained [the original] 62 elements and e other than Mendeleyev's. This being the case, $P(e/\text{-}m)$ was small and $P(m/e)$ correspondingly large. [...] [We] feel secure in denying that the evidential force of the new evidence was due to the evidence's being *predicted*; rather it was due to the evidence's being *explained* by plausible theory. (1991, p. 578; their italics)

I find these comments entirely mysterious. Perhaps they are suggesting that Maher and other predictivists are fooled by a confirmation bias: a strongly held theory appears to get an extra confirmatory boost by making a prediction, just because it is a strongly held theory. To further support their anti-predictivist stance, they ask us to consider (a fictional) Mendeleyev who has formulated in his mind the Periodic Table prior to the discovery of the three elements. We suppose that these elements are discovered independently of Mendeleyev's knowledge of the Table. Mendeleyev then does two things: first, he publishes his Table, making clear to the scientific community how the Table coheres with the newly discovered elements (the non-predictive case). Then, after letting his first pronouncement settle in, he reveals that his Table had, in fact, anticipated the existence of these elements beforehand (the predictive case). From here Howson and Franklin make the following remarks:

> our thesis is that this autobiographical revelation should have made no difference to the evidential status of e. According to the predictivist thesis, on the

> other hand, it should have made every difference. But this seems absurd: the theory and the statement of the relevant physical facts are both independent of this information about Mendeleyev's personal history. (1991, pp. 578-579)

But there is no argument here. Instead of an argument, they provide an appeal to authority: they quote the particle physicist Yuval Ne'eman who makes the following comment about the successful prediction of omega minus by the eight-fold way:

> the importance attached to a successful prediction is associated with human psychology rather than scientific methodology. It would not have detracted at all from the effectiveness of the eightfold way if the omega minus had been discovered *before* the theory was proposed. (Quoted in Howson and Franklin 1991, pp. 579-580; Ne'eman's italics)

The last two quotes illustrate a potential confusion in discussions of novel predictions, one resulting from a failure to distinguish between two different kinds of novelty – temporal novelty and heuristic novelty. This distinction arose in the philosophic literature almost twenty years before Howson and Franklin published their paper (see Zahar 1973, p. 101; Musgrave 1974, p. 11), and it is a distinction that is glossed over by them in their paper. The difference between these two notions of novelty is this: evidence is *temporally novel* if no one had any knowledge of its occurrence prior to experiment or observation; and evidence is *heuristically novel* if it was not used in the construction of the hypothesis under test. Why this distinction matters is because one can be a predictivist and contend that heuristically novel evidence has extra confirmatory value, while still denying that temporally novel evidence has this value. That is, one can be a predictivist and *agree* with Howson and Franklin that an autobiographical revelation that Mendeleyev had not known of the elements before proposing his Table is irrelevant to the confirmatory worth of observations of these elements (so long as, had he known of the elements, this knowledge would not have played a role in his construction of the Table). And again, one can be a predictivist and *agree* with Ne'eman that it would not have detracted at all from the effectiveness of the eight-fold way if the omega minus had been discovered *before* the theory was proposed (so long as such an observation of omega minus would not have played a role in the construction of the eight-fold way).

Of course, Howson and Franklin might still contend that, whether understood temporally or heuristically, novelty is evidentially irrelevant. But a Bayesian analysis of the issue, using their own formalism (understood properly), leads to the opposite conclusion. Moreover, such a Bayesian analysis also leads to the surprising result that it is not the *plausibility* of the hypotheses supported that is at issue with prediction, but the *implausibility* of the hypothesis that the subject has reliable advance knowledge. For, as I have argued, where $P(m)$ is low $P(h/e) \approx .5$ in the accommodation case and $P(h/e) \approx 1$ in the prediction case, whereas $P(h/e) \approx 1$ in either case if $P(m)$ is high.

3. Howson's argument for predictivism

Despite denying that, in the usual case, prediction has special evidential value (and correlatively affirming that accommodation *does* have evidential value, contrary to what he takes some philosophers, such as John Worrall, seem to have claimed), Howson in various places (1984, pp. 249-250; 1990, pp. 236-237; Howson and Urbach 1993, pp. 411-412) allows there to be particular circumstances where prediction *does* have such special value. These are circumstances involving parameter adjustment with a suitably general hypothesis. We follow Howson and Urbach's 1993 presentation of this contention (though practically identical arguments occur in the other two cited papers). Consider a hypothesis h containing underdetermined parameters. Observation generates evidence e which suffices to fix these parameters, leading to the constructed hypothesis h'. However, we also suppose there is a hypothesis h'' that predicts e directly, without any parameter adjustment. Finally we assume that h and h'' have equal prior probabilities, and that e is logically implied by both h'' and h'. By probability theory, $P(h''/e) - P(h'') = P(h'')(1 - P(e))/P(e)$ and $P(h'/e) - P(h') = P(h')(1 - P(e))/P(e)$. Clearly, then, the level of support e provides for h'' as compared to h$'$ depends on $P(h'')$ and $P(h')$; but $P(h) > P(h')$ (h' is a special case of h, and so is less likely); and since $P(h'') = P(h)$ (by hypothesis), $P(h'') > P(h')$. Thus, under these circumstances, a hypothesis that predicts some evidence is better supported than a hypothesis that is constructed solely to capture this evidence.

This argument of course provides cold comfort for predictivists. The extra support garnered by h'' is not in fact the result of it having made a prediction. For if $P(h)$ were larger than $P(h'')$, large enough so that $P(h') > P(h'')$, it would follow that making a prediction decreases the support garnered from the evidence and, conversely, accommodation (parameter adjustment) increases this support. Thus, the benefit we have found with prediction here is not a benefit accruing to prediction *per se*, but to prediction given a certain prior distribution of probabilities; as such, it is a benefit that could just as well accrue to accommodation as to prediction.

Recall from above Howson and Franklin's discussion of the Mendeleyev case in which they claim that the support for a hypothesis increases with the prior plausibility of the hypothesis (again, this explains the greater support allotted to Mendeleyev's prediction of germanium). What we have just seen is a Bayesian formalization of this insight applied to the case of parameter adjustment. I claim we should adopt some caution at this stage if we take Bayesianism to provide a normative theory of evidential support, for it is surely excessively conservative to suppose that hypotheses about which we are confident are thereby, by that very reason, granted enhanced evidential support. Admittedly, if Bayesianism is understood as a descriptive theory of scientific rationality then Howson's result can be understood as capturing a typical though probably unfortunate feature of human reasoning, that we tend to see those hypotheses we are already sure about as better confirmed by evidence than hypotheses about which we are dubious. In other words, the common person is subject to a confirmation bias. But a descriptive theory is not what we are after here. So, confronting Howson, we have two problems: first, he hasn't adequately explained what evidential value there is in prediction *per se*; and secondly, his Bayesian analysis unduly legitimizes a conservative hypothesis-testing strategy.

However, with respect to the second objection, doesn't my approach to novelty commit a similar mistake? For I have said that the benefit of prediction over accommodation depends on the prior (im)plausibility of m, on the (im)plausibility of the hypothesis of advance knowledge. Again, if $P(m)$ is high, there is no benefit to prediction over accommodation, whereas there is such a benefit if $P(m)$ is low. How can I legitimately authorize this dependence of evidential support on the value of $P(m)$ in light of my criticism of Howson's view, according to which the level of evidential support depends on $P(h)$?

As a response, let me emphasize that there are some important differences between Howson's approach and mine. First of all, on my account, accommodation never generates *better* support than prediction (i.e., keeping the meanings of h, e and m fixed), whereas it sometimes will with Howson's approach (as we saw earlier). For either $P(m) < 1$ or $P(m) = 1$, and in the former case prediction has special evidential value whereas in the latter prediction and accommodation have the same value. In this regard, my account coheres better with our common understanding of the value of prediction: prediction, possibly, has an evidential advantage over accommodation, but not vice versa. Secondly, the benefit to prediction on my approach is not perpetual. As $P(m)$ approaches 1, prediction and accommodation come to have the same evidential value. Yet with Howson's strategy, given that $P(h'') \geq P(h)$, one could repeat his result again and again, using the same e. In each case, h'' still predicts e directly, and h' is still h with its parameters adjusted to accommodate e; moreover, as h'' is confirmed by e, the value of $P(h'')$ increases relative to $P(h)$, and so the beneficial, predictive effect is magnified. Of course, a halt to this iterative confirmation would stop if evidence e were to become 'old' – but Howson believes he has a response to Glymour's the old evidence problem (1990, p. 238ff), so this option is not open to him.

Finally, as we indicated earlier, the benefit of prediction on Howson's approach seems entirely spurious – it all rests on the values of the prior probabilities allotted to the hypotheses under test, and why these values should matter essentially to the benefit of prediction is left unexplained. On the other hand, my approach redirects focus from the plausibility of the hypotheses under test to the implausibility of m, the hypothesis that the subject has reliable advance knowledge about the observed phenomena under consideration. In the coin-tossing case, for example, m includes awareness of such things as the bias of the coin, the environmental conditions that could influence the flight of the coin and so on. In the Mendeleyev case, m stands for (following Howson and Franklin 1991, p. 577) Mendeleyev's theoretical understanding of the chemical elements, as expressed in the theory of the Periodic Table. On the approach I am suggesting the key is that by making a successful prediction, m is confirmed (that is, the posterior probability of m increases). For example, the coin-tosser's successful prediction of the first 99 tosses confirms the claim that she has precise knowledge about the coin, the relevant ambient environmental conditions, and other relevant factors. Similarly, Mendeleyev's prediction of the first two elements demonstrates his understanding of the chemical elements in the context of the Periodic Table. In both these cases, a successful prediction as opposed to an accommodation demonstrates the predictor's possession of knowledge regarding the observed phenomenon – as we have symbolized this fact, $P(m/e) \approx 1$.[2] And it is this demonstration that leads us to allot enhanced support to the subject's future predictions. Thus, I have not only shown there to be a link between making a prediction and enhanced support for a hypothesis in probabilistic terms; I have provided a link that makes intuitive sense of what value there might be in prediction. That is, if there is any value to prediction, I think my account is much better at explaining this value than Howson's 'parameter adjustment' account.

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[2] In the accommodation case the subject may also possess such knowledge, but this fact is not *demonstrated* by having accommodated past results.

Nonetheless, one might raise the following objection to my approach: I have, as one critic puts it, "conflated and confused credit for the scientist with confirmation of the hypothesis". The ability to make successful predictions, supposedly, only exhibits a cognitive virtue in the scientist making the prediction (this is perhaps one way of interpreting an increase in $P(m/e)$) but says nothing, or should say nothing, about the degree of confirmation the scientist's hypothesis receives. However, I think it is arguable that by demonstrating a scientist's cognitive virtue, one thereby provides extra support for this scientist's predictions. For it is true that in assessing the quality and relevance of observational evidence we often refer to the intelligence and integrity of the individual producing this evidence. Evidence in support of a hypothesis generated by someone who is deadly honest is more probative than similar evidence produced by a liar; here, surely, credit leads to confirmation. Moreover, consider the further important feature of my Bayesian analysis, that where $P(m) = 1$ to begin with there is *no* benefit with prediction over accommodation. In other words, if we interpret m as symbolizing a scientist's 'credit', we have put a point to the above critic's complaint regarding "conflating and confusing credit [...] with confirmation": sometimes credit for the scientist does have confirmatory impact and sometime it does not – what my account does is to provide a way of adjudicating this issue. In particular, where a scientist's credit is firm there is no further boon to her credit by making a successful prediction, and so on my analysis there is no enhanced confirmation for her hypotheses. Conversely, where a scientist's credit is under dispute, making a successful prediction *does* enhance her credit and so does provide extra support for her hypotheses.

Yet, I believe, the 'confusing confirmation with credit' issue is a complete red herring. For m is not the hypothesis that the scientist is 'virtuous' or 'has epistemic credit' or 'is an authority'. Rather, m is a set of claims to knowledge – it is a set of hypotheses made by the subject about the phenomena under inspection. For example, in the coin-tossing case, it expresses the subject's detailed understanding of the nature of the coin, the ambient environment, and so on. Accordingly, by successfully predicting the first 99 tosses, the coin-tosser's possession of such understanding is confirmed. Similarly, by successfully predicting the first two elements, scandium and gallium, Mendeleyev confirms his hypothesis of the theory of the Periodic Table. In each case, we represent probabilistically the effect of this confirmation by assigning a value of 1 to $P(m/e)$. An extra, confirmatory effect on h is then inferred by deducing that $P(h/e) = 1$. Along these lines I believe we are able to explain the benefit accruing to a successful prediction, without mentioning at all the 'virtue' of the scientist, her 'epistemic credit', or whatever. On my approach, credit is not "conflated and confused with confirmation" simply because credit never makes an appearance as an issue at all.

4. Conclusion

The Bayesian formalism I am recommending (which I believe captures the evidential significance of prediction over accommodation) has a precedent in the non-technical literature, to wit, in the work of John Worrall who is a long-time defender of heuristic novelty as a valuable criterion for assessing the worth of evidence. I want to conclude by invoking his approach since he is one of Howson's prime targets as an example of someone who has a faulty understanding of novel evidence. (See, e.g., Howson 1984, p. 250; 1990, pp. 225, 236-237.)

Worrall in his (1985) discusses a case of what one might presume to be a regrettable, ad hoc adjustment of a theory to accommodate evidence. The case involves the discovery by the 19th century optical theorist, Thomas Young, that light is subject to the principle of interference. Subsequent to his discovery, Young was confronted with the problem of explaining why two candles near one another did not produce illumination exhibiting light and dark fringes, as predicted by the interference principle. He responded by constructing a hypothesis: his interference principle, he claimed, applies only when "the two interfering 'portions' of light originate in the same source" (Worrall 1985, p. 311). Now, as Howson understands Worrall's position, such an ad hoc maneuver is completely unacceptable. To illustrate this reading of Worrall, he quotes the following passage from Worrall (1978): "of the empirically accepted logical consequences of a theory those, and only those, used in the construction of the theory fail to count in its support" (Howson 1984, p. 250; 1990, p. 233; originally in Worrall 1978, p. 48). In other words, on Howson's view, Worrall is a supporter of the 'Null-Support Thesis', the thesis that where one deliberately constructs a hypothesis to capture a certain piece of evidence, such evidence, necessarily, does not support the hypothesis; as such, Worrall is presumably compelled to reject Young's ad hoc theoretical maneuver. Conversely, Howson rejects the Null-Support Thesis: for him, where one deliberately constructs a hypothesis to capture a certain piece of evidence, it is false to claim that, necessarily, such evidence does not support the hypothesis. So on Howson's account and apparently contra Worrall, it is possible for Young' adjusted interference principle to receive support from the absence of an interference pattern found with two candles.

However, Worrall's approach is more subtle than Howson thinks. On Worrall's view, are we compelled to discard Young's constructed hypothesis without further notice? Not as such, for as he makes clear what actually went wrong with Young's construction is that "he failed to give the scientific community of his time any reason for the restriction of his principle to the case of 'portions' of light originating from the same source" (Worrall 1985, p. 321) – any reason, that is, other than the fact that this hypothesis 'saves the phenomena'. In other words, there is a tacit acknowledgement here by Worrall that if Young *had* shown the plausibility of the assumption he was using, that is, if he had provided some reason for thinking that interference only applies when "the two interfering 'portions' of light originate from the same source", then Young's maneuver would have been acceptable and his interference principle vindicated. As it happens, Young provided no such defense. By contrast (as Worrall points out) Augustin Fresnel did have a story that could account for the non-occurrence of interference fringes in the two candle case. Fresnel argued that the relevant, two-candle interference patterns would be continuously changing, indeed changing so fast that our eyes are unable to pickup these patterns, thereby creating the illusion of constant illumination. Fresnel's argument, because it is theoretically motivated and not simply ad hoc – that is, not thought up simply to save the phenomena – partially accounts on Worrall's view for the more favorable reception accorded to Fresnel's wave theory over Young's.

Consequently, the moral I think we should take from Worrall's work (*pace* its misrepresentation by Howson) is that when evaluating a hypothesis on the basis of evidence one needs to consider closely the plausibility of the auxiliary assumptions used in connecting the evidence to the hypothesis. Where these assumptions are highly probable, the question whether a hypothesis predicts evidence or is constructed using this evidence is irrelevant. To put this point in terms of the formalism provided earlier, here letting m stand for these auxiliary assumptions, we found that where $P(m) = 1$ there is no benefit to be found in prediction as opposed to accommodation. My contribution in this paper, then, is to go one step further: I am claiming that where such auxiliaries have *low* initial probability, then in fact we do have a benefit with prediction over accommodation. For it is by making a successful prediction that one confirms these less-than-certain auxiliaries, a confirmation which thereby (as we have demonstrated probabilistically) leads to heightened support for the hypothesis given the evidence.

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