WILLIAM WHEWELL ON THE CONSILIENCE OF INDUCTIONS*

Most contributions to Whewell scholarship have tended to stress the idealistic, antiempirical temper of Whewell's philosophy. Thus, the only two monograph-length studies on Whewell, Blanché's Le Rationalisme de Whewell (1935) and Marcucci's L' 'Idealismo' Scientifico di William Whewell (1963), are, as their titles suggest, concerned primarily with Whewell's departures from classical British empiricism. Particularly in his famous dispute with Mill, it has proved tempting to parody Whewell's position in the debate by treating it as a straightforward encounter between an arch-empiricist and an arch-rationalist. There is, however, a danger that an emphasis on the necessitarian and a priori elements in Whewell's philosophy may well obscure the unmistakable empirical emphasis in Whewell's theory of science. I think it is time to begin to redress the balance, by focusing attention on the significant 'empiricist' strains in Whewell's philosophy of science. One of the most important of those strains is connected with the operation which Whewell calls 'the consilience of inductions'.

Indeed, of all the fanciful neologisms which Whewell coined (including 'the colligation of facts', 'the explication of conceptions', 'the decomposition of facts' and 'the superinduction of conceptions'), none denoted a more fertile methodological process nor a more important doctrine in Whewell's methodology, than 'the consilience of inductions'. This fact alone would more than justify a close scrutiny of this doctrine. However, a fuller understanding of the nature of Whewell's views on consilience is not only vital to a comprehension of his philosophy of science, but is also the key to his historiography of science, for it is largely in terms of consilences.

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that Whewell formulates his theory of the \textit{progressive} nature of scientific growth and evolution. This paper is designed to give a brief explication of Whewell's notion of the consilience of inductions, along with an assessment of the rôle which consilience and related concepts play in Whewell's history and philosophy of science.

In the fourteenth aphorism concerning science of the \textit{Philosophy of the Inductive Sciences} (1840), Whewell offers what is probably his briefest characterization of the nature of consilience:

The Consilience of Inductions takes place when an Induction, obtained from one class of facts, coincides with an induction, obtained from another class. This Consilience is a test of the truth of the Theory in which it occurs.\(^1\)

To paraphrase, if two chains of 'inductive reasoning' from seemingly different classes of phenomena lead us to the same 'conclusion', a consilience of inductions has occurred.

In the light of this aphorism, there are two significant questions to ask initially about the consilience of inductions: (a) precisely what is involved in a consilience?\(^2\) and (b) why should a successful consilience count as 'a test of the truth of the theory'? It is these exegetical questions I will examine in Sections I and II below. In Section III, I will discuss how Whewell applies the notion of consilience to the history of science; while in Section IV I will look at the ways in which subsequent methodologists and logicians of science have reacted to Whewell's requirement of consilience.

I

Because the notion of consilience is so closely connected with Whewell's doctrine of induction (being effectively the result of two or more inductions leading to the same general proposition), it is important to be clear at the outset about the nature of Whewellian induction. It is generally accepted that Whewell radically trans-

\(^1\) Quoted from the \textit{Philosophy of the Inductive Sciences founded upon their History} (2d ed., London: John W. Parker, 1847), Vol. II, p. 469. My italics. Hereinafter cited as PIS.

\(^2\) For brevity, I shall generally use the term 'consilience' as a shortened form of the phrase 'consilience of inductions'.

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formed the traditional meaning (s) of ‘induction’, i.e., Whewellian induction is neither *inductio per enumerationem simplicem* nor is it at all akin to the eliminative induction of Bacon or Mill. On the contrary, induction is seen by Whewell as a necessarily conjectural process whereby we introduce a new conception, not immediately available ‘in’ the known evidence, which ‘colligates’ that evidence, while going beyond it both in generality and in abstractedness.\(^3\)

Whewellian induction is thus similar to what Peirce was later to call *abduction* or *retroduction*,\(^4\) and consists essentially in finding some general hypothesis which entails the known facts. As Whewell says,

our Inductive Formula might be something like the following:

‘These particulars, and all known particulars of the same kind, are exactly expressed by adopting the Conceptions and Statement of the following Proposition’.\(^5\)

Using Whewell’s technical terminology, an induction is generally a successful colligation of facts by a clear and appropriate conception. Adapting Whewell’s language to the kind of schema required for a consilience, we might say that *the formula for a consilience of inductions* is as follows:

These particulars of different types, \(A_1 \& \ldots \& A_n (n \geq 2)\), and all known particulars of the same types, are exactly expressed by adopting the conceptions and statement of the following Proposition: \(\ldots\).\(^5\)

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\(^3\) ‘Thus in each inference made by induction, there is introduced some General Conception, which is given, not by the phaenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a New Generality’. *(Ibid., Vol. II, p. 49.)* It is important to stress that the ‘new generality’ is not merely the generality of the logical operator ‘all’; rather, the new generality is a consequence of the fact that ‘we travel beyond the cases which we have before us; we consider them as exemplifications of some Ideal Case in which the relations are complete and intelligible’. *(Ibid.)*

It was largely Mill’s inability to understand this latter sense of ‘generality’ which provoked the classic Whewell-Mill controversy on induction.

\(^4\) Cf. especially Peirce, *Collected Papers of Charles Sanders Peirce* (Cambridge, Mass.: Harvard University Press, 1934), 5.189, as well as his many other discussions of abduction. *(See also below, pp. 388.)*

Given that induction is the formulation of an hypothesis which will explain (or 'express') a class of known facts, it follows that a consilience of inductions occurs when we discover that the same hypothesis explains (or expresses) two (or more) classes of facts. Disguised within this terse statement are several important methodological vectors. Indeed, by unpacking Whewell's various treatments of consilience, I think we can suggest that a consilience occurs under the following circumstances:

(1) When an hypothesis is capable of explaining two (or more) known classes of facts (or laws);

(2) When an hypothesis can successfully predict 'cases of a kind different' from those which were contemplated in the formation of our hypothesis;

(3) When an hypothesis can successfully predict or explain the occurrence of phenomena which, on the basis of our background knowledge, we would not have expected to occur.

Throughout Whewell's writings, he frequently refers to different 'types', 'kinds', 'classes' or 'domains' of facts. However, he never concerns himself with providing criteria for determining that facts are of similar or different kinds. Whewell, however, is not alone in this, for more recent writers like Carnap [see, for instance, Carnap's *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1962), p. 575] and Popper (note 52, below), who adopt principles similar to (1) to (3), are equally vague in referring to events of 'different kinds'. A similar ambiguity can be found in most discussions of the so-called Principle of Limited Variety.

If, as we again find other facts of a sort uncontemplated in framing our hypothesis, but yet clearly accounted for when we have adopted the supposition—we are . . . led] to believe [it] without conceiving it possible to doubt' ([*Ibid*., Vol. II, p. 286. Cf. also *ibid*., Vol. II, pp. 427-28.)

In general, Whewell commentators and scholars have not seen that his notion of consilience of inductions embraces these different characteristics. Robert Butts, for instance, writes as if Whewell's theory of consilience requires no more than that a theory should successfully explain more than it was originally devised to explain. [See Butts, *William Whewell's Theory of Scientific Method* (Pittsburgh: University of Pittsburgh Press, 1968), p. 18.] As I point out below, this is a rather serious misreading of Whewelian consilience. On Butts' account, all consilience amounts to is the extremely weak requirement that a theory should go beyond its evidence. This doctrine has been a commonplace since the time of Bacon, and certainly represents no new insight on Whewell's part. (See my forthcoming *From Testability to Meaning*) If, on the other hand, my interpretation of Whewellian consilience is more or less correct, then Whewell's demand for consilience is a
As these instances suggest, and as I shall argue in detail later, a consilience of inductions occurs when an hypothesis is shown to have achieved a certain minimum of corroborated or confirmed empirical content. More specifically, an hypothesis achieves a consilience when it has shown itself to be capable of successfully explaining either different kinds of facts, or very surprizing facts. My interpretation here, and in what follows, is in rather sharp contrast to that of Robert Butts, who writes as if consilience is entirely a matter of empirical content; a consilience of inductions occurring whenever a certain minimal gain in generality has been achieved.\(^9\)

On the contrary, I am convinced that Whewell’s aim in stressing the consilience of inductions is not to maximize content, but to maximize the confirmation of an hypothesis. Of course, Whewell did believe that in the progressive growth of science, we advance towards theories of greater scope, range and generality. But (and this is crucial) increased generality is only a gain insofar as that greater generality is experimentally confirmed. Consilience is, effectively, a criterion of acceptability which stipulates that those hypotheses are most worthy of belief and acceptance which pass empirical tests of the kind sketched above.

To return to the cases outlined there, it is obvious that they are not mutually exclusive; if condition (3) is satisfied, so generally will condition (2).\(^10\) The grounds for distinguishing them as I have done are twofold: (a) that all three constitute slightly different methodological gambits, and (b) that the justification given for

much more subtle and exacting requirement than the trivial one Butts attributes to Whewell.

(Note added in proof: In Butts’ more recent studies of Whewell, as yet unpublished, he significantly modifies his interpretation of the consilience of inductions.)

In his otherwise excellent study *Le rationalisme de Whewell* (Paris, 1935), Robert Blanché devotes less than a page to the consilience of inductions. C. J. Ducasse similarly oversimplifies the matter by suggesting that the requirement of consilience is ‘in its essence identical [sic] with . . . the well-known maxim connected with the name of William of Occam’. [Blake, Ducasse & Madden, *Theories of Scientific Method: The Renaissance through the Nineteenth Century* (Seattle: University of Washington Press, 1960), p. 212.]

\(^9\) See, for instance, Butts, *op. cit.*, note 8.

\(^{10}\) But not always, for an hypothesis may explain some very surprizing phenomenon which was already known when the hypothesis was devised.
each of the three depends upon slightly different considerations.

Take the first case. A consilience of inductions of this type (which I shall hereinafter refer to as $CI_1$) does not, as $CI_2$ and $CI_3$ do, increase the empirical content of our theoretical knowledge. Such consiliences are concerned neither with the predictive ability of our theories nor with their ability to extend our knowledge into new domains. However, what a $CI_1$ does accomplish is the formal unification or simplification of our theories and hypotheses. By reducing two classes of phenomena—which had hitherto required separate and (seemingly) independent hypotheses or theories for their explanation—to one general hypothesis or theory, $CI_1$ clearly achieves a reduction in the theoretical baggage required to ‘carry’ the known phenomena. The advantages to be gained by this type of consilience are, therefore, formal or systemic, and not empirical, ones.

On the other hand, consiliences of type $CI_2$ [i.e., (2) above] represent an increase in the explanatory content of our science, insofar as the adoption of the new hypothesis (which exhibits $CI_2$) permits us to anticipate natural phenomena which were unknown (or at least unconsidered) when the theory was devised. Such fecundity is clearly an important attribute of a new hypothesis and Whewell views it as a strong mark in favour of an hypothesis if it can achieve a $CI_2$.\footnote{Note that $CI_2$ is to be distinguished sharply from the case where an hypothesis or theory merely predicts events of a kind similar to those already known when the theory was conceived. In the latter case (which I discuss below, p. 376-77), no consilience of inductions occurs, even though our adoption of the hypothesis is content increasing.} As Whewell remarks,

> when the hypothesis, of itself and without adjustment for that purpose, gives us the rule and reason of a class of facts not contemplated in its construction, we have a criterion of its reality, which has never yet been produced in favour of falsehood.\footnote{PIS, 1847, Vol. II, pp. 67-68. Cf. his remark that ‘when such a convergence of two trains of induction points to the same spot, we can no longer suspect that we are wrong’. (Ibid., Vol. II, p. 286.)}

Like $CI_2$, consiliences of type $CI_3$ mark a gain in the explanatory power of our theories. The special strength of the latter type of consilience, however, is that it not only permits us to explain (and
predict) more kinds of phenomena than we could previously, it also enables us to test our theory in a uniquely stringent way by seeing whether it successfully predicts phenomena which, in the absence of that theory, we would have regarded as either impossible, inexplicable, or at least highly unlikely. Embodied in CI3, therefore, is a notion something like what Popper was later to call 'the severity of tests'. If a theory has surprizing consequences which are in fact corroborated, we are inevitably disposed to regard it as a very sound hypothesis.\(^{13}\) Such, in brief, are the three types of consilience implicit in Whewell's methodology.

Although Whewell talks as if the value of a 'consilivie' hypothesis is that it explains (or expresses or predicts) events of different 'kinds' or from different 'classes of phenomena', the real strength of such an hypothesis is usually that it shows that events previously thought to be of different kinds are, as a matter of fact, the 'same' kind of event. For instance, Newton's gravitational hypothesis did not, in its own terms, explain 'different' kinds of events; rather, it showed that (for instance) the motion of the moon and the fall of a heavy body on earth were precisely the same type of event. It was (in a sense) merely a fortunate accident of historical circumstance that Newton propounded his theory at a time when these were regarded as very different types of phenomena.

Such considerations suggest that there is something essentially historical and relative about deciding whether a given theory achieves a consilience of inductions. Vis-à-vis the Cartesian system,

\(^{13}\) To my knowledge, the only philosopher of science before Whewell to formulate a requirement like CI3 was John Herschel. In his Preliminary Discourse on the Study of Natural Philosophy (London, 1830), he observes that 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 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. (Op. cit., para. 180).

Nonetheless, there is a subtle but important difference between Herschel's requirement and Whewell's CI3. Whereas Whewell attaches greatest importance to the explanation of surprizing facts, Herschel seems to lay greatest stress on the successful explanation of facts which had previously been regarded as counter-examples.
for instance, the Newtonian explanation of planetary and terrestrial motions was not consililative (in this respect), for Descartes had regarded both kinds of phenomena as due to the action of roughly similar types of vortices. Thus, the decision as to whether a given theory achieved a consilience of inductions can only be determined by a careful study of those other theories with which it is competing at a given time. Without a thorough knowledge of historical context, and without overt reference to alternative theories (perhaps merely in the form of so-called background knowledge), it is usually impossible for us to decide whether a given theory achieved a consilience or not. The reason for this is clear. A physical theory itself generally can not express the fact that the phenomena which it 'consiliates' are different in kind, for that theory—insofar as it explains them in similar terms—must regard them as phenomena of the same kind. Thus, consilience makes reference to at least two theories. It follows that it is misleading to say absolutely that a theory $T$ has achieved a consilience of inductions; rather, we should say that $T$, relative to the natural kinds specified in other (competing) theories $T_1 & \ldots & T_n$, has achieved a consilience.

II

The obvious question to ask is why Whewell regarded consiliences of the type outlined above as of great value. Construed as methodological rules, there is nothing very surprizing about CI$_1$ to CI$_3$; indeed, CI$_1$ and CI$_2$ had been frequently cited methodological rules many years before the *Philosophy of the Inductive Sciences*.\footnote{For the evidence for this claim, see my *From Testability to Meaning*, forthcoming.} What is interesting, however, are the reasons Whewell gives for regarding consiliences as extremely important characteristics of scientific hypotheses. As Whewell indicated in the Aphorism quoted above, he considers such consiliences as a 'test of the truth of the Theory' in which they occur. Moreover, they are tests in the strong sense of the term; that is, a *consilience of inductions constitutes a sufficient and not merely (nor even) a necessary test of the truth of the theory in which it occurs*. But why should a consilience, whether it brings about formal unification (CI$_1$) or an
increase in empirical content (CI₂ and CI₃), be regarded as a strong indication, let alone a proof, that the theory in which it occurs is true? It is the answer Whewell gives to such questions which I want to explore in this section.

In discussing the logic of hypothesis testing in the *Philosophy of the Inductive Sciences* and in the *Novum Organon Renovatum* (1858), Whewell differentiated several *degrees of severity* associated with the tests to which scientific hypotheses could be subjected. He begins by noting that *all* hypotheses must be sufficient to 'save' the *known* appearances: 'the hypotheses which we accept ought [at least] to explain phenomena which we have observed'.\(^{15}\) This, in itself, is a *very* weak test for an hypothesis, and Whewell feels that it is insufficient:

> they [i.e., hypotheses] ought to do more than this: our hypotheses ought to foretell phenomena which have not yet been observed; at least all phenomena of the same kind as those which the hypothesis was invented to explain.\(^{16}\)

An hypothesis which merely summarizes the known evidence, without successfully going beyond it, scarcely deserves a place in the theoretical structure of science since it merely says, perhaps in a different language, what is already contained in the existing observational foundation. However, if the theory can successfully predict results 'even of the same kind as those which have been observed, in new cases', then this 'is a proof of real success in our inductive process.'\(^{17}\) When a theory successfully manages to make such predictions, we have a natural inclination to regard it as true, or at least very nearly so. This is because the probability of our being able to make successful predictions while using a false hypothesis is, on Whewell's view, rather small.

> Men cannot help believing that the laws laid down by discoverers must be in a great measure identical with the real laws of nature, when the discoverers thus determine effects beforehand in the same manner in which nature herself determines them. . . . Those who

can do this, must to a considerable extent, have detected nature's secret. ¹⁸

But even if successful prediction vastly increases our confidence in a theory, it is still not sufficient to persuade us with certainty of the truth of the theory. Knowing the fallacy of affirming the consequent, and perfectly aware of the correct predictions which false theories had made in the history of science, Whewell is unwilling to take mere predictive success as a sufficient criterion of truth.

However, as one goes further up the scale of increasingly severe tests—to the stage where consilences of inductions begin to occur—it is a very different story. If, instead of being able to predict only phenomena of the same kind as the hypothesis was invented to explain, we can explain and predict, with its help, cases of a different kind (relative, of course, to other theories), then we have indubitable evidence for the truth of our theory:

These instances in which this [consilience] 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 supposition could, after being adjusted to one class of phenomena, exactly represent a different class, where 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. ¹⁹

Notice that this argument is a general one, in the sense that it applies to all three types of consilience (CI₁-CI₈) outlined in Section I. The argument itself clearly begs many questions. The first sentence in the passage quoted above can probably be taken as a reasonably accurate description of the psychological force of consilences. One has only to look at the effect Newton's 'consilience' (the famous 'Newtonian synthesis') had on his followers to see the soundness of Whewell's observation. However, the remainder of this

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¹⁸ Ibid., Vol. II, p. 64.

¹⁹ Ibid., Vol. II, p. 65. In a similar vein, he observes that "when the explanation of two kinds of phenomena, distinct, and not apparently connected, leads us to the same cause, such a coincidence does give a reality to the cause, which it has not while it merely accounts for those appearances which suggested the supposition." (Ibid., Vol. II, p. 285.)
passage seems to be suggesting that this attitude is *logically* justified, that it is simply impossible in principle that any hypothesis could achieve a consilience unless it were the true hypothesis for explaining the phenomena under investigation. But neither in this passage nor elsewhere does Whewell offer any valid argument to support his logical (as opposed to his psychological) claim. Elsewhere in his writings, Whewell similarly asserts the truth-insuring character of consilences: 'When such a convergence of two trains of induction points to the same spot, we can no longer suspect that we are wrong. Such an accumulation of proof really persuades us that we have to do with a *vera causa*.'\(^{20}\) Again he claims—as if repetition were a substitute for justification—that when an hypothesis, of itself and without adjustment for the purpose, gives us the rule and reason of a class of facts not contemplated in its construction, we have a criterion of its reality, which has never yet been produced in favour of falsehood.\(^ {21}\)

Ultimately Whewell falls back on the old saw (formulated by Descartes,\(^ {22}\) Boyle,\(^ {23}\) and Hartley,\(^ {24}\) among others) that we simply cannot conceive that a theory which works so successfully in new domains can still be in error. In almost all of his discussions of consilience, Whewell produces a metaphor, probably first elaborated by Descartes in the *Regulae* and in the *Principes*,\(^ {25}\) likening


\(^{22}\) 'Although there exist several individual effects to which it is easy to adjust diverse causes [i.e., hypotheses], one to each, it is however not so easy to adjust one and the same [hypothesis] to several different effects, unless it be the true one from which they proceed.' [Descartes, *Oeuvres*, ed. Adam & Tannery (Paris, 1897-1957)], Vol. II, p. 199. Cf. also *ibid.*, Vol. IX, p. 123.

\(^{23}\) 'For it is much more difficult to find an hypothesis, that is not true, which will suit with many phenomena, *especially if they be of various kinds*, than but with a few'. [Boyle, *Works*, ed. Birch (London, 1772), Vol. IV, p. 234]. For a detailed discussion of Boyle's methodology, see my 'The Clock Metaphor and Probabilism: The Impact of Descartes on English Methodological Thought, 1650-1665', *Annals of Science*, 22 (1966), 73-104.

\(^{24}\) 'And if the general conclusion of law be simple, and always the same, from whatever phenomena it be deduced, . . . there can scarce remain any doubt, but that we are in possession of the true law inquired after . . .' [Hartley, *Observations on Man* (London: J. Johnson, 1791), Part I, p. 541].

\(^{25}\) This analogy has a long pedigree. Descartes introduced it into modern
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the formation of scientific hypotheses to the decyphering of an inscription in an unknown language. In his reply to Mill, who had denied that consilience is an infallible sign of truth (see below), Whewell formulated the metaphor most explicitly. First, he argues for the strong confirming power of successful prediction:

If I copy a long series of letters of which the last half-dozen are concealed, and if I guess those aright, as is found to be the case when they are afterwards uncovered, this must be because I have made out the import of the inscription. To say, that because I have copied all that I could see, it is nothing strange that I should guess those which I cannot see, would be absurd, without supposing such a ground for guessing.26

Applying this metaphor to the stronger case of the consilences, Whewell suggests that we 'may compare such occurrences [i.e., consilences] to a case of interpreting an unknown character, in which two different inscriptions, deciphered by different persons, had given the same alphabet. We should, in such a case, believe with great confidence that the alphabet was the true one'.27 A consilience thus confers on an hypothesis 'a stamp of truth beyond the power of ingenuity to counterfeit'.28

A second metaphor Whewell sometimes invokes in discussing consilience is the testimony of witnesses to an event. Just as our belief that an event occurred is much stronger if two independent witnesses attest to it than if there is only a single witness, so our

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philosophy in his regula X and in Oeuvres, Vol. IX, p. 323. Boyle, Glanvill, Power and Locke borrowed the analogy from Descartes. For Hartley's version of the metaphor, see op. cit., notes 21, 15 and 350. See also, for other discussions of this methodological metaphor, Boyle's Of the Excellency and Grounds of the Corpuscular or Mechanical Philosophy (1674); Leibniz's Letter to Conring (1678); his Elementa Physicae (Ca. 1683) and his New Essays (1765), Chap. XII; Boscovich's De Solis ac Lunae Defectibus (London, 1760), pp. 211-12; 'sGravesande's Introductio ad Philosophiam (17—); D'Alembert's article 'Déchiffrer' for the Encyclopédie (17—); and Dugald Stewart's Elements of the Philosophy of the Human Mind (1814).

26 On the Philosophy of Discovery (London, 1860), p. 274. (Hereinafter cited as OPD.)

27 Ibid., pp. 274-75.

28 Id. id., p. 274.
confidence in a theory is much greater if it is supported by two separate chains of 'inductive' argument:

It [viz., consilience] is [like] the testimony of two witnesses in behalf of the hypothesis; and in proportion as these two witnesses are separate and independent, the conviction produced by their agreement is more and more complete. When the explanation of two kinds of phenomena, distinct, and not apparently connected, leads us to the same cause, such a coincidence does give a reality to the cause, which it has not while it merely accounts for those appearances which suggested the supposition. This coincidence of propositions is . . . one of the most decisive characteristics of a true theory, . . . [a] *Consilience of Inductions*.29

Whewell maintains that Newton's demand for *verae causae* in his First Rule of Philosophizing was, in reality, a formulation of the requirement of the consilience of inductions.30 Indeed, the ability of a theory to achieve a consilience is prima facie proof that 'we have to do with a *vera causa*.31 He suggests that Newton's *regula prima* can be formulated as the following methodological rule:

we may, provisorily, assume such hypothetical cause as will account for any given class of natural phenomena; but that when two different classes of facts lead us to the same hypothesis, we may hold it to be a *true cause*.32

As I said before, Whewell makes a very plausible case for regarding consilences as giving us greater confidence in the truth of an hypothesis or theory; indeed, a confidence of a much firmer kind than is achieved by the prediction of similars. What his specific arguments utterly and obviously fail to establish is that hypotheses

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32 *Ibid*. As he says elsewhere, 'Newton's [First] Rule of Philosophizing will best become a valuable guide, if we understand it as asserting that when the explanation of two or more different kinds of phenomena . . . lead[s] us to *the same* cause, such a coincidence gives a reality to the cause. We have, in fact, in such a case, a Consilience of Inductions'. (OPD, pp. 276-77).
which achieve a consilience are true and can be known with certainty to be true. That he is giving an accurate reflection of patterns of belief in the scientific community is beyond doubt. But that he has failed to justify such beliefs is equally clear.

In spite of the structural weaknesses in Whewell's argument, his interest in the problem of consilience was of long duration. Not only does it figure in almost all of his published methodological works from 1833 to 1860, but it was even a matter of concern to him in some of his early unpublished writings on scientific method. Thus, in a fragment now in the Wren Library of Trinity College, Cambridge, and probably dating from the late 1820s, Whewell formulates something like the requirement of consilience as one of the 'Rules of Philosophsizing'. His list of rules is worth reproducing in full:

Try to put these in a compendious form—
1. Our hypothesis must be capable of distinctly and appropriately connecting the phenomena.
2. Generalizations resulting from such hypotheses, if correctly obtained and eminently simple, are theories.
3. Theories which explain phenomena, detached from those which were used in the generalization, are highly probable, and advance to certainty as the number of unexplained phenomena is diminished.33

The third rule is not a straightforward statement of the consilience rule, for it contains no clear requirement that a theory must be able to explain phenomena of a kind different from those used to generate the theory in the first place. On its own, therefore, Rule 3 above only makes explicit reference to the fact that theories must be content-increasing. As I mentioned above, this was quite a common requirement by the 1820s. However, in a second draft of those same rules, Whewell adopts a revised form of Rule 3 which moves further in the direction of consilience:

[Rule] 3. If this theory explains uncontemplated and apparently detached facts it acquires a probability—[and] almost a certainty... the theoretical cause becomes a real cause. (ibid.)

33 Trinity College Add. MS. a. 7800. The manuscript is undated, and my dating
In the revised version, therefore, a theory becomes almost certain if, in addition to explaining facts 'detached from those which were used in the generalization' (which was all the first draft required), it explains facts of a kind 'uncontemplated' when the theory was discovered. Whewell still has not put the point as succinctly as he will later do. But two things are clear from this early manuscript: (1) that Whewell was concerned with the problem of consilience at an early stage, and (2) that from the 1820s onwards he was defining Newton's *verae causae* or 'real causes' as those which can achieve a consilience of inductions.

Entwined in almost all of Whewell's remarks on consilience is his view that scientific laws are necessarily true. Recall that Whewell's 'inductive formula' (above p. 369) was a warrant for arguing that if a given hypothesis explains or expresses certain facts, then that hypothesis may be the true expression for those facts. Ultimately, however, Whewell wants to establish a much stronger relation. Specifically, he wants the scientist to be able to say that the given facts can be expressed (or explained) only by adopting a certain hypothesis. This transition, from viewing an hypothesis as a possible explanation for a set of phenomena to regarding it as the only possible explanation for them, is a process which, on Whewell's view, defies logical characterization. It is essentially a temporal process occurring in the mind of a scientist and no formal schema can caputre its subtleties. What is clear, Whewell maintains, is that as an hypothesis or lawlike statement is subjected to increasingly difficult trials,

this conviction, that no other law than those proposed can account for the known facts, finds its place in the mind gradually, as the contemplation of the consequences of the law and the various relations of the facts becomes steady and familiar.

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is based chiefly on handwriting patterns. It is quoted with the kind permission of the Master and Fellows of Trinity College.

34 The ideal inductive formula, in Whewell's view, is this: 'The several facts are exactly expressed as one fact if, and only if, we adopt the Conception and the Assertion of the inductive inference.' (PIS, 1847, Vol. II, p. 90).

35 As he says mysteriously in *On the Philosophy of Discovery*: 'Induction is inconclusive as reasoning. It is not reasoning: it is another way of getting at truth'. *(Op. cit.,* p. 454).*

It is really the accretion of a number of different inductive 'proofs' all pointing towards the same conclusion which persuades the scientists that he has discovered a necessary truth. Our belief in the truth of an hypothesis becomes so strong that we cannot 'conceive it possible to doubt' the truth of that given hypothesis.37

There are basically two factors mitigating against the cogency of Whewell's claim that consiliences of induction guarantee truth. The one factor, which I have already mentioned, is the logical one that it is always possible for a universal theory to be refuted, no matter how successful it has been in achieving consiliences. The second factor is a development within Whewell's own philosophy of science. He goes to some pains to argue (more or less aping Kant) that experience cannot attest to the necessity of scientific truths.38 But if experience, no matter how extensive, cannot be used as evidence for the necessary character of scientific truths, then the consilience of inductions, which is manifestly an experiential matter, cannot justify our claim that a certain lawlike statement or hypothesis is a necessary truth. In spite of the fact that Whewell generally regards a successful consilience as an infallible sign of the truth of the theory in which it occurs, he occasionally is less emphatic about identifying consiliatory power with truth. He points out, for example, that the phlogiston theory was capable of explaining facts in such diverse domains as combustion and acidification.39 Strictly speaking, therefore, the phlogiston theory achieved a 'truth-insuring' consilience of inductions. Nonetheless, as new phenomena emerged which the phlogiston theory was unable to explain (or able to explain only by ad hoc and 'inadmissible operations'), that hypothesis was abandoned. Similarly, he concedes (without using this language) that the emission theory of light


38 In the 15th Aphorism Concerning Ideas, Whewell writes: 'Experience cannot conduct us to universal and necessary truths:—Not to universal because she has not tried all cases:—Not to necessary, because necessity is not a matter to which experience can testify'. (PIS, 1847, Vol. II, p. 445; my italics.)

achieved a consilience in explaining reflection, refraction and (with some difficulty) the colors of thin plates.

John Stuart Mill was quick to see the question-begging character of Whewell's arguments about the truth-guaranteeing nature of consilences. In his *System of Logic* (1843), he focusses specifically on Whewell's analogy between establishing the validity of a cypher and proving the truth of an hypothesis. Mill, who held that correct prediction is no more reliable a test of truth than sufficiency to explain the known evidence, argues that:

If anyone, from examining the greater part of a long inscription, can interpret the characters so that the inscription gives a rational meaning in a known language, there is a strong presumption that his interpretation is correct; but I do not think the presumption much increased by his being able to guess the few 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 also accord with the small remainder.40

Moreover, as Mill was repeatedly stressing, we can generally have no guarantees, however much we may have tested our hypothesis, that its next prediction will not be a false one, for there is nothing in principle to prevent an hypothesis which has achieved numerous consilences from failing dismally on the next occasion when it is put to the test.

Against such an attack, Whewell could only reply by falling back on the rather flimsy empirical observation that theories which have achieved a consilience of inductions have never been subsequently refuted:

the history of science offers no examples in which a theory supported by such consilences, has been afterwards proved to be false.41

III

That Whewell felt he could fall back on the history of science

41 OPD, p. 275. Elsewhere, he claims that 'there are no instances in which a doctrine recommended in this manner [i.e., by a consilience of inductions] has afterwards been discovered to be false'. (PIS, Vol. II, p. 286). And again: 'No
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for convincing evidence of the truth-attesting power of consiliences was no accident. On the contrary, the notion of the consilience of inductions—though not always by that name—provides the leitmotif for the structure of his History of the Inductive Sciences (1837). In the History and in the historical Part I of the Philosophy, Whewell was concerned with tracing the development of science in terms of certain categories of narration which provided natural terminal points for treating the history of a science or a theory. Chief among these narrative categories was his concept of the inductive epoch, along with its prelude and its sequel. Roughly speaking, an inductive epoch is what modern writers would call a scientific revolution. It was characterized by the repudiation of old ideas, the clarification of new conceptions of natural phenomena, and the unification of disconnected facts into a single general theory. The prelude, as its name suggests, was the period immediately preceding the inductive epoch, when major problems in the then dominant theory were exposed, and new concepts articulated. The sequel, in its turn, followed the epoch and consisted in the application of theories developed in the epoch to more and more phenomena in an increasingly precise manner. What chiefly characterized the inductive epoch, or scientific revolution, was one or more major consiliences of inductions. Thus, Newton's theory achieved a CI, vis-à-vis the laws of Kepler and Galileo, a CI, with respect to the perturbations of the moon, and a CI, in its anticipation of the flattening of the earth. Whewell claims that every scientific revolution is accompanied by, and characterizable in terms of, an associated consilience of inductions:

The great changes which thus take place in the history of science, the revolutions of the intellectual world, have, as a usual and leading character, this, that they are steps of generalization; transitions from particular truths to others of a wider extent, in which the former are included.42

However, it is not only to static theories or hypotheses that

example can be pointed out, in the whole history of science so far as I am aware, in which this Consilience of Inductions has given testimony in favour of an hypothesis afterwards discovered to be false'. (Ibid., Vol. II, p. 67).

Whewell applies his doctrine of consilences. He also believes that the consilience notion can function as a device for assessing the cogency and value of prolonged scientific research traditions. If, he argued, we look at the historical development of any scientific school or tradition (e.g., Newtonian mechanics, Cartesian physiology or catastrophist geology), there are generally clear signs in its development and evolution that its basic theories have become either (1) more complex, artificial and ad hoc, or (2) more simplified, more general, more natural and more coherent. In the former case, the degenerating complication of the theory is probably a sign that it is false, no matter how successful the theory may be in empirically explaining natural phenomena. In the latter case, however, where it has been possible to extend the theory to a wider domain of phenomena with no loss of formal simplicity, we have unambiguous evidence that the theory, at least in its fundamental assumptions, is correct. Clearly case (2), with its content-increasing gains at no loss of formal coherence and unity, is an example of a successful consilience of inductions. Whewell takes the view that it is a defining condition of progress in science that 'refuted' theories which are retained but modified must ascend to higher and higher levels of generality (by CI₂ or CI₃), whilst converging towards systemic simplicity and unity (by CI₄). As he puts it,

we have to notice a distinction which is found to prevail in the progress of true and false theories. In the former class all the additional suppositions tend to simplicity and harmony; the new suppositions resolve themselves into old ones, or at least require only some easy modification of the hypothesis first assumed: the system becomes more coherent as it is further extended. The elements which we require for explaining a new class of facts are already contained in our system. Different members of the theory run together, and we have thus a constant convergence to unity.43

With false theories, on the other hand,

The new suppositions are something altogether additional:—not suggested by the original scheme; perhaps difficult to reconcile with it. Every such addition adds to the complexity of the hypothetical

system, which at last becomes unmanageable, and is compelled to surrender its place to some simpler explanation.44

On Whewell's view, research traditions are not abandoned merely because they are refuted, but rather because their development does not result in significant consilences. As he points out in his brilliant 'On the Transformation of Hypotheses in the History of Science',45 virtually any theory can be reconciled with the phenomena if we are prepared to make a sufficient number of *ad hoc* adjustments to it. The only coercive argument against such a patched-up theory cannot be the empirical one that it fails to work, but rather that it has become increasingly complex, cumbersome and logically untidy.

IV

The notion of a consilience of inductions, once formulated by Whewell, became by the late nineteenth century, a frequently-cited characteristic of sound hypotheses. Some methodologists accepted the requirement of consilience without any reservations. Thus, Stanley Jevons in his *Principles of Science* (1874), echoes Whewell's claim that adequacy to explain the known appearances is not enough to make an hypothesis respectable. In what is virtually a paraphrase of Whewell, Jevons goes on to argue:

> 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.46

In spite of Mill's argument that the successful prediction of novel effects is no guarantee of the truth of an hypothesis, Jevons acquiesces in Whewell's belief that a successful consilience is 'the sole and sufficient test of a true hypothesis'.47 Other methodologists,

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45 *Transactions of the Cambridge Philosophical Society*, 9 (1851), 139-47.
such as Thomas Fowler\textsuperscript{48}, conceded that Whewellian consilences have great psychological impact on the minds of the scientists, but refused to accept Whewell's claim that consilience is a \textit{sufficient} test for truth. Fowler insists that 'what is required before a hypothesis can be placed beyond suspicion is \textit{formal proof}' for it, and he charges that Whewell never shows that a 'consiliative inference' corresponds to the canons of valid reasoning.\textsuperscript{49}

The methodological writings of Peirce are filled with discussions of the problem of consilience, although Peirce rarely calls it by the name. Thus, Peirce is but reformulating Whewell's CI\textsubscript{2} when he writes:

The other variety of the argument from the fulfilment of predictions is where truths ascertained subsequently to the provisional adoption of the hypothesis . . . lead to new predictions being based upon the hypothesis of an entirely different kind from those originally contemplated and these new predictions are equally found to be verified.\textsuperscript{50}

And CI\textsubscript{3} is virtually identical to Peirce's demand that a good 'hypothesis must be such that it will explain the surprising facts we have before us'. . . . \textsuperscript{51}

In more recent times, Popper and his school, perhaps more than any other group of contemporary philosophers of science, have addressed themselves to the problem of the consilience of inductions, though they (like Peirce) have not called it by that name.

Indeed, Popper's major 'discovery' of the 1950s was a re-formulation of the problem of consilience. Until his classic 'Three Views Concerning Human Knowledge' (1956), he had not seen the importance of theories which successfully make novel predictions. Since the mid-fifties, however, Popper, in his many discussions of the criteria for \textit{severe} tests, has required a 'good' hypothesis to do


\textsuperscript{49} \textit{Ibid.}, p. 114.

\textsuperscript{50} \textit{Collected Papers of Charles Sanders Peirce} (Cambridge, Mass.: Harvard University Press, 1958), 7.117.

\textsuperscript{51} \textit{Ibid.}, 7.220. See also \textit{Ibid.}, 7.58 and 7.115-116. Any number of similar formulations could be gleaned from a careful pruning of Peirce's writings.
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precisely what Whewell expected it to do. Consider, for instance, Popper's second 'requirement for the growth of knowledge':

... we require that the new theory should be independently testable. That is to say, apart from explaining all the explicanda which the new theory was designed to explain, it must have new and testable consequences (preferably consequences of a new kind); it must lead to the prediction of phenomena which have not so far been observed.\(^{52}\)

Popper's third requirement is that a good theory must also have passed 'some new and severe tests'.\(^{53}\) Taken together, these requirements correspond almost exactly to Whewell's consilience of inductions, particularly those forms I have identified as CI\(_2\) and CI\(_3\). Popper and Whewell are in full agreement that the best hypothesis or theory is the one which has predicted new phenomena, explained phenomena of different kinds, and made startling predictions. Indeed, the only significant difference between Popper and Whewell on this issue concerns the degree of confidence to be accorded to an hypothesis which passes severe tests (or, in Whewell's language, which achieves a consilience of inductions).\(^{54}\)

While Popper and a few other modern philosophers of science have been talking about the problem of consilience without calling it such, and apparently in ignorance of Whewell's treatment of it, there are others who have used the Whewellian phrase 'consilience of inductions' to refer to a rather different kind of problem than the one which interested Whewell. I am referring especially to Mary Hesse (and those like L. J. Cohen, Mackie and Kneale who follow her usage). In a paper entitled 'Consilience of Inductions',\(^{55}\) Hesse attempts to treat 'what Whewell called the "consilience of induc-

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\(^{52}\) Popper, Conjectures and Refutations (N.Y., 1968), p. 241. Italics in original. In his 'Three Views ...', Popper insists that 'there is an important distinction ... between the prediction of events of a kind which is known, ..., and, on the other hand, the prediction of new kinds of events (which the physicist calls 'new effects')'. (Op. cit., p. 117). For more details on this, see the Appendix to my forthcoming From Testability to Meaning.

\(^{53}\) Ibid., p. 242.

\(^{54}\) Another important difference is that Popper curiously denies that consilences of type CI\(_1\) are of any methodological significance whatever.

\(^{55}\) Hesse, 'Consilience of Inductions' in Lakatos, ed., The Problem of Inductive
tions'  from the point of view of confirmation theory. She has focussed there on a problem which, while related to, is nonetheless different from, Whewell's consilience. Specifically, she proposes to investigate a question of the following kind: Imagine that a theory $T$ is supported by evidence $L_1$, and that $T$ entails a law $L_2$, such that $L_2$ has no direct empirical support at the present time. The problem of consilience, as Hesse sees it, is this: Have we any legitimate grounds for believing $L_2$ in light of the fact that it is derivable from $T$ which is highly confirmed (in domains independent of $L_2$)?

This is, of course, an extremely interesting problem, and one which confirmation theorists have not adequately discussed. But this is clearly not the (Whewellian) problem of the consilience of inductions. Indeed, in Hesse's example, it is not even clear that a consilience of inductions has occurred, for there is no reason to think that $T$ has either made surpring predictions, or explained different 'kinds' of phenomena successfully.

Later in her paper, Hesse considers a rather different case, but it too has little to do with Whewellian consilences. In her second example, we are to imagine a theory $T$ (her example, like Whewell's, is Newton's theory) which (with additional premises) entails two laws, $L_1$ (Kepler's law of equal areas) and $L_2$ (Galileo's law). Here, she suggests that the problem of consilience is this: Are there grounds for thinking that the subsumption of $L_1$ and $L_2$ under $T$ increases their respective $c$-values (that is, their degrees of confirmation)? As in the previous case, she seems to have shifted the emphasis of the problem of consilience. From Whewell's standpoint, the real problem in Hesse's second example is this: How much does it increase our confidence in $T$ if it can explain both $L_1$ and $L_2$? He is not at all concerned with the effect of a consilience on the probabilities of the laws which a general theory consilates. Hans Reichenbach, in his Experience and Prediction (1938), discussed the problem that interests Hesse, calling it the problem of 'cross-induction'. I submit that it might avoid serious confusions in the

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Logic (Amsterdam: Holland, 1968), pp. 232-47. The discussions of Cohen, Kneale and Mackie are in the same volume.

Ibid., p. 233.

Reichenbach, Experience and Prediction (Chicago: University of Chicago
future if we were to adopt 'cross-induction' or some similar term for this problem, reserving the 'consilience of inductions' for the very different kind of inference which Whewell coined it to describe.\textsuperscript{58}

In spite of the extent to which considerations akin to consilience have found their way into almost every recent account of scientific method, the basic problem to which Whewell addressed himself is still unresolved. Precisely when, and how, an hypothesis reaches that threshold of confirmation (or severe testing) when it warrants belief is as intractable a problem for modern confirmation theorists as it was for Whewell. Like him, they tend to identify that threshold with a successful consilience, or, like Popper, they deny that any such belief-threshold exists. But their justifications for doing so seem no more clear-cut than Whewell's, in spite of the impressive array of formal tools of analysis which they have brought to bear on the problem.

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\textsuperscript{58}To my knowledge, the only recent technical discussion of the 'consilience of inductions', which uses that term in its Whewellian sense, is in Kneale's \textit{Probability and Induction} (Oxford: Oxford University Press, 1949), pp. 107-109. Strangely, Kneale later uses 'consilience' in the Hesian sense, failing to point out the crucial differences between the two. (Cf. Lakatos, ed., \textit{The Problem of Inductive Logic}, pp. 253-54.)