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The Whewell-Mill debate on predictions, from Mill's point of view

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ABSTRACT

John Stuart Mill, in his debate with William Whewell on the nature and logic of induction, is regarded as being the first to dismiss the supposed value of successful predictions as merely psychological. I shall argue that this view of the Whewell-Mill debate on predictions misconstrues Mill's position and argument. From Mill's point of view, the controversial point was not the question whether predictions 'count more' than ex-post explanations but the alleged assertion by Whewell that the successful predictions of the wave theory of light prove the existence of the ether. Mill argued that, on the one hand, the predictions of the wave theory of light do not and cannot provide evidence for the existence of the ether; as evidence for the laws of the theory, on the other hand, the predictions are superfluous, the laws being already well-confirmed. Mill actually endorsed a requirement of independent support closely resembling Whewell's requirements for hypotheses; the controversy on the value of predictions is a product of the 20th century.

1. Introduction

The question of the epistemic or methodological value of predictions is thought to divide inductivist and deductivist approaches to scientific method; as Ian Hacking puts it,

> By and large inductivists think that evidence consistent with a theory supports it, no matter whether the theory preceded the evidence or the evidence preceded the theory. More rationalistic and deductively oriented thinkers will insist on what Lakatos calls 'the Leibniz-Whewell-Popper requirement that *the — well planned — building of pigeon holes must proceed much faster than the recording of facts which are to be housed in them*'.[1]

John Stuart Mill, in his debate with William Whewell on the nature and logic of induction, is regarded as being the first to dismiss the supposed value of successful predictions as merely psychological. While Whewell argued that successful prediction, particularly of phenomena of a kind not considered in developing a hypothesis, is a characteristic of (only) true theories, Mill objected that "it is strange that any considerable stress should be led upon such a coincidence by scientific thinkers".[2]

What I shall call the 'received view' of the Whewell-Mill debate on predictions comprises four claims:

1. As opposed to Whewell, Mill held that there is no evidential weight to successful predictions (or, independent evidence).
2. Mill acknowledges that successful predictions may impress laymen, but denies that scientists are, and ought to be, impressed.
3. The question whether successful predictions (or, independent evidence) carry evidential weight is a central, perhaps the central, part of their dispute about induction.
4. The Whewell-Mill debate thus marks the beginning of an ongoing controversy in philosophy of science on the value of predictions.

In this paper, I shall argue that the received view is mistaken on a number of counts: it misconstrues central parts of the debate in general and of Mill's position and his argument against Whewell in particular. The question of predictions has not been central to either Whewell or Mill. From Mill's point of view, the main issue of this part of the debate was not the question whether predictions 'count more' than ex-post explanations but the alleged assertion by Whewell that the successful predictions of the wave theory of light prove the existence of the ether. Mill actually argued, on the one hand, that the predictions of the wave theory of light do not and cannot provide evidence for the existence of the ether; as evidence for the laws of the theory, on the other hand, the predictions are superfluous, the laws being already well-confirmed. Thus, the predictive successes are to be expected and ought not to impress scientists (but probably laymen).

In the following, I shall first outline the received view of the Whewell-Mill debate on predictions in more detail (2). Considering

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[1] Hacking, 1983, p. 115, quoting Lakatos, 1978, p. 100.
[2] Mill, 1974, p. 500 (as in Mill, 1843, II, p. 23).

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Mill's account of the Deductive Method, I shall argue that the received view is at odds with this account, in which he claims the exact opposite of what he supposed to argue against Whewell (3). Subsequently, I shall examine in detail the unfolding of the debate from Mill's point of view (4) and give a new reconstruction of his argument on the value of predictions (5). The reconstruction is supported by a similar argument made by William Herschel in his *Discourse on the Study of Natural Philosophy* of 1830, to which Mill's *Logic* was indebted (6). Finally, I shall consider the methodological implications of the reconstruction for Mill's methodology, the debate at large, and the question of the methodological value of predictions from a more systematic point of view (7).

2. The received view of the Whewell-Mill debate on predictions

In *The Philosophy of the Inductive Sciences*, Whewell discusses three tests of hypotheses. The first test is the correct prediction of all the phenomena a hypothesis was invented for:

> The hypotheses which we accept ought to explain phenomena which we have observed. But they ought to do more than this: they ought to *foretel* phenomena which have not yet been observed; – at least all of the same kind as those which the hypothesis was invented to explain. […] And that it does this with certainty and correctness, is one mode in which the hypothesis is to be verified as right and useful.[3]

The second and more forcible test, which Whewell termed 'Consilience of Inductions', is the prediction of phenomena of a different kind:

> We have here spoken of the prediction of facts *of the same kind* as those from which our rule was collected. But the evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence.[4]

The third test, "hardly different" from Consiliences of Induction, is the "*Simplification of the Theory*":[5]

> In the preceding section I have spoken of the hypothesis with which we compare our facts as being framed *all at once*, each of its parts being included in the original scheme. In reality, however, it often happens that the various suppositions which our system contains are *added* upon occasion of different researches. […] This being the mode in which theories are often framed, we have to notice a distinction which is found to prevail in the progress of true and of false theories. In the former class all the additional suppositions *tend to simplicity* and harmony; the new suppositions resolve themselves into the old ones, or at least require only some easy modification of the hypothesis first assumed: the system becomes more coherent as it is further extended. The elements which we require for explaining a new class of facts are already contained in our system. Different members of the theory run together, and we have thus a constant convergence to unity. In false theories, the contrary is the case.[6]

As examples of the second and third test, Whewell referred to the predictions of Newton's theory of gravitation and particularly of the wave theory of light.

Very likely referring to Whewell's tests of hypotheses,[7] Mill states in the *System of Logic* (or *Logic*, for short) that hardly anybody believes theories like that of the undulatory ether to be probably true merely because it explains the known phenomena;

> But it seems to be thought that an hypothesis of the sort in question is entitled to a more favourable reception, if besides accounting for all the facts previously known, it has led to the anticipation and prediction of others which experience afterwards verified; as the undulatory theory of light led to the prediction, subsequently realized by experiment, that two luminous rays might meet each other in such a manner as to produce darkness. Such predictions and their fulfilment are, indeed, well calculated to strike the ignorant vulgar, whose faith in science rests solely upon similar coincidences between its prophecies and what comes to pass. But it is strange that any considerable stress should be led upon such a coincidence by scientific thinkers.[8]

According to the received view, these passages exhibit a central issue of the controversy between Whewell and Mill: while Whewell regarded successful predictions, in particular of phenomena of a different kind as those which led to the construction of the theory, as characteristic of true theories, Mill held that this criterion was methodologically unsound.

3. Problems: predictions and the Method of Deduction

The received view can hardly be correct: the question of the value of predictions has not been regarded as a central part of the debate until the mid 20th century, and neither by Whewell or Mill; it is quite unclear what Mill's objection to Whewell is, and how it related to his argument; finally, and most importantly, Mill's account of the Method of Deduction seems to contradict his alleged claims in every detail.

Is the question of predictions central to the debate? It is now widely agreed that the question of how much, if any, weight should be attributed to successful predictions is a central point of the Whewell-Mill debate. Some philosophers of science who refer to the debate and are interested in current controversies may probably use it merely as a historical illustration. But the importance of the issue within the debate is also stressed by philosophers with an interest in the history of philosophy of science, or a mixed interest – more precisely, it has become to be stressed. Older accounts of the debate and of Mill's *Logic* often do mention the topic only briefly or not at all.[9] Among the first to refer to it is, not surprisingly, Imre Lakatos, who claimed that Mill was correct in his criticism insofar as Whewell's view is

[3] Whewell, 1840, II, p. 228 (1847, II, pp. 62–63, with minor alteration).
[4] Whewell, 1840, II, p. 230 (1847, II, p. 65); emphasis in original.
[5] Whewell, 1840, II, p. 238 (1847, II, p. 73); emphasis in original.
[6] Whewell, 1840, II, p. 233 (1847, II, p. 68, with minor alteration).
[7] Mill does not mention the third test; as to the second, as Whewell complains in *Of Induction*, he seems to have misunderstood it (see below, section 7). Originally, Mill possibly might refer to several champions of the wave theory of light besides Whewell, as John Herschel or George Airy.
[8] Mill, 1974, p. 500 (as in Mill, 1843, II, p. 23).
[9] For instance, Hitchcock/Sober, 2004 and Worrall, 1989 take the issue to be central; Madden, 1960 and Buchdahl, 1971 do not mention it; Strong (1955, p. 228) mentions it only briefly.

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