History and Scientific Practice in the Construction of an Adequate Philosophy of Science: Revisiting a Whewell/Mill Debate

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Abstract:

William Whewell raised a series of objections concerning John Stuart Mill's philosophy of science which suggested that Mill's views were properly informed neither by the history of science nor by adequate reflection on scientific practices. These objections, if accurate, would be devastating given Mill's expressed aim of constructing a philosophy of science that was grounded on historical achievements in the sciences and the practices driving these developments. The aim of this paper is to revisit and evaluate this incisive Whewellian criticism of Mill's views. I accomplish this task by assessing Whewell's critique of Mill's use of the discovery of electrical induction as an illustration of the Method of Difference. The historical evidence demonstrates that Mill's reconstruction of this discovery is inadequate for many of the reasons Whewell cites. But a study of Michael Faraday's research leading to this discovery also raises some questions about Whewell's own characterization of this historical episode in the science of electromagnetism. Thus, this example provides an opportunity to reconsider the debate between Whewell and Mill concerning the role of the sciences in the development of an adequate philosophy of scientific methodology.

Keywords: methodology, experiment, theory, Michael Faraday, electromagnetism,

electrical induction

1. Introduction

The contentious debates between William Whewell and John Stuart Mill may have concealed a common methodological assumption concerning the role of the sciences in the construction of an adequate philosophy of science.[1] Both Whewell and Mill expressed a desire to build a philosophy of the sciences that was informed by the actual practices and historical achievements of the sciences. Whewell, for instance, discussed the common, and to his mind specious, use of detached examples from various scientific domains in service of a preconceived philosophical framework; he contrasted this approach with his own understanding of scientific knowledge and discovery which was derived from "a connected and systematic survey of the whole range of Physical Science and its History" (Whewell 1847, v. 1, 8). Whewell's *History of the Inductive Sciences* (1857 [1837]), originally published three years prior to his *Philosophy of the Inductive Sciences* (1847 [1840]), presented the historical foundations for his philosophical conclusions concerning scientific inquiry.

Mill, for his part, articulated a similar methodological goal in the introduction to his analysis of induction in the *System of Logic*. He declared,

> What Induction is, therefore, and what conditions render it legitimate, cannot but be deemed the main question of the science of logic—the question which includes all others. It is, however, one which professed writers on logic have almost entirely passed over. The generalities of the subject have not been altogether neglected by metaphysicians; but, for want of sufficient acquaintance with the processes by which science has actually succeeded in establishing general truths, their analysis of the inductive operation, even when unexceptionable as to correctness, has not been specific enough to be made the foundation of practical rules, which might be for induction itself what the rules of the syllogism are for the interpretation of deduction: while those by whom physical science has

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[1] For some discussion of these issues see, in particular, John Losee (1983) and Laura J. Snyder (2002, 2006, and 2008).

> been carried to its present state of improvement—and who, to arrive at a complete theory of the process, needed only to generalize, and adapt to all varieties of problems, the methods which they themselves employed in their habitual pursuits—never until very lately made any serious attempt to philosophize on the subject, nor regarded the mode in which they arrived at their conclusions as deserving of study, independently of the conclusions themselves. (John Stuart Mill 1963, v. 7, 284).

Mill was sincere about developing a philosophy of science that was informed by scientific practice and historical developments in the sciences. In fact, he intentionally delayed writing Book III ('Of Induction') of his *System of Logic* so that he could foster a deeper acquaintance with the history of science—an achievement he explicitly acknowledged could not have occurred without reading Whewell's *History*.[2] Mill also revised the manuscript version of the *System of Logic* so that he could incorporate numerous examples from various scientific domains to illustrate his account of scientific methodology.[3]

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[2] Mill (1963, v.7, cxiii) writes, "Whatever may be the value of what the author has succeeded in effecting on this branch of [induction], it is a duty to acknowledge that for much of it he has been indebted to several important treatises, partly historical and partly philosophical, on the generalities and processes of physical science, which have been published within the last few years. To these treatises, and to their authors, he has endeavoured to do justice in the body of the work. But as with one of these writers, Dr. Whewell, he has occasion frequently to express differences of opinion, it is more particularly incumbent on him in this place to declare, that without the aid derived from the facts and ideas contained in that gentleman's *History of the Inductive Sciences*, the corresponding portion of this work would probably not have been written." For further discussion of Mill's use of Whewell's and Herschel's works see John M. Robson's textual introduction to John Stuart Mill (1963, v. 7, lxiii).

[3] Mill borrowed many of these examples from his friend Alexander Bain. Mill (1963, v.1, 255) wrote, "The only person from whom I received any direct assistance in the preparation of the *System of Logic* was Mr. Bain, since so justly celebrated for his philosophical writings. He went carefully through the manuscript before it was sent to press, and enriched it with a great number of additional examples and illustrations from science; many of which, as well as some detached remarks of his own in confirmation of my logical views, I inserted nearly in his own words."

Although Whewell did not challenge the sincerity of Mill's expressed aim to construct a philosophy of science that conformed to scientific practice, he raised a series of penetrating objections suggesting that Mill's views were properly informed neither by the history of science nor by adequate reflection on scientific practices.[4] He maintained that Mill's philosophy failed to account for the historical progression in the various scientific domains. And Mill did not show that his methods were applicable to obvious, undoubted examples of scientific discovery extending throughout the history of the sciences. The particular examples Mill invoked to illustrate his views were either too recent to admit of adequate development and sufficient understanding, unsubstantiated as genuine discoveries, or misinterpreted as instances of inductive discovery when, in fact, they were extensions of known laws by deduction.[5] Whewell also maintained that Mill's philosophy of scientific methodology was flawed because it failed to provide guidance concerning the use of scientific methods to those actually engaged in research. In particular, Mill did not offer directives for reducing the complex relations among phenomena to sets of antecedent conditions and consequent phenomena. A reduction of this kind was necessary if these methods were to be useful in establishing laws of physical causation—the ultimate goal of inductive inquiry according to Mill.

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[4] See Robert Butts 1963, 288-289.

[5] Laura J. Snyder (2002, 2006, and 2008) argues that Whewell's concern was not simply Mill's failure to infer his methods from historically significant scientific examples but that Mill's methods were not such that they could be inferred from the history of science. In her (2008, 153-54) she contends that Whewell "showed us in his works—through numerous apt examples—that his philosophy has been embodied in the practice of science throughout its history" but "Mill was unable to do so." For an extended analysis of Snyder's defense of Whewell's critique of Mill see [removed for purposes of blind review].

The aim of this paper is to revisit and evaluate this incisive Whewellian critique of Mill's use of the sciences in the construction of his philosophy of scientific method. But rather than focusing on Whewell's objections in the abstract, I consider his assessment of a particular example Mill employed as an illustration of his views on experimental methodology—Michael Faraday's discovery of electrical induction.[6] The historical evidence concerning this discovery shows that Mill's appropriation of Faraday's discovery is inadequate for historical, scientific, and philosophical reasons. At the same time, however, Faraday's published discussion of his research also raises some questions about Whewell's own historical reconstruction of this same discovery. Thus, Faraday's research provides an opportunity to revisit the debate between Whewell and Mill concerning the role of the sciences in the development of an adequate philosophy of scientific method.

The structure of this paper is as follows. In Section 2, I provide an account of Faraday's experimental research and the essential background to this debate. In Section 3, I discuss Mill's appropriation of Faraday's discovery within the context of his *System of Logic*. In Section 4, I explicate Whewell's criticisms of Mill's reconstruction and show that Faraday's published discussion confirms Whewell's concerns. In Section 5, I raise some concerns about Whewell's discussion of Faraday's discovery and his underlying assumption that one cannot understand Faraday's experimental work apart from Ampère's theoretical framework. In Section 6, I conclude with some brief reflections on the implications of this discussion for the debates between Whewell and

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[6] All citations are from Michael Faraday (1956, v. 1, 1-41).

Mill about the normative role of the sciences in the construction of an adequate philosophy of science.

2. Faraday's Discovery of Electrical Induction

In 1831 Faraday discovered that electrical currents, magnets, and electromagnets induce corresponding electrical currents in adjacent bodies.[7] The importance of this discovery for the history of electromagnetism should not be underestimated. Electromagnetism emerged as a distinct scientific domain following Hans Christian Oërsted's (1820) discovery that electrical currents generated by a primitive kind of battery—the voltaic pile—caused deflections of a magnetic needle in its immediate vicinity.[8] The voltaic pile consisted of a series of alternating metallic discs (usually zinc and copper) separated by a cloth or cardboard soaked in brine.[9] The voltaic apparatus generated a novel form of electricity that could be conveyed through a conducting wire connecting the terminals of the apparatus.

Following Oërsted's announcement of this discovery, many scientists including Faraday himself expected to discover the reciprocal effect—that magnets could produce electrical currents. But their experiments failed to produce any results recognized as evidence of the reality of this effect.[10] In his first series of *Experimental Researches in Electricity* (hereafter, "First Series") Faraday published an account of the experiments

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[7] For more on this discovery and its significance see Bern Dibner (1949), L. Pearce Williams (1965), William Berkson (1974), and Geoffrey Cantor (1991).

[8] For some helpful discussions of Oërsted's discovery see R.C. Stauffer (1953 and 1957), L. Pearce Williams (1965), David Gooding (1990), and Roberto De Andrade Martens (2003).

[9] Between Oërsted's discovery and Faraday's discovery many improvements were made to this basic apparatus. In his experiments, Faraday employed a voltaic trough which consisted of two dissimilar metals situated within a trough of fluid.

[10] For a thorough discussion of the experiments conducted during this time and the failure to recognize specific results as indicative of electrical induction see Sydney Ross (1965).

establishing the induction of electrical currents by means of a voltaic apparatus, ordinary magnets, and electromagnets. After nearly a decade of failed experimental research throughout various scientific communities in Europe, Faraday demonstrated conclusively that magnets could produce electrical currents. The central, and unexpected, feature of this discovery was the fact that induced electrical currents were momentary rather than continuous currents. Faraday, like his contemporaries, had expected that induced electrical currents would be continuous, but his experiments indicated otherwise.[11]

Before turning to Mill's appropriation of Faraday's discovery, it is important to note that Faraday situated the discovery of induced electrical currents within an established theoretical framework. At the time Faraday conducted his research, the prevailing theory under which many electrical and magnetic phenomena had been subsumed was André-Marie Ampère's electrodynamic theory.[12] Ampère devoted considerable attention to electromagnetic phenomena immediately after the announcement of Oërsted's discovery. Within a few short months, Ampère discovered some novel effects that inspired a bold theoretical account of the entire range of known electromagnetic phenomena. One of the most important discoveries was that Oërsted's results could be replicated without utilizing a magnetic needle in one's experimental system. This indicated that electricity could produce magnetic effects without employing magnetic substances and convinced Ampère that all magnetic phenomena could be explained in terms of the activity of electrical currents. In its ultimate form, Ampère's theory posited that magnetic phenomena were the result of electrodynamic currents

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[11] For an interesting discussion of the effect of Faraday's research on transient phenomena in distinct scientific domains on this discovery see Ryan Tweeney (1985).
[12] For more discussion of Ampère's work see James Hoffman (1987 and 1996).

rotating around the molecules of so-called magnetic substances. Faraday's discovery that voltaic currents, ordinary magnets, and electromagnets produced similar effects strengthened the empirical case for Ampère's reduction of magnetic phenomena to electrical causes. In fact, Faraday (1956, v.1, 16) maintained that his experimental results were "strikingly in accordance with and confirmatory of M. Ampère's theory, and [furnish] powerful reasons for believing that the action is the same in both cases."

But Faraday's explicit discussion of the theoretical implications of his results focused upon two specific concerns: (i) the possible causes for the transient nature of induced electrical currents and (ii) the laws governing both induced electrical currents and related electromagnetic phenomena. With respect to the former, Faraday proposed that the momentary nature of induced electrical currents could be explained by the hypothesis that the wire under induction assumes a "peculiar state"—the electro-tonic state—that resists the production of a continuous electrical current. Faraday described the electro-tonic state as a momentary state acquired by the wire under induction whose existence was dependent upon the activity of inducing currents or magnetic substances. Although Faraday confessed that there was no independent evidence grounding the reality of this proposed state, he noted that adopting it as a hypothesis could provide an account of a mechanism by which an identical cause could produce this diverse array of effects. Faraday's reflection could be interpreted as an attempt to extend Ampère's theoretical framework by supplying a hypothesis concerning the manner in which electricity might act as the underlying cause of induced electrical currents in each experimental trial.

But Faraday followed this speculative discussion of the electro-tonic state with an extensive discussion of the laws governing both induced electricity and related electromagnetic phenomena.[13] At the time, many of Faraday's contemporaries thought that these phenomena were completely mysterious. The phenomena in question involved the magnetic effects of substances scientists generally thought were essentially non-magnetic. In particular, Dominique François Arago had discovered that a rotating copper disc caused a magnet delicately suspended above it to rotate in a corresponding direction. But copper in its natural resting state exhibited no magnetic qualities. Charles Babbage and John F.W. Herschel discovered the reciprocal effect that rotating magnets caused metallic discs in their immediate vicinity to rotate in a corresponding direction.[14] But when these magnets and metallic discs were at rest, they exhibited no detectable magnetic effects.

Faraday argued that the key to explaining these effects was the transient nature of induced electricity. He showed that the motion of any metal around the pole of a magnet gives rise to electrical currents that move in a transverse direction across the metal. Faraday (1956, v.1, 34) writes, "All these results show that the power of inducing electric currents is circumferentially exerted by a magnetic resultant or axis of power, just as circumferential magnetism is dependent upon and is exhibited by an electric current." Thus, in Arago's original experiments, the constant rotation of the magnets in a corresponding direction to the rotating discs is explicable in terms of the currents generated by the motion of the rotating discs around the pole of the magnets in the experimental system. Faraday's experimental research established the exact nature of

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[13] For an extensive discussion of this aspect of Faraday's research see Steinle (1994).
[14] See Charles Babbage and John F.W. Herschel (1825).

induced electrical currents and generated a correct understanding of the laws governing both the phenomena of induced electricity and the magnetic effects of rotating discs.

3. Mill on the Discovery of Electrical Induction

Mill situated Faraday's discovery of electrical induction within an extended discussion of the "theory of induced electricity" (1963, v.7, 410ff). In particular, Faraday's experimental research generated evidence that, when combined with other experimental results, enabled one "to find under what conditions any electrified body, whether positively or negatively electrified, gives rise to a contrary electric state in some other body adjacent to it" (John Stuart Mill 1963, v.7, 410). Mill claimed that Faraday's results illustrated his understanding of the Method of Difference. But it is important to note that Mill was not seeking to give a detailed reconstruction of Faraday's discovery or Faraday's purposes in conducting his experimental research. Rather, Mill utilized Faraday's work because it illustrated his understanding of a scientific method and, thereby, served as an example of the significance of this method for the justification of a proposed explanatory law.[15]

Mill contends that the purpose of Faraday's research was to determine whether a conducting wire carrying a voltaic current would produce an opposite electrical current on another conductor in its vicinity. But Mill maintains that Faraday's research was founded on the assumption that common, or static, electricity is identical with the electricity occasioned by a voltaic apparatus. He writes,

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[15] Struan Jacobs (1991) and Steffen Ducheyne (2008) have argued that in later editions of Mill's *System of Logic*, Mill abandons the notion that his methods are methods of discovery. Instead, they are methods of the justification of proposed laws governing phenomena.

> Since common or machine electricity, and voltaic electricity, may be considered for the present purpose to be identical, Faraday wished to know whether, as the prime conductor develops opposite electricity upon a conductor in its vicinity, so a voltaic current running along a wire would induce an opposite current upon another wire laid parallel to it at a short distance. (John Stuart Mill 1963, v.7, 413)

The results of Faraday's experiments were different from the other cases Mill discussed in connection with the law of induced electricity in that all the other cases involved an electric body inducing an opposite and continuous electrical state in neighboring bodies. Faraday's results in this experiment, according to Mill, showed that the opposite electrical state was produced within the primary conducting wire itself. Mill observed,

> From the nature of a voltaic charge, the two opposite currents necessary to the existence of each other are both accommodated in one wire; and there is no need of another wire placed beside it to contain one of them.... (John Stuart Mill 1963, v.7, 413)

Thus, in the case of Faraday's experiments, Mill believed that the "exciting cause can and does produce all the effect which its laws require, independently of any electric excitement of a neighbouring body" (1963, v.7, 413). Mill took the transient effects Faraday had produced to be phenomena of a different kind than ordinary induced electricity. If the term 'induced electricity' refers only to those cases in which the electricity in one body brings about an opposite and continuous electrical state in a neighboring body, then Faraday's discovery of a momentary effect should not be understood as a case of 'induced electricity'.

Reconstructing Mill's discussion shows that one can construe Faraday's research in accordance with the method of difference. The method of difference involves comparing instances in which a phenomenon occurs with those instances in which it fails to occur in order to discover the particular antecedent condition absent in those cases in which the phenomenon in question fails to occur. In this case, the contrasting classes are those cases in which the phenomenon of induced electricity in a neighboring body is present and those in which it is fails to occur. Recall that 'induced electricity' is a technical term for Mill, referring to the class of phenomena in which one electrified body gives rise to an opposite and continuous electrical state in a neighboring body. Faraday's discovery of the transient effect produced by a voltaic current does not fall within this class because the current-carrying wire does not induce a continuous electrical current in a neighboring body. The circumstantial difference between this instance and those in which induced electricity occurs is that the current-carrying wire in the latter produces the opposite states within itself rather than in a neighboring body. Thus, Faraday's discovery supports Mill's understanding of the law of induced electricity which holds that the production of one kind of electricity depends upon the correlative production of the opposite electricity in a body in the immediate vicinity of the original current.

4. Whewell's Critique of Mill

Whewell summarily dismisses Mill's discussion of the theory of induced electricity as demonstrating a general ignorance concerning some of the simplest doctrines within the domains of electricity and magnetism. And he argues that Mill's reconstruction of Faraday's discovery is a gross mischaracterization of Faraday's explicit views concerning induced electrical effects.[16] After all, Whewell argues, Faraday did not assume the identity of common electricity and voltaic electricity. Furthermore, to say that this assumption was part of the rationale for Faraday's research mischaracterizes the

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[16] See Robert Butts 1963, 288-289.

explicit research agenda Faraday announces at the start of his research. The goal was to discover whether magnets could produce electrical effects since electrical currents could produce magnetic effects. This in no way depended upon assuming the identity of static and voltaic electricity. And Mill's failure to mention Faraday's actual reasons for conducting this research suggests a failure on Mill's part to appreciate the historical significance of Faraday's discovery within the domain of electromagnetism. Whewell also notes that, contrary to Mill's interpretation, Faraday was agnostic about the reality of opposing electrical currents existing in the conducting wire. Although this was an essential commitment of Ampère's electrodynamic theory, Faraday refused to assent to this hypothesis throughout his career.[17]

Whewell does not reveal the historical and philosophical underpinnings of this critique in his explicit discussion of Mill's work, but attention to his reconstruction of Faraday's discovery in his (1847) *Philosophy* will provide a deeper understanding of his dissatisfaction with Mill's views. For Whewell, Faraday's research is but one stage of the progression in the mechanico-chemical sciences towards a more clear and defined notion of polarity. The initial, and vague, use of this idea occurred originally in the context of magnetic phenomena but was later expanded to account for the effects of static electricity. But there were important differences between magnets and electrical conductors; electrical phenomena exhibited the general nature of polarity without localized physical poles.

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[17] Although Faraday recognized that Ampère's electrodynamic theory was well-grounded and fruitful, he had great reservations about accepting any theoretical account of the causes of electromagnetic phenomena at the early stages of research within this domain. In fact, he engaged in correspondence with Ampère in the early years of the 1820s about his on-going concerns with Ampère's views. For more on this F.A.J.L James (1991, 252, 287-288) and L. Pearce Williams (1985).

So, theorists had to find another way to account for these distinct kinds of polarity without referring them to localized and permanent points in a substance. In this context, Whewell claims that theorists accounted for electrical polarity by proposing that there were two kinds of electrical fluid distributed through various physical bodies.[18] While theorists managed to account for all of the known laws of electricity by employing this hypothesis, the voltaic apparatus produced new kinds of polar relations distinct from those exhibited by static electricity. These new phenomena were not explicable in terms of a two-fluid theory of electricity and, as such, scientists had to develop a novel understanding of the idea of polarity which was not dependent upon any of the available hypotheses concerning mechanisms or processes responsible for producing phenomena exhibiting polarity. Whewell writes,

> Thus, we too may remark, all the superfluous and precarious parts gradually drop off from the hypothesis which we devise in order to represent polar phenomena; and the abstract notion of polarity—of equal and opposite powers called into existence by a common condition—remains unincumbered with extraneous machinery. (William Whewell 1847, v. 1, 349)

The primary importance of electromagnetism in this context was that it provided evidence of a connection between distinct kinds of polarities. Oërsted's vague notion of a connection between the polarities exhibited by electricity and magnetism was refined by experiment until they came to be seen as manifestations of one and the same cause—the polarity of Ampère's electrodynamic current. Thus,

> the vague and obscure persuasion that there *must* be *some* connexion between electricity and magnetism, so long an idle and barren conjecture, was unfolded into a complete theory, according to which magnetic and electromotive actions are only two different manifestations of the same forces; and all the above-mentioned complex relations of polarities are

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[18] Whewell 1847, v. 1, 347.

> reduced to one single polarity, that of the electro-dynamic current.
> (William Whewell 1847, v.1, 361)

In Whewell's mind, the principle of the connection between distinct kinds of polar phenomena was a regulative factor guiding and directing research within the domain of electromagnetism; it was not the mere summation of experimental observations but an extra-experiential conception framing research and giving form to experimental results. Ampère's theoretical framework provided the necessary refinement of this idea by reducing distinct kinds of polarity to the polarity of the electrodynamic current. Accordingly, Whewell maintains that Ampère's conceptual framework became an instrument of reasoning for Faraday in his experimental work. He suggests that Ampère's views provided the essential framework from which Faraday conceived and conducted his experimental work. Thus, one cannot understand the nature of Faraday's discovery of electrical induction without attending to Ampère's electrodynamic theory.

Against Mill, Whewell maintains that one cannot clearly discern the nature of Faraday's experimental research on induced electrical effects without attending to the generating theoretical context. Ampère's well-established electrodynamic theory served as a significant motivating influence directing Faraday's research and an essential element of his discoveries. As such, Mill's account mischaracterizes Faraday's motives, his findings, and their historic and scientific significance. Mill's use of this example fails because there is no relationship between the historical and experimental reality of Faraday's research and Mill's own philosophical reconstruction.

5. Reassessing Whewell's Critique

But Faraday's discussion also raises some interesting questions about Whewell's characterization of the theoretical underpinnings of Faraday's research. In particular, the

"First Series" does not establish the exact extent to which Faraday's research was dependent upon Ampère's electrodynamic theory. Was Ampère's theoretical framework merely the theoretical framework that initially motivated Faraday's experiments, or was it a guiding conceptual framework from which Faraday drew suggestions for experimental research or changes in experimental parameters, or, more substantively, was it essential to Faraday's understanding and description of the nature and causes of induced electrical currents? While Mill clearly failed to note the Ampèrian context of Faraday's research, Whewell may have overemphasized the extent to which Faraday's research depends upon Ampère's views. As such, Whewell may have overlooked the ways in which Mill could have employed Faraday's work to illustrate his understanding of scientific methods and his general understanding of the philosophy of science.

While the significance of Ampère's electrodynamic theory should not be understated, Faraday's introductory remarks in his "First Series" suggests that confirmation of Ampère's views may not have been paramount in his mind. At the outset of his paper, Faraday states explicitly that it was a "curious fact" that no one had shown that magnets could induce electrical currents. The fact that no one had discovered these effects was curious, Faraday writes, "Whether Ampère's beautiful theory were adopted, or any other, or whatever reservation were mentally made" (Faraday 1956, v.1, 2). The important point to note here is that the failure to discover the reciprocal effect was curious because of the implications of Oërsted's discovery alone. While Ampère's views were an important part of the context of Faraday's discovery and Faraday's results corroborated Ampère's theory, the significance of Faraday's results could be understood independent of Ampère's theoretical framework.[19]

And Faraday's discussion of the theoretical implications of his discovery shows that Faraday was concerned primarily with accounting for the conditions necessary for the reliable production and understanding of the basic effect and providing a unifying explanation of the laws governing induced electricity and other related phenomena—primarily Arago's phenomena. His speculation about the underlying electro-tonic state of matter, although important for understanding his initial conjectures about the causes of the phenomena in question, was not as important to the account he offers in the "First Series". And Faraday abandoned this hypothesis of the electro-tonic state by the time he published his *Second Series of Experiments in Electricity* just two months after the "First Series". In a footnote added to the section recording his speculations concerning the electro-tonic state, Faraday (1956, v.1, 16) writes, "…later investigations…of the laws governing [induced electrical currents], induce me to think that [these phenomena] can be fully explained without admitting the electro-tonic state. My views on this point will appear in the second section of these researches." Faraday's attempt to understand the laws governing induced electrical currents led him to abandon his earlier conjectures

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[19] Although I did not discuss Faraday's account of the direction of the induced electrical current in his research, this was an important part of his research. In fact, Faraday's initial account of his discovery of electrical induction included in a letter to his friend Richard Phillips contained a significant error concerning the direction of the induced electrical current. Josè Romo and Manuel G. Doncel (1994) argue that Faraday's error stems from his reliance upon expectations derived from Ampère's electrodynamic theory. It was only through subsequent experimental research that Faraday was able to correct his erroneous account of the direction of the induced electrical current. This provides another piece of evidence that Faraday's experimental research is separable from Ampère's views since it was only be conducting his research that Faraday was able to correct the misleading Ampèrian understanding of the effect.

about this hypothetical state. Thus, the explanatory claims he made concerning the laws governing induced electrical currents enjoyed a much higher degree of epistemic credibility in Faraday's mind.

Finally, when one attends to the experiments Faraday performed, especially in his research on Arago's rotating discs, one sees that Faraday's primarily goal is to explore the relationship between the systematic adjustment of experimental parameters and the resulting effects within his experimental system. By the time Faraday was working on these questions, he had developed a stable understanding of the basic or simple case, of induced electricity; he knew how to produce it reliably and which sets of antecedent conditions were essential to the production of this effect.[20] His subsequent experimentation explored the relationship between this basic effect and other related phenomena with an eye to understanding the laws governing these phenomena. Even if Ampère's views were essential to the motivations of Faraday's research program, by the time Faraday had achieved an experimental understanding of an induced electrical current as a basic effect, his goal was not to deepen the support for Ampère's theoretical views. His results could be used to corroborate Ampère's views, but Faraday was much more concerned about articulating a precise account of the basic effect, the conditions for its reliable production, and a general account of the laws unifying this phenomenon with other related phenomena.

Faraday was also interested in technological advancements he could produce as a result of his understanding of these effects and their governing laws. His research had indicated that one could produce a continuous flow of electrical currents through the

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[20] For much more on this aspect of Faraday's work see Steinle (1994, 1997, and 2002).

continuous motion of magnets. This was an important discovery because up until this point, the production of dynamic forms of electricity was inhibited by an apparatus whose power diminished over time. The prospect of producing a continuous flow of electrical currents with an apparatus that would not lose its power was an exciting possibility.

Hence, although Whewell is correct to note the importance of Ampère's work in providing a faithful historical reconstruction of Faraday's work, it is not clear that Faraday's experimental research itself, his understanding of the results, and his articulation of the theoretical implications of these results were fundamentally dependent upon Ampère's views. The experiments themselves, the decisions necessary to make systematic adjustments to experimental parameters, the recording of significant results, and the attempts to explain related phenomena—all of these aspects of Faraday's work, it appears, are separable from the Ampèrian context even if they are relevant to case one can make for Ampère's electrodynamic theory. As such, it is not clear that a philosophy of science must take into account the Ampèrian theoretical framework in order to give an adequate account of the nature and significance of experimental practices in the context of this discovery. However flawed Mill's actual discussion was it is not clear that Mill's failure to appreciate Ampère's electrodynamic theory automatically undercuts the philosophical adequacy of his views concerning scientific methodology.

But are there aspects of Faraday's research that Mill could employ in support of his philosophy of scientific methodology? Answering this question requires a more nuanced account of Mill's mature philosophical views and, thus, it is instructive to consider Mill's response to Whewell's general criticisms recounted above in Section 1. Mill contends that his sole aim in providing an inductive logic was

> to provide rules and models…to which if inductive arguments conform, those arguments are conclusive, and not otherwise. This is what the Four Methods profess to be, and what I believe they are universally considered to be by experimental philosophers, who had practised all of them long before any one sought to reduce the practice to theory. (John Stuart Mill, 1963, v. 7, 430).

His methods, then, should not be taken as methods of discovery; rather, they are formal methods by which one can test whether a discovery satisfies the canons of inductive inquiry. Progress cannot be made in the sciences until one possesses a canon for assessing whether some body of evidence inductively justifies a general explanatory claim.

Furthermore, Mill maintains that Whewell failed to appreciate the idealized context presupposed by his discussion of experimental methods. This context requires that one abstract from the necessarily complicated fabric of causal relations in nature in order to isolate specific causes and their effects. Within this context, Mill's methods should be understood as initial steps in the attempt to satisfy the ultimate aim of inquiry—the unification of lower-level laws under general laws of nature. But in both the idealized context presupposed in his discussion of experimental methods and the actual contexts of inquiry where one must employ deduction and hypothesis in addition to inductive methods, the primary function of experimental methods is to provide a canon for assessing the conclusiveness of one's evidence. So, Mill's view is that the experimental methods he outlines in the *System of Logic* provide a canon for assessing whether some data (i.e., experimental effects) inductively support a general explanatory claim made on the basis of this data.

Given this qualified understanding of Mill's account of scientific methodology, I maintain that there are elements of Faraday's research that Mill could have employed to illustrate his understanding of scientific methodology. First, it is clear that the central goal of Faraday's work, following his discovery of induced electrical currents and his experimental understanding of the conditions essential to its production, was to provide substantive evidence for his explanatory claims concerning the laws governing these effects. This accords within Mill's understanding of the ultimate goal of the sciences. Second, the results generated by Faraday's numerous experimental trials and the systematic adjustment of experimental parameters served to justify his more general claims concerning the governing laws of these phenomena. Again this agrees with Mill's view that the primary function of experimental methods is to justify general explanatory claims concerning the laws governing phenomena. Third, Mill's commentary on the complexity of actual scientific inquiry and the role of scientific methods in insuring that one has isolated particular threads of causation accords with Faraday's own practice in this context. Faraday demonstrates an acute awareness of the complex causal nexus of his various experimental systems. For this reason, he took steps to make sure that his experimental results were neither spurious nor misleading by replicating trials, adjusting parameters, and constructing new apparatus to reproduce the results in independent contexts. These tests helped him to secure his results from subsequent defeat by insuring that they were not mere artifacts of his experimental system. Thus, at minimum Mill's views are consistent with many elements of Faraday's experimental practice.

Given that Mill's purpose in employing actual scientific examples was primarily illustrative, it is instructive to consider whether Mill could have employed any of Faraday's actual experiments reported in the "First Series" to illustrate his views. I conclude this section by considering one example that would effectively illustrate Mill's method of difference. One of Faraday's experiments demonstrating that magnets could produce induced electrical currents involved the following experimental system. Using a hollow cylinder of pasteboard, Faraday constructed compound helices from eight lengths of copper wire. Four of these wires were connected together forming one compound helix (Helix A) and the other four wires formed a distinct compound helix (Helix B). Faraday connected the ends of these elementary helices to a magnetic needle whose deflections would serve to indicate the presence of electrical currents. Using a permanent cylindrical magnet, Faraday inserted one end of this magnet into the axis of the helix and when the galvanometer needle was stationary he thrust it into the cylinder. He left the magnet in until the galvanometer came to its original position and then withdrew it and the needle exhibited a deflection in the opposite direction.

Given that the indicating device was a magnetic needle, Faraday had to insure that the magnets used in these experiments would not cause the deflection directly. He had to prove that the deflections were the result of induced electrical currents not the direct magnetic action of the magnet he employed in his experiments. Faraday observed,

> All care was taken to guard against any direct action of the inducing magnet upon the galvanometer, and it was found that by moving the magnet in the same direction, and to the same degree on the outside of the helix no effect on the needle was produced. (Faraday 1956, v. 1, 12)

Since these magnets did not cause the deflection directly from their motion outside the axis of the helix, the inductive effects could not be attributed to the direct action of the magnets. Thus, Faraday concluded that the deflections were produced by induced electrical currents.

One can reconstruct Faraday's support for this claim in accordance with Mill's method of difference. In the first trial, the initiation of motion of a permanent magnet within the axis of the helices resulted in the deflection of the indicator needle. In the second trial, the initiation of motion of a permanent magnet outside of the axis of the helices did not produce deflections of the galvanometer needle. Since the deflections occurred in the first trial and did not occur in the second trial, one must trace this difference to one of the antecedent conditions. This difference was in the location of the magnet when Faraday initiated motion with the magnet. Given that there was no direct action of the magnet on the galvanometer needle, these experiments supported the general claim that the deflections in the first case were the result of induced electrical currents produced by the motion of the magnet within the axis of the compound helices. Note that this reconstruction of Faraday's research is consonant with Mill's understanding of method of difference and his general idea of the role of experimental methods in the justification of general claims.

6. Conclusion

Whewell was right to challenge the scientific grounding of Mill's philosophy of science. Mill's failure to engage with the complexities of the history of science and his indirect acquaintance with scientific practices led him to mischaracterize specific episodes in the history of science. Close attention to Faraday's discovery corroborates Whewell's critique and warrants a rejection of Mill's reconstruction of Faraday's research. But a study of Faraday's research also cautions against an expeditious acceptance of Whewell's understanding of the dependence of this discovery upon Ampère's theoretical framework. While Whewell is correct to note the Ampèrian theoretical context that informed Faraday's research, it is not clear that Ampère's views were essential to the entire range of Faraday's experimental practices, his formulation and articulation of the laws governing the phenomena in question, and the explanatory implications he took to be central to his discovery. And the fact that these are separable from Ampère's conceptualization of electromagnetic phenomena allows for a reconsideration of the merits of Mill's philosophy of science distinct from his actual inaccurate discussion of Faraday's discover.

In the previous section, I showed how Mill could have employed some of Faraday's work to illustrate the nature and commitments of his philosophy of science. The upshot of this discussion is the following. Whewell can legitimately claim that there is little inductive support for Mill's philosophy of science. He cannot claim, however, that Mill's philosophy of science is not inferable from the history and practice of science. There are several aspects of Faraday's experimental work that accord with Mill's understanding of the structures, aims, and methods of the sciences. As such, Mill's views are consistent with scientific practices in this particular historical episode. Hence, it is possible to present evidence from the history and practice of science that grounds Mill's philosophical conclusions concerning scientific practice.

Whewell may respond that the agreement between a philosophy of science and several instances of experimental practice within one domain of the sciences does not establish the adequacy of this philosophical framework. One must accumulate a great deal more evidence from the history of science and throughout various domains in the sciences in order to establish one's philosophical conclusions. While this rejoinder is fair, it does not demonstrate that Mill's philosophy of science is not inferable from the sciences. In order to show this, it seems, Whewell must show that it is impossible for Mill to derive support from the history and practice of the sciences for his views. Mill's philosophy of science, properly understood, can derive support from specific episodes in the history of science and scientific practice.

But there are several other factors that proponents of this Whewellian critique may invoke in defense of their claim that Mill's views lack substantive foundations in the history and practice of the sciences. They could respond that Mill's philosophy of science understood thus at best characterizes just a preliminary stage of scientific inquiry. And, in this respect, it is not clear that Mill's philosophy is uniquely supported by the evidence from Faraday's research because Whewell himself thinks that discovery of laws is the penultimate goal of scientific inquiry. Even though Whewell and Mill disagree about the ultimate goal of scientific inquiry, they agree that a paramount goal of the sciences is the discovery and justification of explanatory laws. As such, the fact that Faraday's primary goal in this research was to discovery and ground a precise articulation of the laws of induced electricity does not establish that Faraday thinks of this as the ultimate goal of inquiry. And Faraday's speculative proposals concerning the electro-tonic state suggest that, *pace* Mill, Faraday does not think of the justification of laws as the ultimate goal of scientific inquiry.

What this response suggests is that whether Mill's or Whewell's philosophy of science is inferable from the history and practice of the sciences depends upon whether their respective philosophical views adequately characterize the processes and methods of the sciences leading to the discovery and justification of scientific knowledge claims. Answering this question depends in part on the function and epistemic status of theoretical conceptions in scientific inquiry. For Whewell, these conceptions are in part constitutive of scientific knowledge, but as we have seen it is not clear that the historical record in the Faraday case confirms this account. While his basic philosophical stance may accord with numerous examples throughout the history of science, my study of Faraday's discovery raises questions about the theory-dependence of Faraday's experimental research. It simply is unclear how the entirety of Faraday's guided by Ampère's theoretical views. It certainly is plausible to think that Ampère's conceptual framework was an important generating source for Faraday's research, but the detailed experimental work Faraday performed and his careful reasoning from his experimental results suggest that much of this work could be understood apart from the generating theoretical context.

The general conclusion one ought to infer from this discussion is that much more work needs to be done in order to articulate the normative role of the history and practices of the sciences in the construction of an adequate philosophy of science. While there are clear cases where a philosophy fails to accord with the history and the practice of the sciences, until there is a precise and well-grounded account of the function of the history and practice of the sciences in the development of a philosophy of science, it is not clear how one can determine whether, and to what extent, a particular philosophy of science is adequately-informed by the sciences.

ACKNOWLEDGEMENTS

I would like to thank Kent W. Staley, Scott Ragland, and John Greco for feedback on an earlier version of this article.

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