# What Mill Could (and Should) Have Said about Faraday's Discovery of Electrical Induction

Aaron D. Cobb
Department of Philosophy
Saint Louis University
3800 Lindell Boulvard
Saint Louis, MO. 63074
adcobb@gmail.com
(314)494-6046

## Abstract:

John Stuart Mill's awareness of the sciences was, at best, indirect and this has led to serious criticisms of the adequacy of his philosophy of science. In this work I reconsider Mill's views by discussing his account of the theory of induced electricity and, in particular, his appropriation of Michael Faraday's discovery of electrical induction as an illustration of the Method of Difference. Although Mill's discussion contains serious errors, I propose an alternative account of what Mill could, and perhaps should, have said which is consonant with both his understanding of the philosophy of science and Faraday's experimental research.

## 1. Introduction

One of John Stuart Mill's explicit goals in the *System of Logic* was to construct a philosophy of science that was grounded in the historical achievements and practices of the sciences. Concerning his project, Mill observed that

> the task to be performed was that of generalizing the modes of investigating truth and estimating evidence, by which so many important and recondite laws of nature have, in the various sciences, been aggregated to the stock of human knowledge. (John Stuart Mill 1963, v.7, cxii)

To this end, Mill delayed writing Book III ('Of Induction') until he could foster a deeper acquaintance with the history of science—a process which he explicitly acknowledged involved a serious study of works written by his contemporaries John F. W. Herschel and William Whewell.[1] He also revised the manuscript of the *System of Logic* to include scientific examples that illustrated the experimental methods factoring prominently in his understanding of the sciences. In his *Autobiography*, Mill disclosed that he owed his description of many of these examples to his friend Alexander Bain who was an accomplished scientist with deeper understanding of the sciences.[2]

The fact that Mill's awareness of the sciences was, at best, indirect has led to serious questions about the adequacy of his philosophy of science. Whewell, for example, in a letter to Herschel wrote the following:

---

[1] See John Stuart Mill (1963, v.7, cxiii). Mill was referring to Whewell's (1857 [1837]) *History of the Inductive Sciences*.

[2] Mill (1963, v.1, 255) writes, "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."

> Jones will tell you of a new book by young Mill about the philosophy of science, suggested in a great degree by your book on the same subject and by mine. There is in new books of this kind a satisfaction in which both you and I may have a share. I mean that notions and expressions, which were new and strange when we began to write, are now familiarly referred to as part of the uncontested truth of the matter. Mill agrees with you more than with me in the parts where we differ, but he does not appear to me an ally to set much store by; for though acute and able, he is ignorant of science and still entangled in the prejudices of a bad school. (Isaac Todhunter 1876, v.2, 315)

Later, in his 1849 critical review of the *System of Logic*, Whewell expressed grave doubts about Mill's methods and their application to undisputed examples of scientific achievement across a wide array of scientific domains throughout the history of science.[3] In this work I reconsider Mill's philosophy of science and its standing with respect to the history and practice of the sciences. But rather than focusing on this issue in the abstract, I concentrate on Mill's discussion of the theory of induced electricity and, in particular, his use of Michael Faraday's discovery of electrical induction.[4] Mill appropriates Faraday's research as an illustration of the Method of Difference. But his description of Faraday's discovery contains significant factual errors concerning the theoretical context of Faraday's work and the research agenda Faraday was pursuing. It also presents an inaccurate account of the results of Faraday's experiments. Hence, this study seems to confirm the charge that Mill's philosophy of science lacks sufficient grounding in the history and practice of the sciences.

In this project, however, I propose an alternative account of what Mill could, and perhaps should, have said concerning Faraday's experimental research. Much of

---

[3] For a defense of this Whewellian critique see Laura J. Snyder (2002, 2006, and 2008). For an analysis and appraisal of Snyder's defense see [removed for purposes of blind review].

[4] For Faraday's discovery see Michael Faraday (1956, v.1, 1-41).

Faraday's discussion is consistent with Mill's understanding of the function of experimental methods and the nature of explanation in the philosophy of science. Faraday's work provides ample evidence of a piecemeal inquiry involving the systematic adjustment of experimental parameters in the attempt to disclose law-governed relationships between phenomena. This approach fits Mill's understanding of the purposes of a logic of induction—namely, the justification of laws of physical causation. Furthermore, there are examples from Faraday's experimental work that could effectively illustrate Mill's understanding of experimental methods. So, even though Mill's actual discussion of the theory of induced electricity contains numerous errors, he could have appropriated Faraday's research to show that his philosophy of science is exemplified by the sciences.

The structure of this paper is as follows. In Section 2, I briefly describe Mill's understanding the nature of the sciences and the role of experimental methods in the satisfaction of the aims of science. In Section 3, I discuss Mill's account of the theory of induced electricity as an illustration of his philosophy of scientific methodology. In Section 4, I critique Mill's reconstruction by drawing upon Faraday's published discussion of his discovery. In Section 5, I consider how Mill could have put Faraday's research to better use within the overarching context of his own philosophy of science. In Section 6, I conclude this paper by discussing whether my reconstruction effectively answers the charge that Mill's philosophy of science lacks sufficient grounding in the sciences.

## 2. Mill's General Philosophy of Science Explained

Mill provides the following as a description of his understanding of the aims of scientific inquiry:

> To ascertain, therefore, what are the laws of causation which exist in nature; to determine the effect of every cause, and the causes of all effects,—is the main business of Induction; and to point how this is done is the chief object of Inductive Logic. (John Stuart Mill 1963, v.7, 378)

Given the centrality of causation to his understanding of the aims of inductive inquiry, Mill recognizes that he must articulate a precise and determinate account of causation. So, Mill states explicitly that he intends causal terminology to refer only to physical causes—that is, causes that are themselves phenomena.[5] Formally, Mill defines 'cause' as an antecedent phenomenon invariably connected with some consequent phenomenon.[6] But he recognizes that it is often difficult, and perhaps impossible, to isolate a singular antecedent phenomenon that invariably precedes any consequent phenomena. So, he argues that the entire set of conditions and circumstances antecedent to a consequent phenomenon are indispensable to the occurrence of the consequent phenomenon. This set includes those "negative conditions" that would prevent the consequent phenomenon from occurring were they to be present.

Mill further clarifies this invariability account of causation so that he can provide a principled distinction between a genuine causal relation and the mere accidental concomitance between antecedent and consequent phenomena. Discerning a genuine causal relation between antecedent and consequent phenomena requires separating those antecedent conditions upon which the occurrence of consequent phenomenon depends

---

[5] John Stuart Mill 1963, v.7, 326.

[6] Although inconsequential from his perspective, Mill extends his analysis to allow for the possibility that a cause brings about its effect simultaneously rather than sequentially. See John Stuart Mill 1963, v. 7, 344.

from those which make no genuine difference to its occurrence. Ultimately, Mill concludes that in addition to being invariably antecedent to a consequent phenomenon, a cause must also be *unconditionally* antecedent to a phenomenon. More formally, an invariable connection between some set of antecedent phenomena and a consequent phenomenon is a causal connection just in case the consequent phenomenon depends for its occurrence *solely* upon the set of conditions constituting the antecedent phenomena.

In the context of actual scientific practice, Mill argues that there are three steps to the process of discovering physical causes of this kind and their governing laws.[7] In the inductive step, one aims to establish the laws of phenomena through the use of eliminative methods. In the ratiocinative step, one derives a prediction about what will occur in a particular instance given these laws of physical causation and the relevant circumstances. In the verification step, one tests this prediction to see if experience confirms the truth of the law. Mill recognizes the importance of hypothetical speculation in the sciences.[8] But he contends that the scientific use of hypotheses involves a provisional dispensation of the inductive stage of inquiry. Scientists who employ hypotheses reason on the basis of a presumed physical cause or law, derive empirical predictions, and attempt to verify these predictions through experimental tests.

Mill commits himself to an empiricist epistemology of the sciences. He rejects all forms of *a priori* philosophy, believing that it is the source of gross errors and prejudice.

---

[7] See, in particular, John Stuart Mill 1963, v.7, 454ff.

[8] Struan Jacobs (1991) argues that in later editions of the *Logic*, hypotheses take on a more prominent role in Mill's understanding of the processes of discovery and justification of explanatory claims concerning the physical causes of phenomena. This increasing emphasis upon hypotheses and their indispensable role in science leads Jacobs to argue Mill no longer takes inductive generalizations grounded on the results of his eliminative methods as the essential component in discovery and justification of causal knowledge.

Mill writes, “the notion that truths external to the mind may be known by intuition or consciousness, independently of observations and experience, [was]…the great intellectual support of false doctrines and bad institutions” (John Stuart Mill 1963, v. 1, 233). As such, Mill’s epistemology of scientific inquiry is firmly grounded in observation and experiment.[9] The only way to discern real physical causes, according to Mill, is to

> meet with some of the antecedents apart from the rest, and observe what follows from them; or some of the consequents, and observe by what they are preceded. We must, in short, follow the Baconian rule of *varying the circumstances*. This is, indeed, only the first rule of physical inquiry, and not, as some have thought, the sole rule; but it is the foundation of all the rest. (John Stuart Mill 1963, v.7, 381)

Varying the conditions allows one to discern which antecedent conditions are invariably and unconditionally connected with consequent phenomena; both natural and artificial contexts make possible the discovery of such instances.

There are two ways in which one can detect variation among the circumstances of an empirical regularity: passive observation of natural variations among observable phenomena or active introduction of circumstantial variations through experiment. The artificial setting of experimental inquiry allows for a great number of precise, targeted, and controlled circumstantial variations. This allows the experimentalist

> to produce the precise *sort* of variation which we are in want of for discovering the law of the phenomena; a service which nature, being constructed on a quite different scheme from that of facilitating our studies, is seldom so friendly as to bestow upon us. (John Stuart Mill 1963, v.7, 382)

---

[9] As a point of contrast, Whewell’s epistemology of scientific inquiry requires a synthesis of both experience and *a priori* conceptions or ideas. For more on Whewell’s “anti-thetical” epistemology see Menachem Fisch (1991), Margaret Morrison (1997), and Laura J. Synder (2006).

Experiment also produces a reliable understanding of the varied effects occasioned by the introduction and manipulation of known causes. And this enables one to trace the relationships between these causes and a wide variety of other phenomena.

In cases where experimentation is impossible or limited, Mill contends that scientific progress requires an effective employment of the deductive method. The test here is to determine whether the empirical predications of known laws or physical causes accord with what one observes in nature. Nonetheless, Mill maintains that one cannot prove through observation alone that one has discovered a genuine cause; rather, one must perform sets of experiments to determine which antecedent condition is invariably and unconditionally connected to the consequent phenomenon in question. Until one substantiates this connection by experimental evidence, an apparent regularity may be an instance of accidental invariable connection or simply the successive stages of another more fundamental cause.

Mill's ascribes the power of experimental inquiry to the fact that it involves eliminative methods which allow for

> the successive exclusion of the various circumstances which are found to accompany a phenomenon in a given instance, in order to ascertain what are those among them which can be absent consistently with the existence of the phenomenon. (John Stuart Mill 1963, v.7, 392)

The primary methods Mill discusses are the Method of Agreement (MA) and the Method of Difference (MD); these are the simplest and most obvious for "singling out from among the circumstances which precede or follow a phenomenon, those with which it is really connected by an invariable law" (John Stuart Mill 1963, v.7, 388). The other methods—the Joint Method of Agreement and Difference (JM), the Method of Residues

(MR), and the Method of Concomitant Variations (MCV)—each involve some specific application of MA or MD (or both).[10]

For the sake of illustration, suppose that one observes the following sets of antecedent conditions (*ABC*, *BC*, and *ADE*) conjoined with the following sets of consequent conditions (*abc*, *bc*, and *ade*). MA involves investigating diverse instances in which a specific phenomenon occurs to discern whether there is an antecedent circumstance in which they all agree. Noting that *A* is present in both cases in which *a* appears, one infers that the presence of *a* is connected invariably by a law of causation with *A*. MD requires one to compare instances in which a phenomenon occurs with instances in which it fails to occur in order to discover if a particular antecedent condition is absent in the latter case. Noting that *a* fails to appear only in that instance in which *A* is absent allows one to conclude that it is the absence of *A* which explains the absence of *a*. The successful employment of MD requires that the antecedent conditions agree in all circumstances except one (i.e., the one under investigation). But

> it is very seldom that nature affords two instances, of which we can be assured that they stand in this precise relation to one another. In the spontaneous operations of nature there is generally such complication and such obscurity, they are mostly either on so overwhelmingly large or on so inaccessibly minute a scale, we are so ignorant of a great part of the fact which really take place, and even those of which we are ignorant are so multitudinous, and therefore so seldom exactly alike in any two cases, that a spontaneous experiment, or the kind required by the Method of Difference, is commonly not to be found. When, on the contrary, we obtain a phenomenon by an artificial experiment, a pair of instances such as the method requires is obtained almost as a matter of course, provided the process does not last a long time…It is, in short…the very nature of an experiment, to introduce into the pre-existing state of circumstances a changes perfectly definite. (John Stuart Mill 1963, v.7, 393)

---

[10] As such, I will not describe their employment in significant detail in this paper.

In short, Mill contends that MD is uniquely powerful among all the methods for the justification of explanatory claims concerning the laws governing phenomena.

Mill's discussion of these methods presupposes that one can separate individual threads of causation—single effects from single causes. But

> The cause indeed may not be simple; it may consist of an assemblage of conditions; but we have supposed that there was only one possible assemblage of conditions, from which the given effect could result. (John Stuart Mill 1963, v.7, 434)

If this assumption fails, as it can in cases where a plurality of causes are operative or where an intermingling of effects occurs, experimental results cannot establish certain knowledge of laws of physical causation. In fact, the recognition of a plurality of causes implies that a consequent phenomenon can be produced by more than one cause. Hence, an antecedent condition invariably connected with consequent phenomena may not be the cause despite its agreement in diverse cases. One can establish causal laws with certainty only if one derives confirming evidence from MD or through deduction from other known laws.

With respect the intermingling of effects arising from the composition of causes, discovering the unconditional invariable antecedent conditions of consequent phenomena can be achieved deductively or experimentally. Through deduction, one infers from one's knowledge of established laws of physical causation pertaining to antecedent conditions the exact effect of the composition of causes in the particular case. The experimental method takes the whole assemblage of the composition of antecedent conditions and, treating them as one cause, attempts to find instances through observation or experiment with which to compare it. But observation only provides a vague notion of the laws of causation here because of the potential for plurality of causes:

> When an effect results from the union of many causes, the share which each has in the determination of the effect cannot in general be great: and the effect is not likely, even in its presence or absence, still less in its variations, to follow, even approximately, any one of the causes. (John Stuart Mill 1963, v.7, 448)

Likewise, the purely experimental method, absent any appeal to deduction, only offers knowledge that a certain set of antecedent conditions is usually followed by a specific phenomenon.

So, Mill's methods enable one to take initial steps in an idealized context of inquiry—that is, a context in which one is dealing with singular threads of causation between phenomena. But they do not allow for genuine progress in actual contexts of investigation without the aid of deduction and the use of hypotheses.[11] As noted earlier, Mill contends that the use of hypotheses ideally should be provisional—what begins as a hypothesis should come to be grounded on independent inductive evidence. Ultimate proof of any hypothesis, however, requires direct confirmation of the reality of the cause by an application of the method of difference. This is possible only if (i) one can produce contrast cases involving the presence and absence of a particular phenomenon and (ii) one can trace this difference back to the absence of the proposed cause in the case where the phenomenon in question fails to obtain. Mill contends that hypotheses,

> by suggesting observations and experiments, [put] us on the road to…independent evidence if it be really attainable; and till it be attained, the hypothesis ought only to count for a more or less plausible conjecture. (John Stuart Mill 1963, v.7, 496)

## 3. Mill's Philosophy of Science Illustrated

---

[11] Steffen Ducheyne (2008) also recognizes the increasing importance Mill attributes to hypothetical speculation in successive editions of the *System of Logic*.

Upon completing the manuscript of his *System of Logic*, it was clear that Mill needed to address a significant lacuna in this work. Alexander Bain wrote,

> The main defect of the [*System of Logic*]…was in the Experimental Examples. I soon saw, and he felt as much as I did, that these were too few and not unfrequently incorrect. It was on this point that I was able to render the greatest service. Circumstances had made me tolerably familiar with the Experimental Physics, Chemistry and Physiology of the day, and I set to work to gather examples from all available sources. (Alexander Bain 1969 [1882], 66)

Mill needed to accumulate historical and contemporary examples of the ways in which the sciences exemplified his philosophical principles. One of the examples Mill borrowed was Bain's discussion of the theory of induced electricity. Understanding this discussion requires some preliminary discussion of the scientific study of electrical phenomena in the early Nineteenth Century.[12]

Prior to 1800, the chief electrical phenomena scientists had studied experimentally were forms of frictional, or static, electricity. This was no accident given that the original electrical phenomenon garnering significant attention in the Modern period was the attractive effects exhibited by amber. Soon electric machines had been constructed that enabled experimentalists to achieve a reliable and efficient production of frictional electricity from various substances. The invention of the Leyden jar enabled scientists to store electrical discharge generated by these machines. And they could discharge the Leyden jar through conducting wires. The invention of the Leyden jar facilitated more reliable experimentation on the nature of frictional electricity; it enabled a careful and systematic exploration of electrical phenomena impossible with purely passive observation of natural forms of static electricity.

---

[12] For helpful discussion of the science of electricity in the Early Modern period see John Heilbron (1979) and Brian Baigrie (2006).

Near the end of the 18th-century, Alessandro Volta developed an apparatus that enabled the production of a dynamic, rather than static, form of electricity. The so-called 'voltaic pile' consisted of a series of alternating metallic discs (usually zinc and copper) separated by a cloth or cardboard soaked in brine. It produced electricity in a dynamic form that could be conveyed through a conducting wire connecting the terminals of the apparatus. The voltaic apparatus generated a number of interesting effects and opened up a new wave of research on various electrical effects and their connections to other kinds of phenomena in separate scientific domains (e.g., chemical decompositions). In fact, the science of electromagnetism emerged as distinct scientific domain following the 1820 discovery that electrical currents generated by a voltaic pile caused the deflection of a magnetic needle situated in close proximity to the conducting wire.[13]

The term 'induced electricity' had typically been used to refer to the power of static electric bodies to produce opposite electrical states in bodies within their immediate vicinity. Thus, according to Mill, the study of the theory of induced electricity had as its primary object the discovery of "the law of what is termed *induced* electricity; 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 begins his discussion of the theory of induced electricity by noting the most common phenomena exhibiting this property—the static electric effects produced by electrical machines. For instance, when scientists place pith balls in close proximity to the conductors of these machines, the pith balls to acquire an opposite electric charge to

---

[13] See Hans Christian Oërsted (1820).

the conductor itself. Mill attributes this effect to the conductors themselves or to the conducting influence of the atmosphere directly surrounding the conductors. Having acquired an opposite charge to the conductor itself, the pith balls are then attracted to the conductor. If scientists removed them from the immediate vicinity of the conductor, they attract any other body with an opposite charge. Mill concludes that

> the accumulation of electricity in an insulated conductor is always accompanied by the excitement of the contrary electricity in the surrounding atmosphere, and in every electrical conductor placed near the conductor. It does not seem possible, in this case, to produce one electricity by itself. (John Stuart Mill 1963, v.7, 411)

Mill then examines all other positive instances in which an electrified body produces an opposite electrical state in its immediate vicinity. Considering additional experiments with electrical machines and their effects on glass cylinders and plates as well as experiments involving the Leyden jar, voltaic instruments, magnets, and electromagnets, Mill quickly canvases “all the known modes in which a body can become charged with electricity” (John Stuart Mill 1963, v.7, 411). And in every case, the charging of a body is unconditionally accompanied by “the excitement of the opposite electric state in some other body or bodies” (John Stuart Mill 1963, v.7, 411-12). Given the invariable connection between these phenomena, it seems that an indispensable condition for the excitement of electricity in any body is the development of the opposite electric state in some neighboring body.

As a decisive confirmation of this proposed law of induced electricity, Mill adduces Faraday’s discovery that voltaic currents, magnets, and electromagnets produce corresponding electrical currents in adjacent bodies.[14] Before summarizing Mill's discussion of this example, it is important to clarify two features of Mill's account. First, Mill is not seeking to give a detailed reconstruction of Faraday's discovery or Faraday's purposes in conducting his experimental research. Rather, Mill utilizes Faraday's work because he believes that it illustrates his understanding of the Method of Difference and, thereby, serves as an example of the significance of this method for the justification of a proposed explanatory law. Second, Faraday defines the term 'induction' so that it refers to any effect produced by electricity- on bodies within the immediate vicinity of a conducting wire. This was an extension of the received understanding, and Mill's use, of the term 'induction' which referred only to the powers of static electricity to produce opposite electrical states in bodies within their immediate vicinity. Faraday's experiments indicated that voltaic currents, magnets, and electromagnets produce a momentary or transient effect on a body in the proximity of current-carrying wires, but they did not produce electrical effects the kind most, including Mill, typically associated with induced electricity.

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 voltaic electricity. He writes,

> 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

---

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

> 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 diverged from the other cases Mill considered in connection with the law of induced electricity in that all the other cases involved an induction of 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. Recall that 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 (understood in the more narrow Millian sense) in a neighboring body is present and those in which it is fails to occur. The current-carrying wire in Faraday's experiments 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. Nonetheless, Faraday's experiments confirmed the basic law that the production of any kind of electricity depends upon the correlative production of an opposite electrical charge.

## 4. Evaluating Mill's Reconstruction

Given the central role that the method of difference plays in Mill's account of the sciences, it is important to evaluate Mill's reconstruction of Faraday's discovery. In fact, Mill's description contains numerous errors. He mischaracterizes some of the foundational assumptions of Faraday's research, fails to acknowledge the theoretical context and research program generating his experimental work, and attributes to Faraday a view of the theoretical implications of the experimental results that Faraday himself did not endorse. In order to provide a framework for understanding these criticisms, a brief account of Faraday's published discussion of this discovery is instructive.

Following the 1820 discovery that voltaic currents could produce magnetic effects, 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.[15] After nearly a decade of failed experimental research throughout various scientific communities in

---

[15] 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).

Europe, Faraday demonstrated conclusively that magnets could produce electrical currents. The central, and unexpected, feature of this discovery was the fact that these induced electrical currents were momentary rather than continuous currents.[16]

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.[17] 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 and magnetic phenomena. Ampère maintained 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 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 areas: (i) the possible causes for the transient nature of induced

---

[16] 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).

[17] For more discussion of Ampère's work see James Hoffman (1987 and 1996).

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.[18] With respect to the latter, Faraday 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. This provided a unifying explanation of both induced electrical effects and related electromagnetic phenomena many other scientists considered mysterious.[19]

Mill’s reconstruction of Faraday’s research mischaracterizes Faraday’s explicit account of this discovery in several ways. First, Faraday did not assume the identity of common electricity and voltaic electricity as Mill claims. Faraday’s use of term ‘induced electricity’ to refer to any electrical effect on neighboring bodies indicates that he sees these effects as analogous in some way, but he does not presuppose that these forms of electricity are identical. Faraday admits that some of his initial experiments designed to substantiate a stronger connection between these kinds of electricity produced no clear indication of a connection; in fact, the kinds of experiments he performed could not produce evidence substantiating their identity.[20]

Second, to say that this assumption was part of the rationale for Faraday’s research inaccurately depicts the explicit research agenda Faraday describes at the outset of his published results. The goal was to discover whether magnets could produce

---

[18] Michael Faraday 1956, v.1, 16-24.

[19] For more on this aspect of Faraday’s discovery see Friedrich Steinle (1994).

[20] Michael Faraday 1956, v.1, 6-7.

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.

Third, contrary to Mill's interpretation, Faraday was agnostic about the reality of opposing electrical currents existing within the conducting wire. Although this was an essential commitment of Ampère's electrodynamic theory, Faraday explicitly refused to assent to this hypothesis.[21] The closest Faraday came to endorsing this hypothesis was his speculation concerning the electro-tonic state, but just two months after publishing his results concerning induced electrical currents, Faraday retracted this hypothesis due to the fact that the established law governing induced electrical effects provided a more satisfactory explanation of its transience.[22]

Mill uses Faraday's discovery effectively as an illustration of the method of difference. But this brief study of Faraday's research suggests that Mill's appropriation of this discovery required him to divorce it from the historically-situated reality of Faraday's research. As such, Mill does not show that the history and practice of science exemplifies the general principles of his philosophy of science. Given Mill's indirect acquaintance with scientific practices and his remote understanding of the history of

---

[21] 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 see F.A.J.L James (1991, 252, 287-288) and L. Pearce Williams (1985).

[22] See Michael Faraday 1956, v.1, 16.

science, there is good reason to worry that many of Mill's illustrative examples involve systematic interpretive errors of this kind. And this suggests that Mill's philosophy of science may not be adequately informed by the sciences.

## 5. What Mill Could and Should Have Said

While Mill's discussion of induced electricity may be deeply flawed, this does not imply that Mill's general philosophy of science cannot derive any support from the sciences. In fact, Mill's claim that his logic provides a canon for assessing the evidential merits of proposed scientific discoveries rather than a logic of discovery may provide grounds for a rebuttal of critique delineated above.[23] Furthermore, Mill maintains that his account of experimental methods is situated within an idealized context of inquiry abstracted from the necessarily complicated fabric of causal relations in nature; the methods of agreement and difference assume that one can isolate specific, individual 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 historically-situated contexts of actual scientific inquiry, where one must employ deduction and hypotheses in addition to inductive methods, the primary function of experimental methods is to provide a canon for assessing the conclusiveness of one's evidence.

Given this qualified understanding of Mill's account of scientific methodology, Mill could have employed elements of Faraday's research to illustrate his understanding of scientific methodology. After all, it is clear that the central goal of Faraday's research

---

[23] John Stuart Mill 1963, v.7, 430.

subsequent to 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. Additionally, 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. Finally, 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 experimental practice. 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.

Beyond this, Mill could have borrowed several examples from Faraday's published discussion to illustrate his understanding of his experimental methods. Consider for instance one experiment representative of the Method of Agreement. Recall that Faraday had not suspected that induced electrical currents would be transient or momentary. One worry Faraday had concerned whether the induced electrical effect was essentially transient or whether its transience was dependent upon the fact that establishing or disconnecting a circuit to the voltaic apparatus is itself a momentary event. So, Faraday conducted a new set of experiments that could rule out the possibility that the transience of the effect was an artifact of the experimental system. In these experiments, Faraday constructed two copper wires so that every part of each wire would touch its exact corresponding point on the other if they were to come into contact. He connected one of these wires to a galvanometer needle and the second wire to a voltaic apparatus. Then, with the second wire, he initiated motion either towards or away from the first wire. When the wire was in motion, Faraday found that there was a corresponding deflection in the galvanometer needle connected to the first wire. Furthermore, a deflection occurred regardless of whether the motion was towards or away from the neighboring wire. When there was no motion between the wires, however, there was no detectable effect on the galvanometer needle.

In this experiment, Faraday did not establish or disconnect the circuit to the voltaic apparatus like his previous experiments. As such, there were materially different circumstances in the distinct experimental trials but the results were identical. Thus, the transient nature of the effect was not dependent upon the momentary act of completing or disconnecting the circuit to the voltaic apparatus. Faraday could surmise that some common antecedent cause was operative in both sets of trials given the similar effect.

Consider another example as an illustration of the 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 this final experiment in accordance with the Method of Difference as follows. 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

Mill's discussion of the theory of induced electricity and, in particular, his reconstruction of Faraday's discovery of electrical induction was clearly flawed. And this fact confirmed the general impression that Mill developed his philosophy of science without due consideration of the history and practice of the sciences. While this critique is telling, it does not entail that Mill's philosophy of science lacks sufficient grounding in the sciences. So, in this concluding section, I address this issue more systematically and discuss the implications of Mill's approach for understanding the complex relationship between the sciences and the philosophy of science.

To the extent that the sciences employ experimental methods for the purposes of (i) generating a reliable understanding of specific effects and the causal relations among phenomena, (ii) grounding predictions about future occurrences of these phenomena, and (iii) producing evidence that can be employed in scientific arguments about explanatory laws, it is clear that aspects of scientific inquiry fit Mill's account of scientific methodology. Nonetheless, there are elements of scientific inquiry that Mill's philosophy of science does not adequately represent. The discussion of Faraday's experimental research demonstrates Mill's failure to acknowledge the importance of theoretical or hypothetical notions in the generation of scientific research and in guiding the experimental process. It is not clear that Mill's philosophy of science can accommodate this aspect of scientific inquiry though it seems just as central to scientific inquiry as the systematic performance of experiments. So, if one looks at scientific inquiry as a whole, then it seems that the sciences do not conform to Mill's more narrow understanding of the nature of science.

Mill's can respond to this charge in two ways. First, he can distinguish between those scientific processes which lead to the initial discovery of important scientific phenomena or laws and those scientific processes which are essential to justifying these discoveries as legitimate and well-founded. It is clear that theoretical and hypothetical ideas play a very important role in the discovery process, it is not clear that they are essential to understanding the methods that function in a justifying role. So, while it is clear that his logic of induction does not provide a model or analysis of the modes discovery, Mill can argue that these processes aren't the aspects of scientific inquiry that make science distinctive as a knowledge-generating practice. In effect, Mill can argue that it is those methods and processes that function in a justificatory capacity that an adequate philosophy of science must describe. Second, even if his logic does not represent the discovery process adequately, Mill can maintain that he both understands and appreciates the significance of theoretical and hypothetical notions in scientific inquiry. Their use, he would argue, is heuristic and valuable insofar as it leads to the discovery of more fundamental laws unifying distinct domains of the sciences.[24]

Critics of Mill's philosophy of science are likely to respond to these claims in two ways. To the claim that theoretical and hypothetical ideas are merely heuristics, they will argue that this fails to account for the actual use of theory and hypotheses in the sciences. While it is true that there are theoretical or hypothetical ideas that are purely scaffolds for additional research, it doesn't follow that *all* theoretical or hypothetical speculation is heuristic. More importantly, Mill's response fails to acknowledge the fact that scientists often attempt to establish theoretical or hypothetical notion on the basis of experimental evidence.

To Mill's distinction between the scientific processes leading to discovery and those processes essential to the justification of a discovery, they will argue that this narrow view of the sciences reflects a flawed understanding of the aims of scientific inquiry. Scientists attempt to discover and substantiate the theoretical or hypothetical notions generating and guiding their research because they believe that this is part of the ultimate aim of the sciences. Scientists aim to discover the productive causes of phenomena and, as such, they are not content with the more limited understanding of the laws governing phenomena. An ultimate explanation requires uncovering the agents that produce these effects according to the established nomological relationships among phenomena.

Mill is likely to concede as a descriptive fact that scientists often engage in this kind of speculation. Furthermore, he will acknowledge that many scientists and

---

[24] See, in particular, Mill's discussion of hypothetical inquiry around John Stuart Mill 1963, v. 7, 495.

philosophers think that the aim of scientific inquiry is to discover these kinds of ultimate efficient causes posited by higher-level theoretical speculation. Nonetheless, from his normative epistemological standpoint, Mill argues that it is impossible to know the underlying efficient causes of phenomena. Any attempt by a scientist to produce evidence substantiating a speculative claim about these kinds of causes will fail because it is impossible to adduce evidence, independent of the putative empirical effects of these proposed causes, of their reality. The only kinds of hypotheses or theories susceptible of proof on Mill's account are those which can be subjected to test and proof by the Method of Difference. Hence, if the discovery of underlying efficient causes is the ultimate aim of the sciences, then it cannot be satisfied using only the tools and methods available in scientific inquiry. For Mill, this is sufficient reason to think that the ultimate of the sciences is the production of warranted knowledge of the general laws governing empirical phenomena. This is a goal that can be satisfied through scientific inquiry.

Given this dialectic, what is the proper response to the question of whether the sciences provide sufficient grounding for Mill's philosophy of science? One cannot answer this question independently of some conception of the nature of sciences and, in particular, the aims and scope of scientific inquiry. This conception will be grounded in some underlying epistemic commitments. If Mill is right to think that all human knowledge originates in, and finds its justification in, experience alone, then it is not surprising that Mill takes science to have more limited aims than traditionally supposed. Furthermore, it is no wonder that Mill takes scientific methods to have a more limited scope in the justification of explanatory claims. Insofar as scientific inquiry contains elements that go beyond Mill's understanding of the limited aims and scope of scientific inquiry, then it is obvious that Mill's philosophy of science will not adequately represent these aspects. According to Mill, however, a legitimate philosophy of science does not need to explain those elements of scientific inquiry that are not reflective of the proper understanding of science as such.

This discussion suggests a distinctive account of the relationship between science and the philosophy of science. Mill's epistemological commitments ground a normative understanding of the essential or legitimate elements of scientific inquiry. Rather than reflecting on the history and practice of the sciences and attempting to construct a philosophy of science that accommodates the wide range of practices and processes of science throughout its history, Mill's extra-scientific epistemological commitments guide and determine his conception of those elements of the sciences that are essential to understanding and explaining its success. The history and practice of the science cannot function as an independent tribunal for assessing the adequacy of a philosophy of science because the philosophy of science itself must provide an analysis of those elements of the sciences that are essential to its success and its distinctive qualities as a knowledge-generating practice.

How then should one understand the criticism, from Whewell and others, that Mill's philosophy of science lacks sufficient grounding from the history and practice of the sciences? If Mill's analysis is correct, then one might understand this as an objection arising from a divergent set of extra-scientific epistemological commitments. That is, philosophers of science who assume a radically different epistemological standpoint are likely to find Mill's strict empiricist reading of the sciences as misguided. But these critics still approach the history and practice of the science presupposing their own epistemological framework and employing this framework in their articulation of what it is about the sciences that fuels their success. Thus, the claim that the sciences don’t support Mill’s philosophy of science is not grounded in a detached assessment of the history and practice of the sciences as an independent arbiter in this disagreement. And this moves the debate from an assessment of the historical and scientific adequacy of one’s philosophy of science to a philosophical dispute about epistemology as such.

If Mill’s response is inadequate, there is a distinct lesson one can draw from this discussion. In particular, the criticism of Mill’s philosophy of science as an inadequate representation of the history and philosophy of science may reflect a distinct view about the nature of the relationship between science itself and the philosophy of science. On this view, the use of detached examples from various scientific domains in service of a preconceived philosophical framework is a radically misleading and unfounded approach to the philosophy of science. Rather than approaching science from this detached perspective, a philosopher of science constructs a true and well-grounded philosophy of science only through a careful, systematic, and thorough evaluation of the historical record and the contemporary practice of the sciences. In this respect, the history and practice of the sciences act as a constraint and normative ground on the philosophy of science. If this account of the relationship between science and the philosophy of science provides an more accurate and fruitful account of the dynamic between science and the philosophy of science, much more work must be done to understand the nature of the scientific practice and the history of science independent of any pre-conceived philosophical reconstructions.

## REFERENCES

Baigrie, Brian. 2006. *Electricity and Magnetism: A Historical Perspective*. Westport, Connecticut: Greenwood Press.

Berkson, William. 1974. *Fields of Force: The Development of a World View From Faraday to Einstein*. New York: Wiley Publishers.

Butts, Robert E.. 1968. *William Whewell's Theory of Scientific Method*. Pittsburgh: University of Pittsburgh Press.

Dibner, Bern. 1949. *Faraday Discloses Electro-Magnetic Induction*. New York: Burndy Library.

Ducheyne, Steffen. 2008. "J.S. Mill's Canons of Induction: From True Causes to Provisional Ones." *History and Philosophy of Logic* 29(4): 361-376.

Faraday, Michael. 1956. *Experimental Researches in Electricity*. New York: Dover Publications, Inc..

Fisch, Menachem. 1991. "Antithetical Knowledge." Pp. 289-310 in *William Whewell: A Composite Portrait*. Eds. Menachem Fisch and Simon Schaffer. Oxford: Clarendon Press.

Heilbron, J.L.. 1979. *Electricity in the 17th & 18th Century: A Study of Early Modern Physics*. Berkeley: University of California Press,.

Hoffman, James R.. 1987. "Ampère, Electrodynamics, and Experimental Evidence." *Osiris* 2: 45-76.

————. 1996. *André-Marie Ampère*. Cambridge: Cambridge University Press.

Jacobs, Struan. 1991. "John Stuart Mill on Induction and Hypotheses." *Journal of the History of Philosophy* 29: 69-83.

James, Frank A.J.L. (Ed.). 1991. *The Correspondence of Michael Faraday: Volume 1 (1811-1831)*. London: Institution of Electrical Engineers.

Mill, John Stuart. 1963. *Collected Works of John Stuart Mill* Volumes 1-33. Toronto: University of Toronto Press.

Morrison, Margaret. 1997. "Whewell on the Ultimate Problem of Philosophy." *Studies in the History and Philosophy of Science* 28: 417-437.

Oërsted, Hans Christian. 1820. "Experiments on the Effect of a Current of Electricity on the Magnetic Needle." *Annals of Philosophy* 16: 273-277.

Ross, Sydney. 1965. “The Search for Electromagnetic Induction: 1820-1831.” *Notes and Records of the Royal Society of London* 20: 184-219.

Snyder, Laura J.. 2002. “Whewell and the Scientists: Science and Philosophy of Science in 19th Century Great Britain.” Pp. 81-94 in *History and Philosophy of Science: New Trends and Perspectives*. Eds. M. Heidelberger and F. Stadler. Dordrecht: Kluwer Press.

————. 2006. *Reforming Philosophy: A Victorian Debate on Science and Society*. Chicago: University of Chicago Press.

————. 2008. “‘The Whole Box of Tools’: William Whewell and the Logic of Induction.” Pp. 163-228 in *Handbook of the History of Logic. Vol. 4: British Logic in he Nineteenth Century*. Eds. Dov M. Gabbay and John Woods. Amsterdam: Elsevier B.V..

Steinle, Friedrich. 1994. “Experiment, Speculation and Law: Faraday's Analysis of Arago's Wheel.” *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* 1: 293-303.

Todhunter, Isaac. 1876. *William Whewell, D.D. An Account of His Writings, with Selections from His Literary and Scientific Correspondence*. 2 vols. Macmillan, London.

Tweeney, Ryan. 1985. “Faraday's discovery of induction: A Cognitive Approach.” Pp. 189-210 in *Faraday rediscovered: Essays on the life and work of Michael Faraday 1791-1867*. Eds. David Gooding and F.A.J.L. James. New York: Stockton Press/London: Macmillan.

Whewell, William. 1857. *The History of Inductive Sciences, from the Earliest to the Present Time.* 3rd ed., with additions. 3 vols. London: John W. Parker.

Williams, L. Pearce. 1965. *Michael Faraday: A Biography*. New York: Basic Books.

————. 1985. “Faraday and Ampère: A Critical Dialogue.” Pp. 83-104 in *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday, 1791-1867*. Eds. David Gooding and Frank A.J.L James (Eds.). New York: Stockton Press.