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Hypothesis and Consilience in the Nineteenth Century

1. INTRODUCTION

Perhaps the most important development in nineteenth century philosophical discussions on scientific methodology was the dynamic re-emergence of *the method of hypothesis*. Simply put, the method of hypothesis starts with the formulation of a hypothesis to explain observed facts. Then, the observable consequences of the hypothesis are deduced. If the deduced consequences are verified by observation and experiment, the hypothesis is validated. If the observed consequences of the hypothesis disagree with observation and experiment, the hypothesis is modified or discarded. This development came as a reaction to traditional scientific methodology which regarded scientific inferences mostly as inductive generalizations from empirical facts. More specifically, for traditional methodology scientific investigation was a purely empirical process that started with observation and experiment, and then cautiously, but surely, led to the formulation of general statements that provided the true description and explanation of the observed facts. In this context, the formulation of hypotheses about the object of investigation was considered unnecessary and potentially harmful to scientific inquiry. During the nineteenth century, however, philosophers—especially the ones sensitive to the complexities of scientific practice as revealed also by the study of the history of science—started to realize that traditional inductive methodology could not accommodate new scientific developments, particularly those related to the investigation of unobservable processes, entities, and phenomena. The scientists investigating such unobservables, rather than generalizing from particular facts, constructed hypotheses and deduced predictions from them which then they compared with the observable facts (Laudan 1981).

Amidst all the criteria for evaluating hypotheses, the ability of a hypothesis to successfully predict, explain, and/or be supported by a variety of classes of empirical facts—especially facts that played no role in the initial formulation of the hypothesis—was considered to be the highest criterion of truth. Support from different classes of facts was thought to give rise to a *no-coincidence argument* for the truth of the hypothesis; namely that it would be an improbable coincidence for a hypothesis (usually about unobservables) to be able to accommodate a variety of (observable) facts and yet for it to be false. This criterion is found more explicitly in William Whewell's (1840b; 1847; 1858) notion of the *consilience of inductions*, but it can also be encountered in the writings of many other nineteenth century philosophers such as John Herschel (1830), William Stanley Jevons (1874), and Charles Sanders Peirce (1878, c. 1905). Throughout this chapter we use the term 'consilience' to refer to the ability of the theoretical hypothesis to account for different classes of facts (independently of whether the authors discussing this ability use this term to refer to it).

This chapter examines the method of verifying hypotheses in the thought of Whewell, J. S. Mill, Herschel, Jevons, and Peirce. It focuses especially on expounding and evaluating the reasons these philosophers gave for the epistemic force attributed to the criterion of consilience, i.e., their response to the question: why the ability of a hypothesis to explain different classes of facts should be considered (or should not, in Mill's case) as an argument for its truth? The chapter is structured as follows. Section 2 examines the structure, epistemic role, and epistemic import of Whewell's consilience of inductions. Section 3 discusses Mill's and Herschel's views on the method of the hypothesis and the criterion of consilience. Section 4 examines the same topics in the thought of Jevons and Peirce. Section 5 concludes by summarizing the main points.

2. WHEWELL AND THE CONSILIENCE OF INDUCTIONS

According to Whewell, the process of induction by which all our knowledge of the external world is produced consists in the synthesis of an ideal (*a priori*) component with an empirical component. The ideal component is represented by the *fundamental ideas*. The fundamental ideas are certain comprehensive forms of thought, such as *space*, *time*, *cause*, and *resemblance* which provide the structure or form for the multitude of sensations we experience (Whewell 1840a, 26; Snyder 2017). The fundamental ideas are supplied by the mind itself and are not received from our experience and observation of the world. The empirical component of knowledge, on the other hand, is represented by the empirical facts which are provided by observation and experiment. For the fundamental ideas to be applied to the empirical facts, they need to be appropriately modified. These special modifications of the fundamental ideas Whewell calls them *conceptions*. For example, *a circle, a square number, an accelerating force, a genus*, are offered by Whewell as respective examples of modifications of the fundamental ideas of space, time, cause, and resemblance (Whewell 1840b: 171).

The inductive process, therefore, consists in the application of the ideas and conceptions furnished by the mind to the empirical facts provided by observation and experiment. The most essential part of this process consists in what Whewell calls the *colligation of facts*. This is the binding together of a class of facts by superinducing upon them an exact and appropriate conception. More specifically, during the colligation process, a class of clear and true facts is explained by being bound together by the appropriate clear and distinct conception (i.e., inductive hypothesis). Thus, according to Whewell, in every inductive inference there is some general conception superinduced upon the facts. The conception is not found in the facts themselves, but it is derived from the fundamental ideas of the mind (Whewell 1840a, 43). During the inductive process, 'The particular facts are not merely brought together, but there is a new element added to the combination by the very act of thought by which they are combined. There is a conception of the mind introduced in the general proposition, which did not exist in any of the observed facts' (Whewell 1840b; 213, 242). Whewell's induction thus stands in contrast with the British empiricist tradition from Bacon onwards, which regarded induction as the process by which a general proposition is derived from the observation, examination, and juxtaposition of empirical facts (Whewell 1840b: 215-225; 1860: 150-151).

Using a metaphor, Whewell referred to the conception used to make sense of the facts, as the language key which is used to interpret the letters of a cryptogram. Whereas the letters and their order are empirically perceived, the language key is provided by the mind (Whewell 1840a: 44-45). Often, new discoveries are made, argued Whewell, not when new facts are discovered, but when the appropriate conception is applied to already known facts. Whewell used as an exemplar of induction Johannes Kepler's colligation of the points of the Martian orbit obtained by observation, by using the conception of an elliptical curve. In this case, the points of the orbit were observed by Tycho Brahe, but only when Kepler applied to them the conception of an ellipse, the observed points were conceived as constituting the true path of Mars' orbit (Whewell 1840b, 253; Snyder 2017).

Having clarified the induction process by which knowledge is generated, Whewell was faced with the question: how do we know that we have used the appropriate conception (or inductive hypothesis) to colligate (i.e., to bring together and thus explain) the facts of observation? For Whewell, there seems to be four main criteria or tests for verifying the colligating character of an inductive hypothesis: *consistency with all the observed facts*, *prediction of unobserved facts*, *consilience of inductions*, and *simplification of the theory*. These criteria are, in the order mentioned, the one more severe than the other. Among them, consilience is a sufficient criterion of truth.

The consistency criterion requires that the inductive hypothesis is consistent with *all* the relevant observed facts (i.e., with all the facts that it was invoked to explain). This is the most minimal criterion, and it is already implicit when the colligation process takes place (Whewell 1840b: 227-228). The prediction criterion is an extension of the consistency criterion to all the kinds of facts that the hypothesis was invoked to explain, even if they have not yet been observed (Whewell 1840b: 228). The successful prediction of unknown facts is an argument for the truth of the inductive hypothesis: the agreement of the prediction with what is afterwards found to be the case is not strange if the inductive hypothesis is true, but it would be surprising if the hypothesis is false. Whewell uses again the cryptogram metaphor to explain the rationale underlying prediction, and its difference from consistency. Using the inductive hypothesis to unite together and explain known facts is similar to using the language key to interpret the (visible) symbols of a cryptogram, whereas using the inductive hypothesis to successfully predict unknown facts of the same kind is similar to using the already interpreted part of the cryptogram to predict what is written in the unseen part of the cryptogram, and then see the prediction verified when the unknown part of the cryptogram is revealed. The success is a sign that we have used the correct language key to interpret the cryptogram (Whewell 1840b: 229-230).

Consistency and prediction are indications that the inductive hypothesis is valuable and, at least to a great extent, true. They are not, however, sufficient criteria of truth (Whewell 1840b: 228-229). Consilience—i.e., the ability of a conception or inductive hypothesis to successfully colligate, without receiving any further adjustments or additions, facts of a different kind from those which were contemplated in its initial formulation—on the other hand, is such a criterion. Consilience gives rise to what we can call a *no-coincidence* argument for the truth of the inductive hypothesis: namely it would be a highly improbable coincidence if an inductive hypothesis could account so well for different classes of facts and yet to be false. 'When such a convergence of two trains of induction points to the same spot, we can no longer suspect that we are wrong' (Whewell 1840b: 447).

According to Whewell, the instances where a hypothesis has satisfied the consilience criterion constitute some of the greatest scientific discoveries and belong to some of the best-established theories that the history of science presents. As an exemplar of consilience, Whewell cites Newton's discovery of the universal law of gravitation, which not only was able to explain Kepler's third law of the proportionality of the cubes of the distances to the squares of the periodic times of the planets but also Kepler's first and second laws of the elliptical motion of the planets, although no connection between these laws had been conceived before: 'Here was a most striking and surprising coincidence, which gave to the theory a stamp of truth beyond the power of ingenuity to counterfeit' (Whewell 1840b: 231).

One can distinguish two explicit arguments that Whewell gives to defend (or illustrate) consilience as a criterion of truth. The first appeals to the psychological force of the no-coincidence argument from consilience to compel our assent on the truth of the hypothesis.

> …the evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false suppositions could, after being adjusted to one class of phenomena, so exactly represent a different class, when the agreement was unforeseen and uncontemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from *that* being the point where truth resides (Whewell 1840b: 230).

Using the cryptogram metaphor to convey the psychological force of the rationale underlying consilience, Whewell compares the latter with the case of two different cryptographers who, while deciphering two different cryptograms, arrive at the same language key for interpreting the symbols, independently of one another (Whewell 1860: 274-75).[i]

The appeal to the psychological force of the no-coincidence argument, however, seems insufficient to establish the truth of the consilient hypothesis. Why is it impossible for a false hypothesis to colligate different classes of facts? Is it because we cannot possibly conceive such a thing happening? This is not convincing; not only from a twenty-first century point of view, but also from a nineteenth century one. John Stuart Mill, for example, argued against Whewell that there is 'ample experience to show, that our capacity or incapacity of conceiving a thing has very little to do with the possibility of the thing itself; but is in truth very much an affair of accident, and depends on the past history and habits of our own minds' (Mill [1843] 1974: 238).

Although Whewell undoubtedly appeals to the psychological force of consilience, his position was that a false hypothesis cannot, as a matter of fact, colligate different classes of facts. Only truth can give rise to consilience, falsehood cannot (Whewell 1840b: 236). Perhaps this is where Whewell's second explicit argument in favor of consilience as a criterion of truth enters. This argument appeals to the history of science. We can be certain that consilient inductions are true because history of science tells us that this is the case, and this in two specific ways. First, the most successful theories in the history of science are full of examples of consilience (Whewell 1840b: 232). Second, and most importantly, no false hypothesis was ever found to have the consilience property (Whewell 1840b: 232-33).

The appeal to the history of science supports the claim that Whewell's argument on the epistemic import of consilience was not a psychological, but a logical one: it is conceivable that there might be cases of false consilient hypotheses, but history of science shows that this is not the case. However, and as other scholars have pointed out (Laudan 1971; Fisch 1985), even the appeal to history of science is not sufficient to defend the logical claim that false hypotheses cannot achieve consilience. And this, for at least two reasons. No matter how much evidence is amassed from the history of science, it is not sufficient to establish the validity of the universal claim that Whewell wants to make. Second, even if we accept that in Whewell's time there were not any false consilient hypotheses, today we know that the paradigmatic cases of consilient theories that Whewell appealed to—namely, Newton's theory of universal gravitation and the undulatory theory of light—are not, strictly speaking, true.

It seems, however, that the appeal to the history of science was not meant to prove the claim that consilience is a criterion of truth as much as it was an attempt to illustrate it. Although Whewell appeals to the past of science to show that no false theory can achieve consilience, he does not leave open the possibility that the future of science may prove him wrong. Although Whewell presents simplification (i.e., consilience which occurs over time) as the ultimate criterion of truth, he does not consider the possibility that a genuinely consilient hypothesis can be found to be faulty or in conflict with other facts and/or other consilient hypotheses (Whewell 1840b: 233-34).

One cannot help but think that there is a deeper reason for Whewell's conviction that consilience is an undeniable criterion of truth. The best candidate for this is the *a priori* and divine origin of the ideas and conceptions which are used to colligate the empirical facts (Whewell 1840b: 581-586; Whewell 1860: 354-375). According to Whewell's theological views, a Supreme Divine Mind (i.e., God) created the universe in accordance with certain 'Divine Ideas'. The reason we can have knowledge of the physical world is because the fundamental ideas and conceptions that we use to organize and explain the empirical facts were implanted in us (in their incipient form) by this Supreme Intelligence and they resemble the Divine Ideas that He used to create the world (Whewell 1860: 371). In Whewell's words, 'our Ideas correspond to the Facts of the world, and the Facts to our Ideas, because our Ideas are given us by the same power which made the world, and given so that these can and must agree with the world' (Whewell 1860: 358-359). For example, the Supreme Mind has constituted and constitutes the universe according to the idea of space, and has implanted in us the idea of space, so it corresponds to this reality.[ii]

Note that the problem of justifying the sufficiency of Consilience as a criterion of truth is a problem of underdetermination of hypothesis by evidence. The underdetermination problem refers to the possibility of there being many other (incompatible between them) hypotheses that can account the same well for the same evidence. To establish that a consilient hypothesis is true Whewell does not simply need to show that it can account for different classes of facts, but also that there is no other hypothesis that can do the same. The *a priori* and divine origin of the inductive hypotheses can solve this problem. The fact that the germs of the conceptions were implanted in us by God to correspond with the laws of the physical world implies that: (a) there is a limited number of conceptions to be unfolded from the fundamental ideas, and (b) God intends for us to have knowledge of the physical world. If one can use a conception to colligate not one, but two different classes of clear and distinct facts, one can be certain that one is not making a mistake. Consilience provides a too strong no-coincidence argument for the truth of the hypothesis. I.e., it would be highly improbable for the Supreme Divine Mind to have created such a complicated world that a faulty conception can colligate so well disparate classes of facts. This would go against His attempt to construe a physical world which is amenable to the conceptions with which He has endowed the human mind.

Whewell defense of consilience as a criterion of truth appears to be part of a long philosophical tradition which appealed to metaphysical principles such as the simplicity of nature and/or the ingenuity of a Divine plan to defend the truth of explanations that could account for diverse phenomena. Many other thinkers such as Copernicus, Kepler, Descartes, Leibniz, and Newton, in one way or another, had appealed to such metaphysical principles.[iii] The problem with this defense of the epistemic force of consilience, however, is that once we move away from the innate origin of scientific hypotheses and the divine guarantee of scientific knowledge, we are once again faced with the problem of underdetermination of hypotheses by evidence, aggravated even further by the history of science which reveals the existence, in the past, of false consilient hypotheses.

3. JOHN STUART MILL AND JOHN HERSCHEL ON *INDUCTION* AND *HYPOTHESIS*

The confirmatory virtue of consilience was an important part of the Whewell-Mill debate on the nature of induction (Whewell 1860: 238-291; Snyder 2006). Mill tried to make most of the perceived weaknesses in Whewell's effort to establish the truth guaranteeing nature of consilience. He used Whewell's favorite inscription metaphor to argue that the ability of a hypothesis to explain unknown facts is no more a reliable test for its truth than its ability to explain known facts. It is true, argued Mill, that the ability to interpret the characters of an inscription so that the latter gives a rational meaning in a known language provides strong evidence for the correctness of the interpretation. But the strength of the evidence is not much increased by the ability to guess the remaining letters without seeing them: "for we should naturally expect . . . that even an erroneous interpretation which accorded with all the visible parts of the inscription would accord also with the small remainder; as would be the case, for example, if the inscription had been designedly so contrived as to admit of a double sense" (Mill [1843] 1974: 501-502).

The metaphor Mill refers to here is the metaphor that Whewell used to exemplify prediction. But for Mill there is no difference between prediction and consilience. The ability of a hypothesis to explain or predict facts different from the ones contemplated in its formulation is no different than its ability to explain known facts. Whether the evidence is known or unknown, expected, or unexpected, makes no difference with respect to the truth of the hypothesis. It does not eliminate the underdetermination problem, i.e., the possibility that a different hypothesis can account the same well for the same evidence (Mill [1843] 1974: 502).

For Mill, induction is the process of scientific inquiry that concerns the discovery and establishment of general propositions starting from particular facts. More specifically, induction is defined as the operation of the mind that allows the inference from what we know to be true in a particular case or cases to what will be true in all cases which resemble the former in certain assignable respects (Mill [1843] 1974: 288). Such an inference from known to unknown cases, argued Mill, was not allowed in Whewell's account of induction. Whewell was wrong to think that during the colligation process a new element is added to the facts by the mind which did not already exist in the facts themselves. Mill accepted that when one subsumes a class of facts under a general expression a conception of the mind is needed, but he denied that this conception existed before the facts and/or that it was constructed *a priori* by the mind. If a class of facts is correctly subsumed under a conception during the colligation process, it is because there is in the facts themselves something of which the conception is itself a copy. This something is the general characteristics in which the observed facts resemble one another or other known facts. The conception is, therefore, obtained by abstraction on the members of the class of facts which is then called to connect. Colligation is simply a process of abstraction. In some cases, however, it is difficult to abstract the conception from the facts which one is attempting to colligate because they are out of the reach of what can be readily observed. In these cases, one selects the appropriate conception from among those which have been previously collected by abstraction from other facts. This, according to Mill, is what happened in Kepler's case. If one could observe the paths described by the planets, one would be able to easily identify that they were ellipses. But since the planetary orbits were out of the reach of what could be readily observed, their general description could not be collected by abstraction from the observations themselves. The mind had to supply it hypothetically from among the conceptions it had previously obtained by observing other facts. The way the procedure works is like the method of hypothesis. The mind makes a supposition respecting the general nature of the facts, and then asks: if this is the correct general description of the facts, what would the observable consequences be? It then compares these consequences with the phenomena observed. If the two agree, the hypothesis can serve as a description of the phenomenon. If not, the hypothesis is necessarily abandoned, and another one tried (Mill [1843] 1974; 296-297).

According to Mill, what allows the transition from known to unknown cases is the knowledge of causal relations. The latter is the main goal of inductive inquiry. Every phenomenon has a cause, asserted Mill, and this cause must be found in some phenomenon or concourse of phenomena that immediately precede it. To assign effects to causes and *vice versa*, one needs to follow the Baconian rule of *varying the circumstances*. One needs to have some of the antecedent phenomena or circumstances apart from the rest and observe what consequents follow them, or some of the consequents apart from the rest and investigate from what antecedents they are invariably preceded. Mill used the term *direct induction* to refer to the methods of observation and experiment that are used to vary the circumstances and to determine what phenomena are related to one another as cause and effect (Mill [1843] 1974: 388-406).

Reacting to Mill's account of (direct) induction, Whewell emphasized the method's inability to accommodate the history of science and scientific practice. Even if Mill's direct induction could be implemented successfully, it was doubtful whether it was historically real or that any light could be thrown on the great scientific achievements of the past if they were interpreted as having resulted from its application (Whewell 1860: 262-269; Snyder 2006). Mill, however, recognized the limited relevance of direct induction for actual scientific practice and discovery (Mill [1843] 1974; 429-453). According to Mill, it is not the inductive but the *physical* or *concrete deductive method* which constitutes the main source of knowledge of physical phenomena. It is by using the deductive method that scientists have historically arrived in formulating the most successful theories in which a vast number of different and complex phenomena are subsumed under a few and simple laws. The deductive method is employed when the complexity of phenomena renders direct induction (i.e., the methods of observation and experiment that are used to establish causal relations) inapplicable or insufficient to achieve its goal. Direct induction usually constitutes the first part of the deductive method. In other words, the deductive method does not rely for the discovery of the causal laws of phenomena exclusively on their direct experimental study. It tries to determine the laws of an effect from the consideration of the laws of the different causes or tendencies of which the effect is the joint result. The deductive method consists of three operations: *direct induction*, *ratiocination*, and *verification*.

Since the problem of the deductive method, according to Mill, is to find the laws of the production of an effect from the laws of the different causes of which it is a joint result, its first step consists in ascertaining the laws of each one of these different causes separately. This is done with the application of direct induction, i.e., by varying the circumstances with the use of observation and experiment. The second step, ratiocination, uses the laws of the separate causes established by direct induction to calculate what effect any combination of these causes produces or what causes can produce a given effect. However, because the laws of each separate cause rarely can be determined with certainty, and because the process of calculating the combined effects of the different causes is rarely done with accuracy, the deductive method needs tests to ascertain whether the conclusion of the two first steps is the correct one. This role is played by a third step, verification. Verification compares the conclusion derived by the first two steps with facts obtained by observation and experiment. If the comparison confirms the conclusion, then one can safely trust it to hold even for instances for which there is not yet specific experience. In particular, the certainty of a causal law deduced from ratiocination is significantly increased by its ability to explain a complex effect not previously thought in connection with it (Mill [1843] 1974: 462). If verification disconfirms the conclusion, and we cannot show where the fault lies, the deduction is imperfect, and it cannot be relied upon. According to Mill, the science which gives the most perfect example of the employment of the deductive method is Newtonian Physics. The latter was able to determine the laws of the causes (forces) which determine the motions of the bodies here on earth by means of experimentation and observation (i.e., direct induction), and then, by composing the causes (forces) that at each instance influence the motion of the celestial bodies was able to deduce their laws of motion (i.e., ratiocination), which then were confirmed by the celestial bodies' observed positions and velocities (i.e., verification) (Mill [1843] 1974: Chapter XI)

It is often possible, notes Mill, to dispense with the first step of direct induction and replace it by the method of hypothesis: the causal law being sought being hypothetically assumed as opposed to being derived from observation and experiment (Mill [1843] 1974: 492). This allows the deductive method to be applied earlier to phenomena and is justified when the verification can be carried out with sufficient rigor to establish not only that the causal hypothesis used accounts for the facts, but also that no other hypothesis can do so. To the deductive method, declares Mill, 'the human mind is indebted for its most conspicuous triumphs in the investigation of nature' (Mill [1843] 1974: 462). Although it did not place much epistemic import on the consilience criterion, the net import of Mill's account of scientific method is the acknowledgment of the superiority of the deductive method over direct induction and the legitimation of the method of the hypothesis (see also Ducasse 1960a: 231).

Mill's views dominated the philosophical discussions on scientific methodology in the second half of the nineteenth century. This dominance obscured the fact that all which was perceived as new and important in his contribution to scientific methodology were first set forth with clearness in John Herschel's historical treatment of the inductive science.[iv] Indeed, some of the most important elements comprising Mill's account of scientific method: the rendering of causal relations as invariance of succession, the use of the inductive methods of observation and experiment to discover causal relations, and the need to supplant the inductive part of scientific method with a deductive part, were first exposed with clearness in Herschel's (1830) *Preliminary Discourse on the Study of Natural Philosophy* (Ducasse 1960b).

Considering himself a true follower of Bacon, Herschel regarded induction as the process consisting in the formulation of generalizations and natural laws from concrete facts (Herschel 1830: 102-104). To achieve this goal, the main role of induction is that of *discovering* and *verifying* causal connections between the phenomena. Herschel's description of induction is better understood as composed of two parts: (a) the discovery of causal relations between phenomena, and (b) the proof or verification of causal connections (Herschel 1830: 104). Like Bacon, Herschel regarded the discovery of causal connections as a process of analysis which proceeds by applying *methodological rules* to assemblages of data about phenomenon of interest. Mill's account of direct induction is directly traceable to Herschel's methodological rules (Herschel 1830: 151-156).

Herschel's methodological rules aimed to establish causal connections between phenomena mostly by eliminating from a number of hypotheses regarding the cause of a phenomenon those which were incorrect. Herschel, however, seems to have been aware of the problems faced by induction as a strategy of elimination. From a logical point of view, the elimination of alternatives did not offer a positive argument for the truth of a causal hypothesis, and it could not solve the underdetermination problem. In addition, Herschel seems to have been aware of the tensions between the inductive accounts of scientific method and the increasingly successful use of deductive and hypothetical methods for justifying hypotheses in physics and chemistry during his time, especially hypotheses which dealt with the unobservable aspects of nature (Gower 1997: Chapter 6; Herschel 1830: 170-171). To solve both problems, Herschel added a second, deductive in nature, stage to traditional induction in which the first stage inductions are verified. In a move that perhaps influenced Whewell's consilience criterion, Herschel claimed that an inductive hypothesis is best confirmed when it is verified 'in the course of investigations of a widely different nature from those which gave rise to the inductions themselves' (Herschel 1830: 171).

> The surest and best characteristic of a well-founded and extensive induction…is when verifications of it spring up, as it were, spontaneously, into notice, from quarters where they might be least expected, or even among instances of that very kind which were at first considered hostile to them. Evidence of this kind is irresistible and compels assent with a weight which scarcely any other possesses (Herschel 1831: 170).

Herschel believed that we have strong reasons to accept the inductive hypothesis if it is verified by new and unexpected facts. But he is not explicit on where the epistemic strength of such a verification lies. In some passages, Herschel seems to regard it as being psychological in nature: 'In extending our inductions to cases not originally contemplated, there is one step which always strikes the mind with peculiar force, and with such a sensation of novelty and surprise, as often gives it a weight beyond its due philosophical value' (Herschel 1830: 172).

4. WILLIAM STANLEY JEVONS, CHARLES SANDERS PEIRCE, AND THE RISE OF HYPOTHESIS

The British polymath William Stanley Jevons was one of the few scholars who preferred Herschel's account of induction to that of Mill. In his methodological writings, Jevons attempted a similar integration of inductive and deductive methods of scientific investigation. In his influential work *Principles of Science* (1874) Jevons recognized the nineteenth century developments on both scientific methods and on the philosophical discussions on scientific methods (Madden 1960a). For Jevons, there is no such a thing like the distinctive method of induction standing in contrast to deduction, but that the two are mirror images of one another with induction being simply the inverse of deduction (Jevons 1874: vi):

> Within the last century a reaction has been setting in against the purely empirical procedure of Francis Bacon, and physicists have learnt to advocate the use of hypotheses. I take the extreme view of holding that Francis Bacon, although he correctly insisted upon constant reference to experience, had no correct notions as to the logical method by which from particular facts we deduce laws of nature. I endeavour to show that hypothetical anticipation of nature is an essential part of inductive inquiry, and that it is the Newtonian method of deductive reasoning combined with elaborate experimental verification, which has led to all the great triumphs of scientific research (Jevons 1874: vii).

Deduction, argued Jevons, is the development of the specific consequences of a general law. In deduction we learn the meaning, contents, results or inferences which attach to a general proposition. Deduction is a relatively easy process: based on specific rules, we determine what results or consequences will follow from a certain law. Induction is the exactly inverse process: given certain results or consequences, we are required to discover the general law from which they follow. In a sense, all knowledge is inductive because we cannot develop general laws of nature without observing particular facts. However, pointed out Jevons, the observation of particular facts is not sufficient for establishing general laws. There are no rules for guiding the transition from the observation of particular facts to the determination of the laws obeyed by them. For Jevons, induction is plagued by a problem of underdetermination (or *indetermination*, as he calls it). Using mathematics as an example, he points out the difference between induction and deduction and the difficulty in ascertaining the general law obeyed by observed facts.

> Given a general mathematical expression, we can infallibly ascertain its value for any required value of the variable. But I am not aware that mathematicians have ever attempted to lay down the rules of a process by which, having given certain numbers, one might discover a rational or precise formula from which they proceed. The problem is always indeterminate, because an infinite number of formulae agreeing with certain numbers might always be discovered with sufficient trouble (Jevons 1874: 142).

Another example echoes Whewell's favorite cryptogram metaphor:

> Compare again the difficulty of decyphering with that of cyphering. Anyone can invent a secret language, and with a little steady labour can translate the longest letter into the character. But to decypher the letter having no key to the signs adopted, is a wholly different matter. As the possible modes of secret writing are infinite in number and exceedingly various in kind, there is no direct mode of discovery whatever… Induction is the decyphering of the hidden meaning of natural phenomena. Given events which happen in certain definite combinations, we are required to point out the laws which have governed those combinations. Any laws being supposed, we can, with ease and certainty, decide whether the phenomena obey those laws. But the laws which may exist are infinite in variety, so that the chances are immensely against mere random guessing. The difficulty is much increased by the fact that several laws will usually be in operation at the same time, the effects of which are complicated together (Jevons 1913: 143-44).

The only way out of this problem, claimed Jevons, is to already possess general knowledge in the form of general propositions and natural laws formulated as hypotheses and, by deduction, ascertain the exact particular facts required to make them true. For Jevons, the invention and successive trial of hypotheses constitute the very essence of the scientific method.

> Being in possession of certain particular facts or events expressed in propositions, we imagine some more general proposition expressing the existence of a law or cause; and, deducing the particular results of that supposed general proposition, we observe whether they agree with the facts in question. Hypothesis is thus always employed, consciously or unconsciously (Jevons 1874: 305).

More specifically, the method in question consists of three steps: (1) framing of some hypothesis as to the character of the general law, (2) deducing consequences from that law, and (3) observing whether the consequences agree with the particular facts under consideration (Jevons 1874: 308). Like the philosophers examined so far, Jevons was aware that the mere agreement of the hypothesis with already observed facts is not sufficient. Like Whewell and Herschel, he claimed that the verification of the hypothesis by new and unexpected facts is often the sole and sufficient test of a true hypothesis.

> When once we have obtained a probable hypothesis, we must not rest until we have verified it by comparison with new facts. We must endeavour by deductive reasoning to anticipate such phenomena, especially those of a singular and exceptional nature, as would happen if the hypothesis be true (Jevons 1913: 504).

Although he was one of the few nineteenth century scholars to disavow certainty as a hallmark of scientific knowledge Jevons also claimed that: 'when a number of different facts, observed under the most diverse circumstances, are found to be harmonized under a supposed law of nature, the probability of the law approximates, closely to certainty' (Jevons 1874: 300). But even less than his predecessors, Jevons gave no explicit argument on why the ability of a hypothesis to be verified by different facts should make the hypothesis almost certain, nor he defined what is meant by 'different' in these cases.

Charles Sanders Peirce's method of *abduction* can be considered as a variant of the method of hypothesis. We should be aware, however, that Peirce's views on scientific methodology are found scattered through the large corpus of his work and are not always developed in detail. Further, Peirce's views on the matter were not fixed but changed through the course of his career. For the most part of his career, however, Peirce distinguished between two different types of ampliative (synthetic) inferences: induction and abduction (or hypothesis). Induction is where we generalize from a number of cases of which something is true and infer that the same thing is true of a whole class. Or, where we find a certain thing to be true of a certain proportion of cases and infer that it is true of the same proportion of the whole class. Abduction is where we find some very curious circumstance, which would be explained by the supposition that it was a case of a certain general rule, and thereupon adopt that supposition. The important difference between induction and abduction is that in induction we conclude that facts, similar to observed facts, are true in cases not yet examined. In abduction, on the other hand, we conclude the existence of a fact quite different from what has been observed and from which, according to known laws, what is observed would necessarily follow and therefore would be explained. Induction is reasoning from particulars to a general law. Abduction or hypothesis is reasoning from effect to cause. Induction classifies, abduction explains (Peirce 1878, §624). Like the philosophers before him, Peirce gives no definition on what is meant by 'different' or 'similar' in these cases. The lack of a clear criterion of what counts as similar or different blurs somewhat the distinction between induction and hypothesis. Sometimes Peirce provides another difference between the two. Thus, hypothesis, in contrast to induction, is more suitable for cases that are not possible to observe directly.

> The great difference between induction and hypothesis is, that the former infers the existence of phenomena such as we have observed in cases which are similar, while hypothesis supposes something of a different kind from what we have directly observed, and frequently something which it would be impossible for us to observe directly (Peirce 1878, §640).

For Peirce the method of hypothesis is intimately related with the study of the unobservables. Peirce's examples of hypotheses that involve assumptions about unobservable entities range from the "in principle unobservable" entities of atomic theory to the "practically unobservable" entities that are assumed in historical explanations (Peirce 1878, §625). The use of hypothesis to study the unobservable realm is another crucial characteristic that differentiates it from induction. According to Peirce, when we stretch an induction beyond the limits of what can be observed the inductive inference takes the form of a hypothesis. The inference becomes weaker the farther beyond the limits of the unobservable we go. If an induction is extended far beyond the limits of what can be observed then not much credence can be given to it unless it is found that such an extension explains some facts which we can and do observe (Peirce 1878, §640).

Usually, hypothesis provides a weaker argument compared to induction. Not all hypotheses are equal, however. When a hypothesis is first surmised to explain an observed fact, it is indeed a weak kind of inference. Peirce, however, distinguishes between mere hypotheses and successful theories. Whereas the term hypothesis is restricted to mere suppositions that have little evidence in their favor besides the fact(s) it has been invoked to explain, successful theories are guided by reason and have in their favor a variety of evidence which for the most part is different from the evidence it was initially invoked to explain. As an example of a hypothesis that became a successful theory Peirce mentions the kinetic theory of gases. When the kinetic hypothesis was first proposed by Bernoulli in 1738, it was only to explain the macroscopic behavior of gases and especially Boyle's law. Thus, it was hypothesized that matter is composed by small, solid particles, which in gases stand at great distances from each other, moving with great velocity, without sensible attractions or repulsions, until they happen to approach one another very closely. Since these suppositions were made in order to account for known facts, the kinetic hypothesis, as formulated by Bernoulli, was deservedly neglected. But, at the end of the nineteenth century, continues Peirce, the theory was supported by a "*considerable number of observed facts of different kinds*" (my emphasis) as well as from the mechanical theory of heat. Not only was the kinetic theory no more a hypothesis but it was something much more than an induction (Peirce 1878, §639).[v]

5. CONCLUSION

Perhaps the most important development in the nineteenth century philosophical discussions on scientific methodology was the effort to legitimize the use of the method of the hypothesis. The latter was thought to provide a more accurate representation of the scientific method used in the history of science and in the scientific practice of the time. It was especially considered to be the appropriate strategy for investigating unobservable phenomena, processes, and entities. Within the context of legitimizing the use of method of the hypothesis, the ability of a hypothesis to successfully explain or predict different kinds of facts—especially facts that played no role in the hypothesis' original formulation—was considered by many to a sufficient criterion of truth. This criterion was based on a no-coincidence argument: namely would not it be an improbable coincidence if a hypothesis can explain different kinds of facts, and yet be false? It is explicitly found in Whewell's notion of the consilience of inductions, but also in the writings of other nineteenth century philosophers like John Herschel, William Stanley Jevons, and Charles Sanders Peirce.

Despite the importance awarded to the consilience criterion in the nineteenth century, its function as a criterion of truth was plagued by several weaknesses. A first major weakness concerns the kind of independence involved. For consilience to function as a criterion of truth, the ability of a hypothesis to account for one class of facts ought to be independent from its ability to account for another class of facts. But although the proponents of consilience defined it as the ability of a hypothesis to colligate, explain, unify, or predict different 'classes', 'types', or 'kinds' of facts, they did not define what was meant by these terms.[vi] The failure to provide a criterion by which to differentiate between different classes of facts, meant that the decision to consider groups of facts as similar or different has an intrinsically historical dimension: it is a decision relative to the way knowledge is organized at a specific time-period—in fact, one can go as far as saying that it is a decision relative to the knowledge available to a specific researcher at a specific time and place. Thus, it seems that whether a hypothesis is consilient, is not based on some objective underlying—'ontological'—order but is contingent on the historical context and the knowledge available at a specific time and place (Laudan 1971; Musgrave 1974; Thagard 1978; Fisch 1985). Larry Laudan (1971: 374-375) has, in addition, pointed out the ironic fact that the historical role of consilience is to establish that facts thought to belong to different classes are similar exactly in virtue of their being accounted for by the same hypothesis. Second, even if one assumes the existence of a criterion able to differentiate between different classes of facts, it is not clear why the ability of a hypothesis to explain different classes of facts ought to be regarded as a criterion of truth. No convincing argument was provided for its strong epistemic force besides the blunt, psychological in nature, no-coincidence argument already discussed. Although the blunt rationale was sometimes bolstered with appeals to the history of science, this appeal in the end does more harm than good. Opponents of scientific realism cite the very same consilient theories appealed to by nineteenth century philosophers to show that the ability to explain or be supported by 'different' classes of facts can also be a characteristic of rejected theories. Whewell's effort to justify consilience by appealing to the *a priori* origin of hypotheses and the simplicity of nature is also inadequate from an empiricist point of view which requires such claims to be the result of scientific investigation rather than its premises.

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[i] A second metaphor that Whewell uses to convey the psychological force of Consilience is that of the independent testimony of two witnesses (Whewell 1840b: 446).

[ii] Snyder 2006, especially chapter 1, provides a detailed account of the theological foundations of Whewell's epistemology.

[iii] Janssen 2002 provides a historical account of this pattern of reasoning

[iv] This, however, was not the fault of Mill who fully acknowledged his debt to Herschel's discussion of induction (Mill [1843] 1974: xlv).

[v] In the final phase of his writings, Peirce relinquished the belief that there was any fundamental difference between induction and hypothesis and argued that the two are intimately tied to one another (Laudan 1973). According to this later view, hypothesis or abduction is the procedure which suggests the theories which inductions subsequently verifies (Peirce c. 1905, §755).

[vi] This is a problem that Whewell himself, unwittingly perhaps, seems to have recognized (Whewell 1860: 193-194)