ABSTRACT

William Whewell is famous for his philosophy of scientific discovery. Too few recognize that Whewell has important things to say on the normative question of theory testing, perhaps because J. S. Mill minimized Whewell’s contribution at every point. We aim to show how the many facets of their debate arise systematically from a single key issue—the normative function of concepts in science. The resulting theory of scientific inference could provide new insights into the nature of inference in general.

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1. The Central Argument

After John Stuart Mill read William Whewell’s *The Philosophy of the Inductive Sciences*, he decided to expand his own treatise on deductive logic to include a book on inductive logic. Mill extracted everything that made sense to him in Whewell, who was most famously a historian of science at that time. The end product, published as Book III of Mill’s *System of Logic*, was the logical wheat of Whewell’s philosophy of science winnowed away from the non-logical chaff. Or, at least, that is the perception of those who have failed to understand the parts of Whewell that Mill filtered out. Whewell eventually responded to Mill in writing (“Mr. Mill’s Logic”, 1849, reprinted in Butts, ed., 1989), and Mill documented his continuing disagreement with Whewell in long footnotes added to subsequent editions of *System of Logic*. Mill’s hypothetico-deductive account of science was far more influential, and is far closer to contemporary philosophy of science.

As a logician, Mill must view language as a fixed background receptacle for the formulation of rival theories, which are tested and justified by their deductive consequences. The language of science may be enriched, but the expansion of the language is separate and detached from the inferential processes of science. Whewell took the opposite view, that science is continually rewriting its language by introducing new conceptions, or extending old ones in unanticipated ways, in an effort to understand nature’s true code. This fundamental difference in viewpoint leads to a *systematic* disagreement between Whewell and Mill on a series of issues.

It begins with a dispute about the word “induction.” Both Mill and Whewell agree to use this word to denote whatever process in science leads from particular facts to general propositions; or from observations to theories. Yet Whewell insists that the introduction of new conceptions is an essential part of every scientific induction, while Mill refuses to see it that way (section 2). Nothing less than the nature of scientific knowledge is at stake here, because Whewell’s view of how scientific hypotheses are tested places conceptual innovation at the center of the process. At least, that is the argument that we present in this essay.

We develop this argument in two ways. First, we have sections that highlight the positive features of Whewell’s philosophy, which support our final argument. This is done in sections 2 to 7, and in section 9, which introduces Whewell’s consilience of inductions. Second, we argue against various interpretations of Whewell, or alternatives to Whewell, which have been proposed by Mill as well as by contemporary philosophers of science. This happens in sections 7 to 14 excluding section 9. Then we go back to our positive program, in an attempt to make sense of Whewell’s realism in sections 15 and 16. Here we use the lessons of many recent arguments concerning realism to develop Whewell’s philosophy in a direction that is plausible and interesting.

In more detail, section 2 explains the differences between Mill’s and Whewell’s descriptions of scientific induction, while section 3 spells out those differences in terms of some examples. Section 4 then argues that Mill’s attempt at separating conceptual innovation from induction presupposes an empiricist theory of concepts that is implausible in the simplest of cases; namely, the case of inferring planetary orbits from observed positions. The implausibility of Mill’s view, and the advantages of Whewell’s alternative, are explained in the more general framework of curve-fitting in section 5. However, the strongest example in favor of Whewell’s viewpoint has to be Newton’s argument of universal gravitation (section 6). If there is any case in which the introduction of a new concept goes hand in hand with an inference to a general proposition, it has to be this example.

Section 7, on Whewell’s fundamental antithesis, adds one more essential component to Whewell’s philosophy. Whewell sees all of human knowledge as necessarily composed of both objective and subjective elements. The objective elements are supplied by the world (facts), and the subjective elements are supplied by the mind (concepts and theories). Any attempt by philosophers to clearly separate them in any absolute way is doomed to failure, yet philosophers must analyze these components nevertheless. Whewell’s solution to this dilemma is to draw the distinction differently in different contexts. Just as output of one process can be can be input to another, Kepler’s laws of planetary motion were theory for Kepler; yet facts for Newton.

Thus, Whewell is no stranger to the idea that facts are theory-laden or that facts can be in error. His epistemology is not the foundationalism of Mill, according to which the language of science is objec-

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1 Facts do not necessarily refer to the raw data. In this way, Whewell has already adopted the viewpoint of Bogen and Woodward (1988), whereby theories are compared to raw observational data in two steps: (1) phenomena are derived from the raw data, and (2) theories are compared with the phenomena.
tively fixed, and the observational data expressed in that language are objectively given.

This sets the stage for our argument against various interpretations of Whewell’s philosophy, beginning in section 8, with the idea that theories that pass rigorous tests (of the kind we describe in section 9) become entrenched in the minds of scientists such that they cannot conceive their contrary. As a psychological thesis, Whewell does hold this view, which might be likened to Kuhn’s (1970) view that scientists working within a paradigm never question its truth, except with an element of psychological necessity added in. However, there are those who infer from this that Whewell’s normative epistemology amounts to nothing more than the slogan “We must believe $p$, therefore $p$ is true.” For us, this view of Whewell’s philosophy moves too far in the opposite direction from Mill’s. It ignores the objective, experiential, element of knowledge altogether.

For Whewell the objective component of knowledge enters via the consilience of inductions (section 9). Consilience occurs when two separate inferences, or inductions, show a certain kind of agreement, which is then explained in terms of a common theoretical cause. The agreement is an objective fact, whereas the postulation of the common cause is essentially an act of mind. Both these things are essential elements of knowledge according to the fundamental antithesis. This is the view we develop further in the last two sections of this essay. It is in stark contrast with Mill, who thinks that the only thing of value in consilience is the deductive subsumption under general laws (section 10). It is different from Laudan’s view that Whewell is saying the same thing as Popper (section 11). It conflicts with the received view that Whewell is a historicist for whom the value of consilience derives from the psychological surprise of predictions (section 12). And it is inconsistent with Butts’s view that Whewell was an early hypothetico-deductivist (section 13).

All this leads us to ask whether Bayesianism, the most powerful philosophical framework at work in contemporary philosophy, is able to say what is really valuable in Whewell’s consilience of inductions. After working through some failed Bayesian explications, we propose one of our own, which does capture some of the formal aspects of consilience. But in the end, it is not a good explication of Whewell because it fails to explain the role of conceptual innovation.

For Whewell, concepts are a necessary part of every induction, and therefore a necessary part of every consilience of inductions. Concepts are needed to represent the causes of the phenomena, and without the conception, we have no knowledge of causes. The key to objective knowledge is not to minimize the intrusion of all subjective elements, but to refine our interpretation of the facts through a hierarchically organized process of successive generalizations.

Whewell’s dynamical view of conceptual renewal reminds us of Kuhn’s meaning variance of language, which is an aspect of his incommensurability thesis. Yet their conclusions concerning realism are diametrically opposed. Our explication of Whewell (sections 15 and 16) might be seen as an exercise on how to use meaning variance as an argument for realism, rather than against it.

2. The Seeds of the Whewell-Mill Debate

Mill (1874, p. 208) defined induction as the operation of discovering and proving general propositions, and Whewell (in Butts, 1989, p. 266) agreed with Mill’s definition. It seems obvious and almost banal to ask whether Whewell agrees that to infer “All humans are mortal” from the premise that “John, Peter and Paul, etc., are mortal” is an example of induction. Yet the surprising answer is that for Whewell, inferring “All As are Bs” from the premise that “All observed As are Bs” is an inadequate account of induction. How can they disagree on the examples if they agree on the definition of induction? As Whewell explains it (Butts, ed., 1989, p.140):

…it appears to be frequently imagined that the general proposition results from a mere juxtaposition of the cases, or at most, from merely conjoining and extending them. But if we consider the process more closely...we shall perceive that this is an inadequate account of the matter. 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.

It is not that Whewell is insisting upon some weird psychological thesis about what goes on in one’s mind when one infers “All As are Bs” from the premise that “All observed As are Bs”, it’s that this is simply not the pattern of induction at all.

It follows that Whewell understands the words in Mill’s definition of induction differently, and in particular he has an entirely different understanding of
the term “general”. What is necessary for Whewell is that (Butts, ed., 1989, p. 47) “In each inductive process, there is some general idea introduced, which is given, not by the phenomena, but by the mind.” A new conception is needed in order for the facts to be “bound together so as to give rise to those general propositions of which science consists” (Whewell, 1840, pp. 201-202). Whewell further insists that in every genuine induction, “The facts are known but they are insulated and unconnected . . . The pearls are there but they will not hang together until some one provides the string” (Butts 1989, pp. 140-141). The “pearls” are the data and the “string” is a new conception that connects and unifies the data. “All humans are mortal” does not qualify as general in Whewell’s sense because it contains no new unifying conception (‘human’ and ‘mortal’ do not count because they exist in the observed facts).

Some propositions of the form “All A’s are B’s” may count as general in the relevant sense. For example, “All planets revolve around the sun” may be the result of a genuine induction if the unifying conception of “revolving around the sun” is introduced to explain some common features of the planets individually, provided that those features do not already use the same conception. Induction is not defined by the result of the process, but by the relationship between the premises and the conclusion.

An intuitive way of characterizing Whewell’s idea is that a genuine induction is an inference to the best explanation, in which the explanation must introduce some theoretical “cause” that unifies and explains the facts of experience. “All humans are mortal” does not explain why John, Peter, and Paul are mortal in the appropriate sense, whereas “All planets revolve around the sun” does explain why the retrograde motion of the superior planets (the outer planets) occurs only in opposition to the sun. Thus, Whewellian induction may be thought of as a kind of inference to the best explanation, except that he is requiring more of explanation than deductive entailment.

Whewell’s introduces the special term “colligation” to refer to the conceptualization of facts. He was very explicit about the theory-ladenness, or at least the concept-ladenness, of all observational facts long before it was brought to our attention by Kuhn (1970). As Whewell describes it:

When anyone has seen an oak-tree blown down by a strong gust of wind, he does not think of the occurrence any otherwise than as a Fact of which he is assured by his senses. Yet by what sense does he perceive the Force which he thus supposes the wind to exert? By what sense does he distinguish the Oak-tree from all other trees? It is clear upon reflexion, that in such a case, his own mind supplies the conception of extraneous impulse and pressure, by which he thus interprets the motions observed, and the distinction of different kinds of trees… The Idea of Force, and the idea of definite Resemblances and Differences, are thus combined with the impressions on our senses and form an indistinguished portion of that which we consider as the Fact. (Butts, ed., pp.123-124)

Mill claims to endorse everything Whewell says about the colligation of facts, except for Whewell’s insistence that the colligation of facts is an essential part of every induction. Mill agrees that colligation is common in science, and even essential to science. He even agrees that concepts are added by the minds of scientists. Unlike Whewell, however, Mill thinks that a colligation takes place prior to an induction properly so-called, and should not be conflated with it. Therein lies the heart of the disagreement between them.

Does the dispute have any philosophical relevance today? Some philosophers may be tempted to dismiss the debate as largely terminological: Whewell and Mill agree that colligation occurs in science. Whewell calls every colligation an induction, whereas Mill uses the term “induction” in the more traditional sense of Aristotle and Hume. We agree that in part, the debate is terminological. But it is not merely terminological for two vitally important reasons. (A) It is a substantive question whether examples of scientific inference may be adequately reconstructed so that all conceptualization occurs independently of, and prior to, the generalization. (B) If some important inferences cannot be reconstructed in Mill’s way, then the conceptualization may be playing an epistemological role in the generalization that is more than merely psychological. We plan to support both of these theses.

3. Examples of Induction

Who is right about the scientific examples? Whewell and Mill argued over the precise nature of Kepler’s discovery of the elliptical motion of Mars. Kepler began with observations of the position of
Mars relative to the sun at various times.\(^2\) These observations might be represented as points scattered around the sun, from which Kepler inferred that Mars’s orbit is an ellipse.\(^3\) Could this example be reconstructed as an example of induction in Mill’s sense? Whewell and Mill agree that the conclusion of the inference is that “All points on Mars’s orbit lie on ellipse \(b\)” where \(b\) specifies a particular ellipse. So, in this example, the predicate A is “a position of Mars” and B is “lies on ellipse \(b\)” but Mill insists that the description part of the process, the process of describing those points as lying of ellipse \(b\), occurs prior to the induction itself. That is, he would say that the data are of the form “at time \(t_1\) Mars lies on ellipse \(b\), at time \(t_2\) Mars lies on ellipse \(b\), and so on.” Notice that for Mill, the predicates that appear in the general proposition also appear in the description of the data, and so the inference proceeds from a statement of the form “All observed A’s are B’s” to a conclusion “All A’s are B’s.”

On the other hand, Whewell insists that the conception of an ellipse is introduced during the induction. For Whewell, the data are not described using the concept of an ellipse, but in terms of more rudimentary ideas, such as ‘position’ and ‘time’; “at time \(t_1\) Mars is at position \(x_1\), at time \(t_2\) Mars is at position \(x_2\), and so on.” Only then are these facts “bound together” with the conception of an ellipse “so as to give rise to those general propositions of which science consists” (Whewell, 1840, pp. 201-202). So, Kepler’s conclusion is not a “mere union of parts” or a “mere collection of particulars” (Butts, ed., 1989, p. 163) or an extension of them.

In response to Mill’s protests, Whewell (Butts, ed., p. 280) notes that “The fact that the elliptical motion was not merely the sum of the different observations, is plain from this, that other persons, and Kepler himself before this discovery, did not find it by adding together the observations.” Like many of Whewell’s assertions, this quote may be misread. It could mean that the elliptical motion is not the sum of different observations because it extends to new cases. Or it could mean that the elliptical motion is not the mere sum of observations because the observations do not contain the conception of an ellipse. Whewell means the latter. On Whewell’s view, Kepler never observed an instance of the generalization at all. Instances are only inferred from the generalization after the induction is completed. Instances of a generalization are the building blocks of induction, they are the byproducts of induction. Mill gets the order wrong, and he gets the history wrong as well.

The earlier ‘mortality’ example is an induction for Mill, but not for Whewell. Kepler’s example is an induction for Whewell and apparently for Mill as well, though for quite different reasons.\(^4\) But are there cases that Whewell would see as inductions and Mill would not? Such examples are possible, for Mill insists that “any process in which what seems the conclusion is no wider than the premises from which it was drawn, does not fall within the meaning of the term” (Mill, 1874, p.210). On the other hand, if Whewell is viewing induction as a kind of inference to the best explanation, then the absence of new cases does not destroy the induction (though it might make it hard to test).\(^5\)

So imagine that an economist wants to explain the inflation rate of the Soviet Union, and uses the known inflation rates for all the years from 1917 to 1990. For Whewell, this can count as a genuine inductive inference provided that the explanation introduces a new conception; maybe the concept of price control. But Mill would be forced to say that there is no induction because there are no new instances of the proposition, due to the disintegration

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\(^2\) For a description of the method Kepler used to obtain these data, see the appendix of Hanson (1973), or section 5 of Forster (1988).

\(^3\) The example considers only Kepler’s inference to his first law.

\(^4\) Zac Ernst and Byung-Hoon Jeong have been quick to point out that Mill viewed Kepler’s contribution to the induction as merely providing the right description of Mars’ orbit. This is because he thought that (1) the periodicity of Mars’s motion was already established by prior inductions, and that (2) the orbit being an ellipse is uniquely specified by those observations. That is, the inferential part was done prior to Kepler, and therefore Kepler did not perform an induction at all (Mill 1874, page 214). Mill is wrong on both counts, we think. However, everyone can agree that “the Kepler example” is an example of induction if it includes the part that was not done by Kepler.

\(^5\) This is just about the only disagreement that we have with Snyder (1997), who views induction a la Whewell as involving both the colligation of facts and a generalization to new instances. We can find no direct evidence that this is Whewell’s view. The indirect evidence we have against it is roughly this: A colligation of facts with a generalization to new instance can still take part in higher level consilences of inductions, and can therefore be tested indirectly. If it can be a part of a consilience of inductions, then it should count as an induction.
of the Soviet Union in 1990. We think that intuition is on Whewell’s side in this debate, for the induced proposition does have implications about what the inflation rate would have been in 1991 had the Soviet Union survived. But for Mill this is not enough because there is no testable prediction.

4. Two Kinds of Empiricism

Mill claims that the property of “lying on ellipse $b$” is determined by and read from the data themselves. According to Mill (1874, p.216, Mill’s emphasis) “Kepler did not put what he had conceived into the facts, but saw it in them . . .” However, his argument for this is entirely spurious (Mill, 1874, p. 216): “A conception implies, and corresponds to, something conceived: and though the conception itself is not in the facts, but in our very mind, yet if it’s to convey any knowledge relating to them, it must be a conception of something which really is in the facts…” Mill’s idea is that the ellipse must be in the facts if the inferred proposition is true. Whewell does not deny it (Butts, ed., p. 280): “Kepler found it in the facts, because it was there, no doubt, for one reason; but also, for another, because he had, in his mind, those relations of thought which enabled him to find it.” However, the issue is not whether or not the orbit of Mars is really elliptical. The issue is whether Kepler, or any scientists, could see that it is elliptical prior to the induction. Whewell says no, and Mill has no convincing reply.

The case against Mill is even stronger given that the orbit of Mars is not exactly elliptical, which Newton made plain. Whewell does not take that opportunity to undermine Mill’s position, but he does allow for the fallibility of inductive inferences. In fact, he talks explicitly about the service of erroneous hypotheses in the following passage:

A maxim which it may be useful to recollect is this,—that hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error. The object of such inventions is to bind together facts which without them are loose and detached; and if they do this, they may lead the way to a perception of the true rule by which the phenomena are associated together, even if they themselves somewhat misstate the matter. The imagined arrangement enables us to contemplate, as a whole, a collection of special cases which perplex and overload our minds when they are considered in succession; and if our scheme has so much of truth in it as to conjoin what is really connected, we may afterwards duly correct or limit the mechanism of this connexion. (Butts, ed., p.149)

That is surely what Newton did. He corrected and limited the application of Kepler’s laws and explained why they are of such service to science despite their error. Mill fails to account for the example when he implies that the facts on which the induction is founded are incorrigible.

Mill is an empiricist about the origin of concepts, whereas Whewell is not. But it would be hasty to conclude that Mill’s epistemology is empiricist whereas Whewell’s is not. Certainly, Mill subscribes to a more traditional foundationalism, which de-emphasizes the role of the mind. If empiricism says that experience is the sole source of knowledge; then it seems fair to say that Whewell is not a strict empiricist. However, the issue is complicated by two things. First, this formulation of empiricism is easy to misread. It might be read as saying that experience is the sole source of our beliefs, in which case it is a thesis about the psychology of belief formation. On this reading, Whewell is definitely not an empiricist (but who is?). Rather, it should be read as saying that experience is the sole source for the warrant of our beliefs, in which case it is a thesis about the source of the justification of beliefs. Whewell jumps from psychological questions about scientific discovery to normative questions concerning hypothesis testing without much notice at times; so much so, that Mill (1874, p. 222) thinks that Whewell confuse two very different things, Invention and Proof: “The introduction of a new conception belongs to Invention: and invention may be required in any operation, but it is the essence of none.”

True: Whewell does believe that mental acts are essential features at every stage of scientific induction, and that mental acts are essential to invention or discovery. True: Whewell says nothing against the idea that the contexts of discovery are the same as the contexts of justification. But to say that the contexts are the same does not imply that he thinks that discovery and justification are the same. In fact, Whewell talks about invention (i.e. colligations) and justification (the tests of Hypotheses) and he does not conflate the two things.

Nevertheless, if the primary concern in contemporary philosophy of science is justification, and the Whewell-Mill debate is over the role of concepts in scientific learning, then Whewell’s epistemology must be anti-empiricist. We suspect that many philosophers of science may be skeptical of any view
that gives a justificatory role to mental concepts either because it violates empiricist strictures, or because talk of concepts is likely to be vague and unhelpful. For that reason, we plan to develop the view that Whewell’s epistemology is anti-empiricist in the innocuous way in which recent accounts of curve-fitting are anti-empiricist in an un-mysterious way (Forster and Sober, 1994).

5. Curve-Fitting as a Kind of Colligation

In order to demystify Whewell’s views about the role of mental concepts in theory justification, we think it is useful to explain what role concepts play in curve-fitting. We have already discussed one curve-fitting example (the Kepler example), but Whewell has a quite general description of examples of this kind (Butts, ed., 1989, pp. 211-237).

Whewell begins with a general account of “the Colligation of ascertained Facts into general Propositions” as consisting of (1) the Selection of the Idea, (2) the Construction of the Conception, and (3) the Determination of the Magnitudes. In the special case of curve fitting, these three steps correspond to (1) the determination of the Independent Variable, (2) the Formula, and (3) the Coefficients. The more modern name for “coefficient” is “parameter.” In the Kepler example the independent variable is ‘time’, the data are observations of Mars at various times, and the formula describes the positions of Mars as a function of time. It is the second step that introduces the conception of an ellipse. Formula, as the name implies, only provides the form of the functional relationship. It introduces a family of possible functions; that is, the formula asserts only that the orbit of Mars is some ellipse, without saying which ellipse. In the third step, the family of ellipses is fitted to the data, and the parameters (or coefficients as Whewell calls them) are estimated by their values in the best fitting ellipse. This is the ellipse we called b, in section 3, and this third step yields the specific claim that all points on Mars’ orbit lie on ellipse b.

There are two important points to notice. First, the data first enter the process in step 3. However, step 3 makes no sense unless the formula is already fixed because the “best fitting curve” means “the best fitting curve in a family.” If a different formula were chosen, then the “best fitting orbit” would not be an ellipse.

Does this establish that a new conception is introduced during the induction rather than prior to the induction? We think that it is important to understand the distinction that Whewell makes between the idea(s) that are used to express the facts and the conception(s) used to colligate the facts. “Fundamental Ideas, “says Whewell, “... [are] certain wide and general fields of intelligible relation, such as Space, Number, Cause, Likeness; while by Conception I denote more special modification of these ideas, as a circle, a square number, a uniform force, a like form of flower.” (Butts, 1989, p. 211). All facts, for Whewell, are mind-laden to the core, but the data are laden with ideas rather than conceptions. It may be that ideas are determined by the data, but conceptions are not.

If induction consists of the three steps combined, then the introduction of the conception is certainly an integral part of the induction, by definition. However, perhaps Mill could maintain that the induction proper comes in step 3? The answer is that he could not because there is no generalization, in Mill’s sense, occurring in step 3. Step 3 takes us from the assertion that Mars moves on some ellipse to the proposition that Mars moves on the ellipse b. It takes us from a general proposition to another general proposition, and not from particulars to the general, as Mill maintains.

Nor can Mill solve the problem by claiming that the general proposition in step 2 is “seen in the facts.” This is especially clear in curve-fitting examples because it is always possible that different families, or formulae, could fit the facts equally well. True, these other formulae may be more complicated, but if simplicity is a factor, it is surely “added by the mind.” There is no sense in which the data, by itself, determines the formula.

The same point is more clearly established by another feature of curve-fitting. Step 3 is not a deductive inference in which each curve bar one is falsified by the data. The data is almost always subject to random fluctuations of observational error, which means in general that no curve fits the data exactly. For that reason, step 3 is a statistical procedure, such as the Method of Least Squares, in which the best fitting curve is selected. Thus Mill’s reconstruction of the Kepler example contains a factual error, because it presupposes that the data pass exactly through the best fitting ellipse selected in step 3. How can ellipse b be “seen” in the data when the points do not go through ellipse b exactly?

Whewell, on the other hand, is well aware of this fact, and he discusses the Method of Least Squares...
explicitly. 6  “The Method of Least Squares is a Method of Means, in which the mean is taken according to the condition that the sum of the squares of the errors of observation shall be the least possible which the law of the facts allows,” and “by this method, thus getting rid at once, in a great measure, of the errors of observation, we obtain data which are more true than the individual facts themselves.” (Butts, ed., 1989, p. 223) “If we thus take the whole mass of the facts, and remove the errors of actual observation, by making the curve which expresses the supposed observation regular and smooth, we have the separate facts corrected by their general tendency. We are put in possession, as we have said, of something more true than any fact by itself is.” (Butts, ed., 1989, p. 227.)

In summary; to say that concepts are introduced in curve-fitting is tantamount, for Whewell, to the claim that families of curves are introduced in curve-fitting. As such, the claim is uncontroversial, and there is nothing mysterious, therefore, about the role of concepts curve-fitting. Moreover, it is uncontroversial that they added by the mind, for they are not determined by the facts. It is also plausible that they play a justificatory role, and modern accounts of curve-fitting explain why that should be true (Forster and Sober 1994, Forster 1999a, 1999b).

6  The method was first used by Gauss (1777 - 1855) as a way of inferring planetary trajectories from noisy data at the beginning of the 19th century.
Note that the general proposition that is the result of induction may entail a theoretical redescription of the observations “corrected by their general tendency.” So, for Whewell the process of induction is from a possibly error infested lower-level generalization, to a higher-level consilient law, back to a corrected version of the lower-level generalizations, which may be further tested to provide further justification. On Whewell’s account, Newton can use Kepler’s laws to argue for universal gravitation, and then go back and correct Kepler’s laws without defeating his initial inference. Mill is therefore unable to explain the force behind Newton’s style of argumentation.

Perhaps it is now clear why Darwin was so pleased with Whewell’s methodology. No single example in the *Origin of Species* provided convincing evidence of the existence of evolution, and it was also difficult to meet the deductive requirements of Mill’s hypothetico-deductive scheme. But it was possible to convince his readers that the pieces of evidence fit together such that it would be implausible to dismiss the overall pattern of the evidence as a mere coincidence.

7. The Fundamental Antithesis of Philosophy

We have seen that according to Whewell, ideas and conceptions are elements in the production of knowledge; they are what he refers to as the “subjective” elements of knowledge. But he did not consider them alone to be sufficient elements for the production of scientific knowledge. The central premise of Whewell’s epistemology is that all knowledge is constituted of both a “subjective” element and an “objective” element. “We can have no knowledge,” says Whewell, “except that we have both impressions on our senses from the world without, and thoughts from our minds within” (Butts, ed., 1989, p. 57). The objective constituent of science, on Whewell’s account, is composed of the facts—of what he calls “objects of sensation, of observation” (Butts, ed., 1989, p. 57).

Whewell’s fundamental antithesis roughly corresponds to a distinction between the component of knowledge provided by the mind (the subjective part) and that provided by the world (the objective part). His point is not that our beliefs divide into two mutually exclusive categories; subjective beliefs on the one hand, and objective beliefs on the other. Rather, his central point is that there is a fundamental inseparability of the two; the subjective and objective elements of knowledge affect each other in the ongoing processes of reasoning and knowledge construction. All our beliefs, or at least those beliefs that count as knowledge, are infected by subjective and objective elements at the same time. This even applies to those perceptual beliefs that Mill would consider to be the most objective and incorrigible kind. Whewell uses a simple example to illustrate his point:

If we take the terms Reasoning and Observation; at first sight they appear to be very distinct. Our observation of the world without us, our reasonings in our own minds, appear to be clearly separated and opposed. But yet we shall find that we cannot apply these terms absolutely and exclusively. I see a book lying a few feet from me: is this a matter of observation? At first, perhaps, we might be inclined to say that it clearly is so. But yet, all of us, who have paid attention to the process of vision, and to the mode in which we are enabled to judge of the distance of objects, and to judge them to be distant objects at all, know that this judgment involves inferences drawn from various sensations; -- from the impressions on our two eyes; - from our muscular sensations; and the like. These inferences are of the nature of reasoning. . . . All observation involves inferences, and inference is reasoning. (Butts, ed., 1989, p.61)

Reasoning is on the subjective side of his antithesis, while sensations are further towards the objective side of the dichotomy.

The fundamental antithesis between “object” and “subject” extends to scientific knowledge, where it manifests itself as an antithesis between fact and theory. Facts are on the objective side, while theories are the products of our mind, and are therefore on the subjective side of the dichotomy. However, this is not an absolute categorization for Whewell, but one that is relative to a particular historical context. For, that which plays the role of a theory in one context, may play the role of a fact in another:

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7 Whewell did not accept or endorse Darwin’s theory. That is another story, however, which involves discussion of Whewell’s personal relationship with Darwin and Whewell’s own theological commitments. The interested reader should see Ruse (1975) and Ruse (1976).
Is it a Fact or a Theory that the planet Mars revolves in an Ellipse about the Sun? To Kepler, employed in endeavoring to combine the separate observations by the conception of an Ellipse, it is a Theory; to Newton, engaged in inferring the law of force from a knowledge of elliptical motion, it is a Fact. There are, as we have already seen, no special attributes of Theory and Fact which distinguish them from one another. Facts are phenomena apprehended by the aid of conceptions and mental acts, as Theories also are. (Butts ed., 1989, p.176)

This allows for successively higher levels of generalization, in which knowledge is built up in a hierarchical structure. This is not the strictly accumulative view of progress championed by the logical positivists, and attacked by Kuhn (1970), for Whewell allows the higher levels to feedback and correct the lower levels, just as Newton’s corrected Kepler’s laws of motion. Nevertheless, it is the mutual interaction between the subjective and objective elements of our knowledge that drive the correction processes. In Whewell’s words:

In the progress of science, both the elements of our knowledge are constantly expanded and augmented. By the exercise of observation and experiment, we have a perpetual accumulation of facts, the materials of knowledge, the objective element. By thought and discussion, we have a perpetual development of man’s ideas going on: theories are framed, the materials of knowledge are shaped into form; the subjective element is evolved; and by the necessary coincidence of the objective and subjective elements, the matter and the form, the theory and the facts, each of these processes furthers and corrects the other: each element moulds and unfolds the other. (Butts, ed., 1989, p.75).

The opposition between subject and object has many manifestations for Whewell (Butts ed., 1989, p. 57), including Necessary and Experiential Truth, Ideas and Senses, Thoughts and Things, and Theory and Fact.

Notice that necessity falls on the subjective side of the dichotomy in this quote. This is a widely misunderstood facet of Whewell’s philosophy. According to Whewell’s definition (Butts, ed., 1989, p. 55) “necessary truths are those of which we cannot distinctly conceive the contrary.” From this psychological definition of necessity, it is clear that it falls on the subjective side of Whewell’s fundamental antithesis, while ‘experiential’ falls on the objective side of the dichotomy. It is confusing because it is just the opposite of how the contrast is understood in contemporary philosophy, where ‘necessity’ is often understood to be an objective commodity, as “physical necessity,” and experience is thought of as a subjective phenomenon.

Unfortunately, Whewell muddies the waters by talking about necessary truth in his definition, for the concept of ‘truth’ definitely falls on the ‘object’ side of the antithesis. He sometimes slips from one to the other, perhaps because he thought, as a matter of contingent fact, that laws that achieved the status of being necessary were always true. It does not mean that laws are true because their contrary is inconceivable to those entrenched in the new paradigm. Mill’s misunderstanding of Whewell leads to the following bewilderment on exactly this point:

Now I can not but wonder that so much stress should be laid on the circumstance of inconceivableness, when there is such ample experience to show, that our capacity of conceiving a thing has very little to do with the possibility of the thing in itself; but is in truth very much an affair of accident, and depends on the past history and habits of our own minds. There is no more generally acknowledged fact in human nature, than the extreme difficulty at first felt in conceiving any thing as possible, which is in contradiction to long established and familiar experience; or even to old familiar habits of thought. And this difficulty is a necessary result of the fundamental laws of the human mind. (Mill, 1874, pp.177-178)

Mill is correct to say that necessity, in Whewell’s psychological sense, is a poor candidate on which to justify the truth of theories. On the other hand, if empirical theories attain this psychological status if and only if they are empirically well established, then there is no reason why necessity cannot be an indicator of truth. Because this is also a source of controversy in contemporary Whewellian scholarship, we shall explain it more fully.

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8 We think that Whewell is wrong here, but we also think his mistake is not detrimental to his overall epistemology. We argue that in the next section.

9 Harper (1989) and Metcalfe (1991) accuse Forster (1988) of ignoring Whewell’s views about necessity, and his psychologism. This is a response to those criticisms.
8. The Nature of Necessity

Butts (1989, p. 23) is someone who also complains that Whewell “seems to have confused psychological conviction with objective empirical truth, and with inferential validity.” Indeed, it would be puzzling that a psychological conviction served to justify the objective truth of an inductive generality. As Butts points out, it would be like saying: “I believe p, therefore p is true,” or more accurately, “I must believe p, therefore p is true.” But is this what Whewell is saying? Nowhere does Whewell say that if a belief reaches the status of necessity, then this must be the sole source of its justification—that it has somehow lost touch with its experiential basis. In fact, Whewell explicitly denies this when he says (Butts, ed., 1989, p. 58, Whewell’s emphasis) “the terms which denote the fundamental antithesis of philosophy cannot be applied absolutely and exclusively in any case.” That is why it is misleading to think of necessity as Whewell’s criterion of truth.10

Nevertheless, Whewell has an early paper (1834), published ten years before his fundamental antithesis of philosophy, which appears to provide clear and damaging evidence for Butts and Mill’s interpretation of Whewell. In that paper (reprinted in Butts, ed., 1989, pp.79-100), on the nature of the truth of Newton’s laws of motion, Whewell endeavors to explain the apparent contradiction between saying (ibid., p.80) “that a law should be necessarily true and yet the contrary of it conceivable.” Given that this contradiction is only apparent for Whewell, there are three ways in which he could remove the contradiction. (1) The word “necessarily” in the phrase “necessarily true” differently from “necessary” in his psychological sense. (2) It is only apparent that Newton’s laws of motion are necessary. (3) It is only apparent that the contraries of Newton’s laws of motion are conceivable. Butts and Mill seem quite sure that Whewell is taking up option (3). We will argue that Whewell is opting for (2).

But note that even if (3) were Whewell’s view, at best Whewell is establishing that it is consistent to hold that Newton’s laws are necessary, and that they are necessarily true. This does not imply that their truth flows from their necessity. In fact, to the contrary, it is the other way around. Necessary truths must be necessary if both tokenings of “necessary” mean the same thing.

In any case, (3) is not Whewell’s view. In this 1834 paper, Whewell tries to base Newton’s three laws of motion on three axioms that he thinks are necessarily true (in his psychological sense). For example (ibid., p.81), “Axiom I: Every change is produced by a cause” expresses “a universal and constant conviction of the human mind.” Applied to motion (ibid., p. 83), Axiom I asserts that (ibid., p. 83) “when no force acts, the properties of motion will be constant.” He then concludes that:

…so far as the laws are announced in this form, they will be of absolute and universal truth, and independent of any particular experiment or observation whatsoever.

But though these laws of motion are necessarily and infallibly true, they are, in the form in which we have stated them, entirely useless and inapplicable. It is impossible to deduce from them definite and positive conclusions, without some additional knowledge or assumption. (ibid., pp.83-84)

That is to say, the three necessarily true axioms do not by themselves yield Newton’s three laws of motion, and this “will be clear by stating…the proofs of the laws of motion in the form in which they are employed in mechanical reasonings” (ibid., p.84, our emphasis). Thus, Whewell intends to use Axiom I in the form “when no force acts, the properties of motion will be constant” to prove Newton’s first law of motion in the form “a body not acted on by any force will go in a straight line with an invariable velocity.” The axioms are “independent of any particular experiment or observation”, whereas Newton’s laws do have empirical consequences. Therefore, to prove Newton’s laws from the three axioms, Whewell needs to add something that is empirical rather than necessary. If this is right, then Whewell is resolving the apparent contradiction by arguing that Newton’s laws are not necessarily true (option (2)).

It becomes very clear that this is what Whewell is doing when he explicitly states (ibid., p.97) the necessary and empirical parts of Newton’s first law as “Velocity does not change without a cause” and “The time for which a body has already been in motion is not a cause of change of velocity,” respectively. Why is “Velocity does not change without a cause” necessary for Whewell? Because velocity is a prop-

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10 This mistake can be traced back to Butts’s earlier writings. It also appears in Buchdahl (1971, p. 345), who says “Whewell’s necessitarianism was meant to provide an alternative to Mill’s own theory of validation.” Needless to say, it has also influenced a number of other scholars, and it is probably fair to say that it is the received view in Whewellian scholarship.
erty of motion and Axiom I implies that no properties of motion can change without a cause.

Our purpose is not to defend Whewell’s 1834 analysis of Newton’s laws. In fact, we think that it is easy to demonstrate that Whewell’s analysis is incoherent. Consider Axiom I, in the form “Every change is produced by a cause.” This is plainly consistent with the Aristotelian idea that a force is needed to produce any change of position. Yet the Aristotelian version of Axiom I is actually denied by Newton’s first law, which allows that a body can move with a constant nonzero velocity without a cause. That is, changes in position may be uncaused on Newton’s theory. But now it is apparent that the contrary of Axiom I is not only conceivable, but that the contrary of it is actually allowed by Newton’s first law of motion! Hence Whewell analysis is hopeless.11

Our point is that even if Whewell’s analysis were coherent, there are no grounds for saying that Whewell’s epistemology is exhausted by the slogan “I must believe p, therefore p is true.” Whewell is certainly not saying that “The contrary of Newton’s laws are inconceivable, therefore Newton’s laws are true.” And Whewell has definitely not confused psychological conviction with objective empirical truth.

Admittedly, we do read him as saying that the contrary of Axiom I is inconceivable, therefore Axiom I is true. Yet at the same time Whewell is very clear that Axiom I has no empirical content of its own. For him, it has the same status as an a priori arithmetical truth like “2+2=4”. Thus, whatever one thinks of Whewell’s views on necessity, it is very clear that, in the final analysis, they are not intended to provide a justification of truths that are in part empirical. Moreover, every law in the empirical sciences has an empirical part. Therefore, we must turn our attention to what Whewell actually says about testing scientific hypotheses in the Novum Organon Renovatum, which postdates both of the papers discussed in the last two sections.

9. The Consilience of Inductions

In this section, we will explain the role that colligation of facts plays in Whewell’s story about how theories are justified. That is, we will address the earlier worry that the difference between Whewell and Mill is merely terminological. Be warned, however, that Whewell’s words are often ambiguous, and he talks about the evidential and psychological considerations in the same breath. Consequently, it is sometimes hard to unravel the significant novelty and strengths of his approach. We intend our view as a faithful explication of Whewell, and we hope that it will gain its plausibility from the way it draws together the various elements of his philosophy into a unified whole (in a kind of philosophical consilience).

Whewell’s account of the process by which a hypothesis is justified can be seen as a succession of progressively more stringent tests of the hypothesis’s ability to predict and explain phenomena beyond those which had been included in the colligation from which it was constructed.

Test 1) The initial step in verification is rigorously reapplying the hypothesis to the observed phenomena and establishing that the induced proposition accurately represents all of the facts of observation.

“We are never to rest in our labours or acquiesce in our results, till we have found some view of the subject that is consistent with all the observed facts . . .” (in Butts ed. 1989, p.151). In contemporary terms, this is no more than the requirement that a hypothesis fits the seen data. Whewell is aware that this test is exceedingly easy to satisfy, and he proceeds to his second test:

Test 2) “Hypotheses,” says Whewell, “[O]ught to do more (than account for what has been observed): . . . 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 . . . That it does this with certainty and correctness, is one mode in which the hypothesis is to be verified as right and useful” (in Butts ed. 1989, p.151).

That is to say, a hypothesis is tested against data not used in the construction of the hypothesis, but nevertheless of the same kind as that which it was designed to explain. These first 2 tests are very familiar to us all, and there is nothing unique to Whewell here.

Whewell also acknowledges that the history of science contains many examples of false hypotheses that made correct predictions (e.g. the theory of phlogiston); he does not, therefore, think that a limited predictive ability is sufficient to convince us of

11 Note that Whewell’s analysis is not contradicted by the fact that history has since shown that Newton’s laws are false. Whewell would have the option of blaming the empirical part of those laws.
the truth of any hypothesis. It is when a hypothesis passes a more stringent test; a test that constitutes one of the ways in which a *consilience of inductions* can occur, that, according to Whewell, the evidence is persuasive enough to justify the conviction that the theory is a correct one:

Test 3) The *consilences of inductions* offer the most convincing evidence for a hypothesis’ validity in either of two circumstances:

(a) When a hypothesis “enables us to explain and determine cases of a *kind different* from those which we contemplated in the formation of our hypothesis” (Butts, ed., 1989, p.155).

(b) “[W]hen the explanation of two or more different kinds of phenomena (as the revolutions of the planets, the fall of a stone, and the precession of the equinoxes,) lead us to the *same* cause, such a coincidence give a reality to the cause. We have, in fact, in such a case, a Consilience of Inductions.” (Butts, ed., 1989, p.296).

Most of the literature on the consilience of inductions has focused on (a), probably because it is formulated in more familiar terms. Unfortunately, it is remarkably hard to see anything special in connection with the colligation of facts. We therefore believe that (b) is the more fundamental feature of consilience for Whewell, and that (a) is a consequence of (b). So we shall try to redress the bias in the present literature by emphasizing the common cause aspects of consilience.

In his *Laws of Phenomena and of Causes* (Butts, ed., 1989, pp. 177-185), Whewell distinguishes between lower-level statements (Laws of Phenomena) and higher-level theories (Laws of Causes) that tell us why phenomena occur. In the latter cases, he claims, we can infer not only that it is the same law that accounts for different kinds of observed facts, but also that those laws can (unexpectedly) be traced back to the same cause.

It is noteworthy that Whewell actually considers two circumstances in which a cause explains two separate classes of phenomena. The first case is when the explanation of one class may be “of the same nature as the explanation of the other class.” Whewell says of an explanation of this type:

That the cause explains *both* classes, gives it a very different claim upon our attention and absent than from that which it would have if it explained one class only. The very circumstance that the two explanations coincide, is a most weighty presumption in their favour. It is 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. (Butts, ed., 1989, p.330).

When two classes of phenomena are known to be of the same kind prior to the test, such as considering separate sets of positions of the same planet at different periods of time, then the test is really of type 2, where the predictions are of the same kind. However, when two *apparently* unconnected classes of phenomena are explained by a common cause, then the test is properly classified as a consilience of inductions:

When the explanation of two kinds of phenomena, distinct, and not apparently connected, leads us to the same cause, which it has not while it merely accounts for those apparent appearances which suggested the supposition. This coincidence of propositions inferred from separate classes of facts, is exactly what we noticed in the *Novum Organon Renovatum* (b. ii. c. 5, sect. 3), as one of the most decisive characteristics of a true theory, under the name of *Consilience of Inductions*. (Butts, ed., 1989, p.330).

In Newton’s theory of gravitation, the mass of the earth is the same if measured by the moon’s motion as it is when measured by terrestrial motion (see our section on Newton’s apple). The agreement of these measurements is an example of the consilience of inductions; the two inductions being Newton’s theory applied to the moon on the one hand, and Newton’s theory applied to terrestrial motions on the other hand. Prior to Newton, celestial and terrestrial phenomena were classified as different kinds.

However, this picture is not quite what one might have expected. One might have thought that the lower level induction leads to the hypothesis that $X$ causes $Y$, where $X$ and $Y$ are observable quantities. Then, in a second induction, we discover the causal law by which the same quantity $X$ is the cause of a second observable quantity $W$. For example, $X$ might have been the position of the Earth, $Y$ the position of a terrestrial projectile and $W$ the position of the moon. The consilience of these inductions might then reveal that $X$ is the common cause of $Y$ and of $W$. That is how causal explanation is understood in
Mill’s four methods of experimental inquiry (Mill, 1874, Ch. VIII, Book III), for example. However, such an understanding of Whewell not only gets the example wrong, but it would also leave no essential role for the colligation of the separate inductions.

It is therefore important to understand that this is not what Whewell has in mind. The correct picture is closer to the following: In the first induction, \( Y \) is a function of \( X \), where the function is characterized by a parameter \( a \). In the second induction, \( W \) is a function of \( V \), where the function is characterized by a parameter \( b \). Following Whewell’s three steps in the colligation of facts (section 5) in each case, we end up with measured values for the parameters \( a \) and \( b \). Then we find that there is a remarkable agreement in these values, which we explain by supposing that the two disparate classes of phenomena are actually the effects of a common cause. It is not the variables \( Y \) and \( W \) that are the effects of a common cause, but the relationships of \( X \) to \( Y \) and of \( V \) to \( W \) that are the effects of a common cause. And the common cause is of the values of the parameters \( a \) and \( b \). Since they represent the same thing (the mass of the earth in our example), they are subsequently replaced by a single parameter \( c \), at which time we have completed the higher level induction. The new parameter \( c \) represents the common cause.

Whewell’s notion of causal explanation is vastly more powerful than Mill’s for it allows that the common cause may be initially hidden from view. There is something in common between two otherwise disparate phenomena, but the common element must be extracted from the complexities of the surface phenomena, tested by consilience, and encoded as a single theoretical quantity. The first step of the process is an essential prerequisite of the other two, and the first step is none other than the colligation of facts. Only if the first step is taken for granted, may the inference be represented in the simpler form. It is therefore hardly surprising that Whewell’s “obvious thing to remark” on Mill’s methods (Butts, ed., 1989, p. 286) is that “they take for granted the very thing which is most difficult to discover, the reduction of the phenomena to formulae such as are here presented to us.”

An adequate understanding of Whewell’s theory of induction requires that common causes are common causes of phenomenological regularities, and that the causes must be abstracted from the phenomena. In our illustration, the common cause was represented by the gravitational mass of the earth. In this case, “the same cause” refers to the same token cause. There is plenty of direct evidence to show that Whewell did have such cases in mind.

However, it is also clear that in other cases, the common effects such as “the revolutions of the planets, the fall of a stone, and the precession of the equinoxes,” do not all refer to the same token cause. For the fall of the stone and the precession of the equinoxes are effects of the earth’s gravitational influence, while the revolutions of the planets are due to the sun’s gravitational pull. In that case, the consilience of inductions cannot refer to the agreement in the measurements of the same quantity. Instead, it must refer to something like the common form of the equations governing those motions, and the fact that they separately exhibit the same type of cause.

The agreement between the independent measurements of the Earth’s mass—the first induced from terrestrial motion, and the second inferred from the moon’s motion—is very much an experiential fact. It falls on the objective side of the antithesis. But Newton’s theory encodes this agreement by a single quantity; the earth’s mass. In this way, the agreement of independent measurements is expected, and it is even impossible to conceive the contrary other than by the intrusion of some kind of error. Once phenomena are redescribed as effects of a common cause, the theory is simpler and more unified, and for subsequent generations of scientists immersed in Newton’s theory, the failure of consilience is psychologically impossible to conceive. Whewell regards this tendency towards unity and simplicity as yet another test of hypotheses:

Test 4) “The last two sections of this chapter direct our attention to two circumstances, which tend to prove, in a manner which we may term irresistible, the truth of the theories which they characterize:—the Consilience of Inductions from different and separate classes of facts;— and the progressive Simplification of the Theory as it is extended to new cases. These two Characters are, in fact, hardly different; they are exemplified by the same cases. ... The Consiliences of our Inductions give rise to a constant Convergence of our Theory towards Simplicity and Unity.” (Butts 1989, p. 159).

Many Whewell scholars, such as Blake et al (1960, p. 212) and Butts (1973, p. 125), believe that the consilience of inductions and the simplicity or unity of theories amount to the same thing for Whewell. We admit that much of what Whewell says is unclear in this regard, but there is one interpretation of Whewell that would explain why such a misunder-
standing has occurred. For Whewell, we suspect, the consilience of inductions and the simplification of theories are examples of inseparable elements falling on opposite sides of his fundamental antithesis. On this reading, the consilience of inductions would clearly fall on the objective ("Facts") side of his fundamental antithesis, while simplification falls on the subjective side.\(^{12}\)

One thing remains certain. Whewell’s description of the consilience of inductions requires that “induction” be understood in his sense, and not in Mill’s sense. Therefore, any reason to value Whewell’s account of consilience is a reason to view the Whewell-Mill debate is more than merely terminological.

10. Mill’s Subsumption Under General Laws

In place of the consilience of inductions, Mill talks about the deductive subsumption of lower level empirical laws under more fundamental laws. As a preliminary step towards supporting our view that Whewell’s theory of confirmation is a genuine contribution to the philosophy of science, we argue that Mill’s account falls short.

As Mill describes it, “The subsumption (as it has been called) of one law under another [is] the gathering up of several laws into one more general law which includes them all” (1874, p. 335). From here, he goes on to relate the details of how, “The most splendid example of this operation was when terrestrial gravity and the central force of the solar system were brought together under the general law of gravitation” (1874, p. 335). This certainly resembles the process of the “jumping together” of inductions that Whewell describes. Mill also says that the laws of nature state that the fewest and simplest assumptions, which being granted, the whole existing order of nature would result (1874, p. 230). The trouble is that Mill does not tell us how one counts assumptions or what counts as simple; aside from a few examples, he gives no argument or analysis. Whatever analysis can be gleaned from what Mill has to say in regard to subsumption goes no further than saying that a \(K\) and a \(G\) are subsumed under \(N\) if \(K\) and \(G\) are deductively entails \(N\) and \(N\) is simple. Nor does Mill say what counts as simple. Presumably, \(K\) and \(G\) would not be subsumed under the conjunction \(K\&G\) because there are two assumptions here, rather than one. The problem here is that \(N\) could also be expressed as two assumptions, or as three, or as how-

\(^{12}\) Simplicity is subjective in the sense that it is a property of how the facts are represented or conceptualized.

ever many assumptions one is ingenious enough to devise. So far, there is nothing in Mill’s philosophy to compete with Whewell’s epistemological system.

Whewell’s account of consilience also gets around the common objection to deductive subsumption. The objection is that the account includes too much. For example, the idea behind the deductive account is that Galileo’s theory of terrestrial motion, call it \(G\), and Kepler’s theory of celestial motion, \(K\), are subsumed under Newton’s theory \(N\) because \(N\) deductively entails \(G\) and \(K\). The problem is that \(G\) and \(K\) are also subsumed under the mere conjunction of \(G\&K\). Therefore, subsumption by itself does not capture the idea that \(N\) is more unified or consilient. Whewell’s view does not have that consequence because the conjunction \(G\&K\) does not provide a common cause explanation of the effects described by \(G\) and \(K\).

True: the subsumption account triggers the right intuitions. But it fails to deliver on the important details; especially on key points to do with unification, simplicity, and explanation. In contrast, Whewell has a number of interesting things to say about all of these issues.

11. Laudan on the Consilience of Inductions

Laudan (1981) has one of the best discussions of the consilience of inductions in the literature, and yet it tends to minimize its more important elements. After correctly noting that Whewell sees induction as a process “whereby we introduce a new conception, not immediately given ‘in’ the available evidence, while going beyond it both in generality and degree of abstraction”, Laudan (1981, p. 164) proceeds to characterize the consilience of inductions in a way that makes no essential reference to the colligation of facts. He describes three circumstances under which consilience can take place (Laudan, 1981, p. 165):

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.
The notion of ‘explanation’ in (1) is too vague to do justice to the detailed constraints Whewell places on the consilience of inductions. (1) gives no credibility to Whewell’s insistence that inductions always superimpose a new conception on the facts not already seen in them. So, when Laudan says (1981, p. 166) that “Such consiliences are concerned neither with the predictive ability of our theories nor with their ability to extend our knowledge to new domains,” he is overlooking an obvious response from Whewell to the effect that knowledge of a common cause is new knowledge. Such knowledge is predictively useful because the measurement of the cause in one induction predicts its value in the other. And the same consilience also leads Newton to extend this knowledge to new domains. Therefore, (1) and (2) occur in all the same circumstances. They are not mutually exclusive as Laudan suggests.

Re (3): While we agree that the psychological expectations of scientists are important for Whewell, it would be misleading to suggest that a low prior expectation of the predicted phenomena is necessary for consilience. In fact, Laudan’s suggestion cannot apply in the case in which the second phenomenon is explained. How can we explain something that we do not expect to occur? For, almost by definition, something being explained is something that is already known and believed.

Laudan’s analysis of consilience leads to a number of spurious comparisons with other philosophers of science. On page 176, Laudan claims that condition (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….’” While (3) may be a sufficient condition for the presence of consilience, it does not support the view that Whewell’s consilience of inductions is identical to Peirce’s criterion.

Laudan (1981, p. 176) quotes Popper as requiring that a 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.

We do not think that this quote from Popper, or any other, proves Laudan’s claim that (p. 176) “Taken together, these requirements correspond almost exactly to Whewell’s consilience of inductions.” It is roughly true that (Laudan, 1981, p. 176) “Popper and Whewell are in full agreement that the best hypothesis or theory is the one that has predicted new phenomena, explained phenomena of different kinds, and made startling predictions.” But Popper has left out the same details as Laudan—details that go beyond anything that Popper has to say in important respects. It’s simply not correct to state, as Laudan (1981, p. 176) does, that “the only significant difference between Popper and Whewell on this issue, concerns the degree of confidence to be accorded to an hypothesis that passes severe tests (or, in Whewell’s language, which achieves a consilience of inductions).”

12. Logical versus Historical Theories of Confirmation

Earlier, in section 4, we took issue with Mill’s claim that Whewell confuses invention and proof. Yet we certainly admit that Whewell is not very careful in distinguishing between the psychology of confirmation and the normative facts of theory testing. He mostly talks in psychological terms, about the conviction of scientists, or their inability to conceive of contraries. Nevertheless, he also talks of truth, which clearly lies on the objective side of his fundamental antithesis. In most cases, we can easily infer his normative views from what he says about the psychological convictions of scientists. However, there are two cases in which this is not so straightforward. One case concerns the role of necessity in Whewell’s normative theory of hypothesis testing (section 8). The second case concerns whether evidence is stronger if it is discovered after, rather than before, the construction of a hypothesis.

Mill’s subsumption under general laws is a logical account of theory evaluation which gives no place to temporal considerations of this kind. How else can Whewell repudiate Mill’s logical theory if not by insisting on the relevance of historical consideration? This simple idea lends a priori support to the suspicion that Whewell’s is a historicist account of theory evaluation. We plan to argue that while Whewell agrees that scientists may be psychologically influenced by such considerations, it is not a part of Whewell’s underlying normative theory of hypothesis testing.

Mill thinks that Whewell’s views are historicist, and he explicitly challenges him on these grounds.13

13 See Musgrave (1974) for an excellent introduction to the distinction between logical and historicist accounts of confirmation. Other good discussions are found in Achinstein (1994), Mayo (1993, ch. 8) and Snyder (1994). Everyone credits Whewell with the historicist view.
Here is how Whewell himself describes the challenge (Butts, ed., 1989, pp.293-4):

> These expressions of Mr. Mill have reference to a way in which hypotheses may be corroborated, in estimating the value of which, it appears that he and I differ. “It seems to be thought,” he says (ii, 23), “that an hypothesis of the sort in question is entitled to a more favourable reception, if, besides accounting for the facts previously known, it has lead to the anticipation and prediction of others which experience afterwards verified.” And he adds, “Such predictions and their fulfillment are indeed well calculated to strike the ignorant vulgar;” but it is strange, he says, that any considerable stress should be laid upon such a coincidence by scientific thinkers.

Notice that Mill himself is not so very careful to separate the psychological issues from the normative issues. Whewell squarely addresses the psychological issues in his response (Butts, ed., 1989, pp.294):

> It was not the ignorant vulgar alone, who were struck by the return of Halley’s comet, as an evidence of Newton’s theory. Nor was it the ignorant vulgar, who were struck by those facts which did so much strike men of science, as curiously felicitous proofs of the undulatory theory of light … predicted by the theory and verified by experiment.

Mill is surely right that, in the case of Halley’s comet, the mere return of the comet should not have impressed anyone. It doesn’t take a rocket scientist to predict that a comet observed in 1531, 1607, and 1682, will return in 1759. More precisely, if the period were constant, then it would have passed the perihelion (the closest point to the sun) in the middle of 1759. However, the point of the example is that Halley’s comet did not return in the middle of 1759, but nearer the beginning of 1759, and that that fact was predicted by Clairaut’s computations of the perturbations due to the attractions of Jupiter and Saturn (Moulton, 1970, p. 431). Whewell, the historian of science, was well aware of these details.

What if Clairaut has not done his computations until after Halley’s comet reached its perihelion on March 13, 1759? For those who were capable of understanding and checking Clairaut’s calculations, the postdiction need not have been any less impressive. For those “ignorant vulgar”, who were not capable of checking whether Clairaut’s computations were fudged, the result would have been less convincing. That is, even on a logical view of confirmation, historical facts may be relevant to evaluating the strength of confirmation if the full logically facts are unknown.14

Our view is that Whewell subscribes to a logical theory of confirmation at the normative level. For instance, a sentence just below the ones already quoted directly supports our contention: “If we can predict new facts which we have not seen, as well as explain those we have seen, it must be because our explanation is not a mere formula of observed facts, but a truth of a deeper kind.” (Butts, ed., 1989, p. 294, our emphasis.) An historicist would insist that novel prediction is not only sufficient, but also necessary for confirmation, but Whewell never says that.

Given that Whewell frequently cites Newton’s explanation of the precession of the Earth’s equinoxes as an impressive example of consilience, he must be denying the historicist viewpoint because the precession was an already well established phenomenon. Whewell is therefore implying that the temporal order is not crucial from a normative standpoint, although the historical circumstances may change the way in which the evidential relationship is described. Hence, Whewell describes two different manifestations of the consilience of inductions: (a) when the second induction is new, it is described as a novel prediction, and (b) when the second induction is not new, it is described as a (common causal) explanation. These are different historical manifestations of the same test, and the strength of the test is just as strong in the second case so long as the explanation is correct (that is, so long as the phenomenon is correctly deduced from the theory). This process, of checking deductive relationships, is important, and something that Whewell emphasizes in discussing his tests of hypotheses.

13. Why Whewell is Not a Hypothetico-Deductivist

When philosophers of science contrast logical and historicist accounts of confirmation, they often assume that non-historicist accounts of confirmation must fall within a broadly hypothetico-deductive mold. Yet, we want to say that Whewell’s normative account of confirmation is non-historicist and that it is not hypothetico-deductivist. In this, we are disagreeing with Butts (1989, p. 17) when he labels

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14 Thus, statements scattered throughout the literature (to the effect that on the logical theory historical facts cannot be relevant to confirmation, e.g., Synder, 1994) are misleading, and require qualification.
Whewell as “one early form” of hypothetico-deductivism, and with Jeong (1991), who endorses Butts’s view. If the suggestion is that Whewell’s normative view of confirmation reduces to Mill’s subsumption under general laws, then they are wrong. In its most naïve form, hypothetico-deductivism says that evidence $E$ confirms hypothesis $H$ if and only if $E$ is true, and $H$ entails $E$. In the pages immediately following Whewell’s description of his tests of hypotheses (section 9), Whewell devotes considerable space to the need for checking the deductive relationship from theory to fact. It is this chapter VI “Of the Logic of Induction” in the Novum Organon that appears to lend support to the view that Whewell is a hypothetico-deductivist.

Whewell does think that lower level laws are deducible from higher level laws and he likens this relationship to a schedule of accounts. “Induction recognizes the ore of truth by its weight; Deduction confirms the recognition by chemical analysis.” (Butts, ed., 1989, p. 175.) On this view, induction, and consilience of induction, is still an essential part of recognizing the truth. It is not replaced by the deductive confirmation. Thus, in this chapter, Whewell repeatedly emphasizes that:

when we say that the more general proposition includes the several more particular ones, we must recollect what has before been said, that these particulars form the general truth, not by being merely enumerated and added together, but by being seen in a new light. No mere verbal recitation of the particulars can decide whether a general proposition is true... (Butts, ed., 1989, pp. 169-170)

We take this to be clear evidence that Whewell does not intend that this checking of the deductive “ledger” to replace his previous tests, or to make them redundant or irrelevant.

Moreover, it is clear that the statement of facts, such as Kepler’s laws, deduced from the more general proposition, in this case Newton’s theory of gravitation, will be richer in vocabulary than the versions originally formulated by Kepler. Kepler’s ellipses are conic sections, the areas swept out equal in equal times are areal velocities, and the period squared divided by mean radius cubes is proportional to the gravitational mass of the sun. In addition, Kepler’s laws are only derived on the basis of idealized assumptions that Newton knew to be false. In fact, it was exactly the fact that Newton was able to correct Kepler’s laws in a direction that accorded with the facts known in Newton’s time, that made Newton’s derivation so convincing. Yet at the same time, Newton’s inductive inference proceeded from the basis of Kepler’s laws. In fact, Whewell quotes Mill with great approval in this regard: “if Newton had been obliged to verify the theory of gravitation not by deducing from it Kepler’s laws, but by deducing all the observed planetary positions which had served Kepler to establish those laws, the Newtonian theory would probably never have emerged from the state of an hypothesis.” (Butts, ed., p.301). As Whewell is well aware, and Mill fails to notice, the inductive inference from Kepler’s laws to Newton’s laws placed Kepler’s laws in a new light. From this new point of view, Newton then argued that his laws explain the deviations of the observed facts from Kepler’s laws. Ultimately, Newton’s theory received additional support from deductions, which implied that Kepler’s laws were incomplete descriptions of the facts (i.e., that they are false). This is not consistent with hypothetico-deductivism, for how can a theory be supported by the deduction of facts if those facts are false?

For Whewell, the induction Newton’s laws from Kepler’s laws serves a different purpose. Its purpose is to demonstrate the unity achieved by the conception superimposed on the facts. Gravity is the cause that explains some relational facts represented by Kepler’s laws, and the laws of phenomena do not have to be exactly true in order to represent these relations. Whewell’s chapter on the logic of induction acknowledges that the explanation is not verified until the deduction in performed in the opposite direction. The problem with Whewell’s discussion is that he does not explicitly acknowledge that role that auxiliary assumptions play in these deductions. While it is very hard to believe that a historian of science of Whewell standing was aware of these details, it is also hard to believe that Whewell would not recognize Mill may be ignorant of them. In either case, it is clear that Whewell’s methodology does not reduce to Mill’s subsumption under general laws.

In a section in Whewell’s reply to Mill (Butts, ed., 1989, pp.265-308) Whewell criticizes Mill’s hope from deduction in “promoting the future progress of
Science.” Again, Whewell is making it very clear that his own recipe of learning from induction is quite distinct from Mill’s hypothetico-deductive method. For example, the following concession to Mill would not make sense if there were no distinction:

I am quite ready to admit than in Mental and Social Science, we are much less likely than in Physical Science, to obtain new truths by any process which can be distinctly termed Induction; and that in those sciences, what may be called Deduction from principles of thought and action of which we are already conscious, or to which we assent when they are felicitously picked out of our thoughts and put into words, must have a large share; and I may add, that this observation of Mr. Mill appears to me to be important, and, in its present connexion, new. (Butts, ed., pp.303-304.)

It is equally clear from this passage that Whewell is pointing to the difference between him and Mill concerning the “hypothetico-” part of hypothetico-deduction. For Whewell, the invention of hypotheses involves the colligation of facts, followed by the consilience of inductions and a convergence towards simplicity and unity. Mill’s hypothetico-deduction makes imposes no such constraints on theory construction. Moreover, the constraints on theory construction play an essential role in Whewell’s normative evaluation of science (that is, you can’t separate the context of discovery from the context of justification).

The conclusion of this section is that, if you try to force Whewell into a hypothetico-deductive mold, you will leave out essential parts of his normative philosophy of science. The same may be said of trying to force Whewell into a Bayesian mold.

14. Failed Bayesian Explications of Consilience

According to Bayesianism, the bearing of evidence on the confirmation of hypotheses is exhausted by relationships of probability. For example, a classical version of Bayesianism says that evidence \( E \) confirms hypothesis \( H \) if and only if \( E \) raises the probability of \( H \); that is, \( P(H|E) > P(H) \). This idea is an extension of naïve hypothetico-deductive, for it implies that if hypothesis \( H \) entails \( E \) then \( E \) confirms \( H \) (except in a special case in which \( P(H|E) = P(H) \)).

It follows from this classical Bayesianism, that the consilience of inductions, or any evidential consideration for that matter, leads to the confirmation of a hypothesis if and only if it raises the probability that the hypothesis is true. There are three ways in which this can happen: (1) The consilience raises the prior probability of \( H \), (2) increases the likelihood of \( H \), or (3) lowers the prior probability of the evidence \( E \), or some combination of the above. That follows from Bayes theorem:

\[
P(H|E) = \frac{P(H)P(E|H)}{P(E)}
\]

where \( P(H) \) is the prior probability of \( H \), \( P(E|H) \) is the likelihood of \( H \) and \( P(E) \) is the prior probability of \( E \). All three of these possibilities have been tried by Bayesians in an attempt to see what confirmational value there is in the consilience of inductions. None have succeeded in our opinion, and Bayesians tend to admit as much. Many commentators have concluded that there is no special value in consilience rather than blaming the Bayesian framework. While we do not recommend that Whewell should be seen through Bayesian spectacles, we do plan to show how Bayesian explications can do better if the introduction of the conception in the colligation of facts (the formula, or family of curves, in section 5) is taken seriously. First, we shall see why simpler explications, which ignore this structure, fall short.

Achinstein (1990) explores the idea that consilience is evidentially relevant because the prior probability of the evidence is low. This amounts to identifying consilience with circumstance (3) in Laudan’s list (see section 11): “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.” As we have already argued, it is a mistake to assume that the defining idea of consilience is the prediction of facts of a different kind, where it is essential that the facts are surprising in the sense that their prior probability is low. There is nothing in Whewell’s notion that precludes the explanation of facts that have a high prior probability from participating in a consilience of inductions. Achinstein’s explication ignores the other dimensions of consilience, such as Whewell’s description of consilience in terms of common cause explanation, and Whewell’s thesis that consilience leads to unification and simplicity. Achinstein points only to incidental properties of the evidence, whereas the consilience of inductions is a relationship between two colligations of facts.

Hesse (1968) and Laudan (1971) set up the problem in a way that may include such relationships: Let \( K \) be the hypothesis that the moon’s motion is Keplerian, \( G \) be the hypothesis that motion of terrestrial projectiles is Galilean, and let \( N \) be the hypothesis that terrestrial motion and the moon’s motion are both a certain kind of Newtonian motion that applies
to the terrestrial and celestial environment of the Earth. \( N \) deductively entails \( K \) and \( G \). Here we have two competing hypotheses about motion near the Earth. One it the disunified conjunction \( K \& G \), which treats projectile motion as Galilean and the moon’s motion as Keplerian. The other is Newton’s model, which enjoys the advantage of consilience and unification. The problem is to understand how that advantage translates into a confirmational advantage in the Bayesian framework.

Hesse’s (1968) story about how this works is roughly this: Initially \( K \) and \( G \) are seen as independent hypotheses in the sense that each is probabilistically independent of the other. That means that if we find direct evidence for \( K \), it will not raise the probability of \( G \). Then Newton comes along and adds a “conception of mind” (namely vector acceleration and the inverse square law of gravitation) which shows that both \( K \) and \( G \) follow from a unified model \( N \). Hesse’s intuition is that once \( K \) and \( G \) are seen as describing phenomena of the same kind, they are no longer independent; and so the direct confirmation of one will indirectly boost the probability of the other. The trouble is that the mere fact that \( K \) and \( G \) follow logically from \( N \) cannot have this effect on the probabilities. The reason for this is that coherent probabilities must already respect logical relations, for otherwise the probability assignments do not conform to the axioms of probability. So if \( K \) and \( G \) are independent before the deduction, they are also independent after the deduction. So, there is no boost in probability after all. For us, this is a reductio against Hesse’s premise that the consilience of inductions reduces to Mill’s subsumption under general laws (section 10).

Laudan gives up on Hesse’s idea that consilience makes \( K \) and \( G \) dependent and focuses directly on the confirmational advantage of \( N \) over \( K \& G \). The trouble is that both \( N \) and \( K \& G \) appear to have the same likelihood, in which case the only way in which consilience is going to favor \( N \) over \( K \& G \) is if \( N \) has a higher prior probability than \( K \& G \). The idea is that unified theories are a priori more likely to be true, presumably because we know a priori that nature is simple? This is proposed by Salmon (1970), and is the solution recommended by Butts (1977). However, this explication gives up on the idea that consilience has an experiential component whatsoever. It too gives up on the idea that consilience is a relation between colligations of facts this time by making consilience a property of hypotheses alone, instead of the evidence alone.

A major problem with the Bayesian analyses mentioned above is that they fail to analyze the example in sufficient detail. The key distinction is between a predictive hypothesis and a model (Forster, submitted). A predictive hypothesis is a hypothesis that is specific and precise enough to make predictions about the motion of a body – it specifies, for example, the precise trajectory of a body. \( K \) is not a predictive hypothesis because it simply says that the moon’s motion is Keplerian without specifying a particular Keplerian motion. Instead, \( K \) specifies a family of predictive hypotheses. There are various ways of labeling the members of \( K \), but for our purposes we can think of the various Keplerian orbits as indexed by different values of Earth’s mass. These values are merely ways of singling out members of \( K \), for the concept of mass does not appear explicitly in Kepler’s laws. Let \( K(m) \) be the predictive hypothesis that corresponds to the numerical value \( m \) for the Keplerian mass of the Earth. Then \( K \) asserts that \( K(m) \) is true for some value of \( m \). Similar remarks hold for \( G \). Thus, we have:

\( K = \text{There exists a number} \ m, \text{such that} \ K(m) \text{is true.} \)
\( G = \text{There exists a number} \ m, \text{such that} \ G(m) \text{is true.} \)

Given these distinctions, we may map the description Whewell gives of the colligation of facts in curve-fitting (section 5) onto this example. In each of the two inductions, \( K \) and \( G \) represent the formulae introduced in step 2. The parameter \( m \) is the “coefficient” and the determination of its value in step 3 will lead from \( K \) and \( G \) to predictive hypotheses \( K(m_1) \) and \( G(m_2) \), respectively, for some specific numbers such that \( m_1 \neq m_2 \). These numbers will be close, but not exactly equal due to observational errors and inaccuracies in the formulae.

The conjunction of \( K \) and \( G \) is defined as:

\( K \& G = \text{There exists a number} \ m, \text{such that} \ K(m) \text{and} \ G(m) \text{are true.} \)

Note that the \( m \)’s fall within the scope of different quantifiers. The truth of the conjunction \( K \& G \) does not require that the very same value of \( m \) that makes \( K \) true is the value that makes \( G \) true, which is why the conjunction \( K \& G \) is a disunified hypothesis. If the two inductions were treated as one, then the formula \( K \& G \) would lead to the composite predictive hypothesis \( K(m_1) \& G(m_2) \). The whole is no more than the sum of the parts.

In contrast, the Newtonian alternative is:

\( N = \text{There is a number} \ m, \text{such that} \ K(m) \text{and} \ G(m) \text{are true.} \)

\[ \text{16} \] The problem of old evidence raises a similar issue (see Eells, 1985, for discussion).
The Newtonian model requires that the Earth’s mass is a common cause of the Keplerian motion of the moon and the Galilean motions of terrestrial projectiles, and so the two token values of $m$ are constrained to take on the same value. That is the sense in which $N$, in contrast to $K\&G$, insists that celestial and terrestrial motions are of the same kind. If we select $N$ as the formula and colligate the facts $E_K&E_G$, then we arrive at the predictive hypothesis $N(m_3)$, where $m_3$ is different from the other two values, but close to them. Notice that we already have a mechanism whereby higher level laws may correct lower level ones to the extant that $K(m_3)$ is different from $K(m_1)$.

Note the logical relations between $N$ and $K\&G$: $N$ entails $K\&G$, but $K\&G$ does not entail $N$. $N$ is strictly stronger than $K\&G$. $N$ is like the assertion “There is someone in this class who is majoring in physics and philosophy” while $K\&G$ is like the assertion “There is someone in this class majoring in physics and there is someone in this class majoring in philosophy.” If $N$ is true then $K\&G$ must be true. But if $K\&G$ is true, it is possible that $N$ is false. At the level of predictive hypotheses, these logical relationships do not obtain.

Now, here is how the story of consilience should be told. There are two ways of telling it; one where we gather evidence $E_K$ about the moon’s motion first, and then look at how well the competing models predict evidence concerning terrestrial projectiles, $E_G$. This is consilience under description (a) in section 9. The likelihood of $K\&G$ with respect to $E_K$ is just the likelihood of $K$, since $E_K$ is irrelevant to $G$. Surprisingly, the likelihood of $N$ is the same, since we have no direct information pertaining to $G$ under this scenario. So, $N$ has no confirmational advantage with respect to predicting facts of the same kind (nor should it really, since both models make the same predictions about the moon). But the noticeable difference between $N$ and $K\&G$ at this stage is that $N$ makes quite precise predictions about terrestrial motions. The reason is that the moon’s motion has fixed a value for $m$, and this value now predicts the exact type of Galilean motion experienced on the surface of the Earth. $N$ is “sticking its neck out” and if its predictions are correct, then it should be rewarded with a boost in confirmation. So, the confirmational value of $N$ manifests itself when the evidence $E_G$ is as $N$ predicts.

It is a difference between prediction and accommodation that is at issue here: $N$ predicts $E_G$ while $K\&G$ merely accommodates $E_G$. All of this works out in terms of likelihoods as follows:

\[ P(E_K, E_G | K\&G) = P(E_K | K) P(E_G | E_K, (K\&G)) \]

where we have used the fact that $P(E_G | K\&G) = P(E_G | K)$. Both likelihoods on the right hand side of the equation are pretty low, so $P(E_K, E_G | K\&G)$ is very low. However:

\[
\begin{align*}
  &= P(E_K | K) P(E_G | E_K, N),
\end{align*}
\]

where we have used the previously mentioned fact that $P(E_K | N) = P(E_K | K)$. It follows that the likelihood of $N$ is greater than the likelihood of $K\&G$ to the extent that $P(E_G | E_K, N)$ is greater than $P(E_G | E_K, K\&G)$. But $P(E_G | E_K, N)$ is greater than $P(E_G | E_K, K\&G)$ for the reasons already explained: $N$ makes more specific predictions than $K\&G$ about $E_G$ when it is combined with the evidence $E_K$. On this Bayesian explication, consilience is therefore an evidential consideration, reflected in the likelihoods, which depends on the relationship between two colligations of facts.

Notice that Hesse’s intuition does not pan out. That is, there is no sense in which a confirmational boost in $K$ is transferred to $G$. In fact, we could imagine the case in which $K$ and $G$ are very well established, with probabilities equal to 1. After all, the hypotheses are very weak. If that is the case, then there is no confirmational boost for either $K$ or $G$. The role of the evidence $E_K$ is not to confirm $K$, but to fix the value of $m$. The main thing that happens after seeing the evidence $E_K$ is that $N$ makes predictions, not about the general truth of $G$ (which it already entails) but about which member of $G$ is the true hypotheses. We suspect that Hesse’s intuitions have led her astray because she has failed to make a careful distinction between models and predictive hypotheses.

Actually, the imaginary story fits the actual history fairly well. $K$ and $G$ were fairly well accepted as approximations in Newton’s time. Their confirmation was not at issue. At issue was whether the true member of $K$ and the true member of $G$ were consistent with the Newtonian model, which had probability less than one. That is, was the Keplerian mass of the Earth the same as the Galilean mass of the Earth? Since the issue was whether the acceleration at the Earth’s surface and the acceleration of the moon towards the Earth fitted the inverse square law of gravitation, Newton needed to know the radius of the Earth and the radius of the Earth’s orbit to answer that question. He initially worked with an incorrect estimate of the Earth’s radius, and therefore concluded that the consilience was bad, until that estimate was corrected ten years later. There is even
speculation that this delayed the publication of the *Principia*.

The other way of describing consilience in this example is in the style of description (b) in section 9: We collect the evidence $E_K$ and $E_G$ first, perform the two inductions $K$ and $G$ separately, and then notice that the Keplerian mass for the Earth is approximately equal to the Galilean mass of the Earth. At this point, the two inductions “jump together” to suggest that these are actually measurements of exactly the same quantity; viz. the mass of the Earth. The coincidence of the two measurements of the Earth’s mass tells us that if we were to use the unified model to extend the Keplerian induction to the Galilean domain, as described in the previous paragraph, then the prediction would fit. In other words, the consilience of two different inductions tells us that the unified model has not only the advantage of being unified, but also that it will not be disadvantaged by bad fit, or bad likelihood. For it is only when those things are combined that we should declare a confirmational victory. So, this view of the process boils down to the same Bayesian analysis as before.

The difference between these stories ((a) and (b)) concerns the temporal ordering in which the facts are gathered and the hypotheses are constructed. That the Bayesian analysis is the same in each case implies that the historical ordering is irrelevant to the confirmation (section 12).

So why don’t we recommend looking at Whewell through Bayesian spectacles? Well, the Bayesian view leads to a problem: $N$ logically entails $K&G$, so the probability of $N$ can never be greater than the probability of $K&G$ no matter how much evidence is collected, and no matter what prior probabilities we assign! So, even though $N$ may be boosted in probability, this boost is never enough to push it above the probability of $K&G$. Bayesians standardly redefine $K&G$ as equal to $K&G$ minus $N$, so that the deductive relation no longer holds (see Forster, 1995, section 3). It seems unhistorical to us that older laws should be reinterpreted in light of newer ones. Certainly, Whewell does not want to deny the deductive relationship in examples like this. He does not speak in terms of probabilities either, so there is more than one reason to reject Bayesianism as a good explication of Whewell.

The problem with the Bayesian formulation of the problem is that Achinstein, Butts, Cohen, Hesse, Laudan, Salmon, and others, have applied their Bayesian ideas to models rather than predictive hypotheses. But it is consistent with Whewell, and common sense, to think of consilience as relevant to the confirmation of predictive hypotheses. The predictive hypotheses that best fit the data are the bolder and more interesting candidates for the title of “true hypothesis.” The issue is not whether $N$ improves upon $K&G$, but whether $N(m_3)$ improves upon $K(m_1)\&G(m_2)$.

### 15. Linguistic versus Operational Realism

*Aphorism XXIV: Inductive truths are of two kinds, Laws of Phenomena, and Theories of Causes. It is necessary to begin in every science with the Laws of Phenomena; but it is impossible that we should be satisfied to stop short of a Theory of Causes.* (Whewell, in Butts, ed., 1989, p. 177)

The progression from Kepler’s and Galileo’s laws to Newton’s theory of gravitation is, for Whewell, an example of progress from laws of phenomena to a theory of causes. But should these causes, in this case the Earth’s gravity, be treated as real, or merely as convenient fictions that are useful for the purpose of tying together different parts of our models enabling them to predict successfully? There is little doubt that the increasing predictive power of our models is accompanied by their postulation of theoretical causes. This association is exactly what is implied by Whewell’s two descriptions of the consilience of inductions: (a) the prediction of novel facts, and (b) the postulation of common causes. But do we have good reason to think that the causes are real?

Those opposed to the realist point of view are quick to point out that this association is not obligatory. To see their point, let us represent a particular predictive hypothesis in the Newtonian model as $N(\text{mass}(m))$, where “mass” refers to the concept of gravitational mass introduced by Newton to quantify the strength of the Earth’s gravitational influence, and $m$ is a numerical assignment to the mass of the Earth. The concept “mass” actually introduces a class of mass predicates, $\{\text{mass}(m) \mid m \in \mathbb{R}^+\}$, where $\mathbb{R}^+$ is the set of all positive real numbers. The properties denoted by these predicates are mutually exclusive in the sense that any body can only truly possess one mass. The more complete representation of the Newtonian model is $N(\text{mass})$, where $N(\text{mass})$ asserts that for some $m$, (1) the Earth has the property $\text{mass}(m)$, and (2) the trajectories of all bodies under the influence of the Earth’s gravity are described by the predictive hypothesis $N(m)$. The problem for re-
alism is that part (1) of Newton’s assertion does not appear to play any essential role in the increased predictive power that $N(mass)$ has over $K&G$. That is, what would we lose if we were to replace $N(mass)$ with $N$, where $N$ simply represents a family of trajectories, where $N$ makes no mention of the concept of mass. It should be clear from the previous section that the logic of consilience goes through unscathed when we replace $N(mass)$ by $N$.

So, the Whewell-Mill debate has led us to this: Whewell’s insistence that a new concept is introduced by the mind in every act of induction appears to fit the history of science well, but what essential normative role does it play? Do concepts merely serve the bookkeeping needs of mental processes—needs for simplicity, order and systematization, without which the mind is unable to operate? If their purpose is to serve this psychological function, then where does this leave the realist idea that theoretical concepts truly capture features of the external world?

Friedman (1981, 1983) is one of many philosophers who have defended the realist side of the issue. His view is that some of the formal structure posited by theories should be taken seriously to the extent that it does real work in unifying the various phenomena that it explains. Our analysis in the previous section explains the sense in which $N(mass)$ does do some real work in unifying $K$ and $G$. The problem is that $N$ does this work just as well without the concept of mass.

This issue has been discussed in the philosophy of science before, under the heading of the Theoretician’s Dilemma (Hempel, 1965, p. 179):

The use of theoretical terms in science gives rise to a perplexing problem: Why should science resort to the assumption of hypothetical entities when it is interested in establishing predictive and explanatory connections amongst observables? Would it not be sufficient for the purpose, and much less extravagant at that, to search for a system of general laws mentioning only observables, and thus expressed in terms of the observational vocabulary alone?

Hempel’s own answer to the problem (Hempel, 1965, p. 180) is that “the assumption of nonobservable entities serves the purposes of systematization: it provides connections amongst observables in the form of laws containing theoretical terms, and this detour via the domain of hypothetical entities offers certain advantages…” However, these advantages appear to be nothing more that pragmatic advantages, rather than reasons that prove that theoretical entities really exist.

Earman (1978) tries out a different argument for realism, which he attributes to Ramsey. Imagine that there are two separate communities of scientists. One community consists of realists, who introduce theoretical concepts explicitly into their theories, and take the postulation of these theoretical entities seriously. A separate community of instrumentalist scientists works on the same prediction problems, except that they do not introduce theoretical concepts into their theories. Earman’s argument is that the instrumentalists will be less successful in solving the prediction problems. There is no Newton in their midst who is able to see the connection between celestial and terrestrial motions. The best they can do is send spies to the realist camp, steal their newest theories, and reinterpret them according to their anti-realist strictures. Earman refers to them as wait-and-see instrumentalists, because they are never the first to discover the important new theories.

Given the actual psychology of human beings, Earman’s story is highly plausible. As Whewell (Butts, ed., 1989, p. 184) aptly describes it, “In many subjects the attempt to study the laws of phenomena, independently of any speculations respecting the causes which have produced them, is neither possible for human intelligence, nor for human temper.” But there is little reason to think that it is logically impossible for instrumentalists to seek out ways to restrict the class of trajectories allowed by $K&G$, and thereby discover the restricted family of trajectories $N$ without making use of the concept of gravitational mass. Moreover, they are well able to test the model $N$ by its deductive consequences. In fact, it seems to us that this methodology of innovation and deductive testing is just what hypothetico-deductivism recommends.

But are the instrumentalists also capable of extending their models to novel phenomena? Are they able to learn from their successes, and apply the same patterns of discovery to the motion of Jupiter’s moons, or the motion of twin stars? Again, we see this is unlikely for human beings, which is to say that Whewell’s methodology has greater normative value for human beings than the hypothetico-deductive alternatives. However, this still does not resolve the issue concerning realism. Suppose that the normative value in Whewell’s methodology were limited to the goal of predictive accuracy. If science were limited to predictive accuracy, then Earman’s arguments persuade us that human beings are better off acting as if realism is true. The greater success of the
realists over the instrumentalists would not prove that realism is true. The practice or beliefs of scientists do not settle the issue of realism that concerns us here.

We therefore propose that the question should be asked differently: If a model is predictively accurate, does its accuracy indicate that it “latches onto” the world better than a less predictively accurate alternative? In our view, the answer to this question is in the affirmative. From this realist viewpoint, it doesn’t matter how a model is discovered, whether by realists or instrumentalists, or whether the model is represented in terms of the concept of mass or without it. The fact remains that the resulting hypotheses are predictively accurate because the world is a certain way; namely that there exists a common cause that accounts for the consilience of inductions. In some formulations of the theory, the cause is explicitly represented, while in other formulations (mainly ones that are invented by philosophers), the cause may not be represented. The slogan is: Look at how the science operates, and not necessarily at what it says.

There are many independent reasons for paying less attention to the linguistic content of theories. The history of science is full of dubious theoretical commitments, from crystalline spheres in the heavens, to light made up of particles, and then of waves, of gravitational forces that give way to distortions of space-time, and so on. Whewell rejects the atomic hypothesis despite its ability to “explain the occurrence of definite and multiple proportions” (Dalton’s law) and despite the fact that it correctly predicts other phenomena as well (Whewell, 1858, 2:49). As documented by Snyder (1997, p.188), the chemical facts do not support “any inference with regard to the existence of certain smallest possible particles” for Whewell, because “the assumption of indivisible particles, smaller than the smallest observable, which combine, particle with particle, will explain the phenomena; but the assumption of particles bearing this proportion, but not possessing the property of indivisibility, will explain the phenomena at least equally well.” (Whewell, 1858, 2:49-50.) History has since proved Whewell to be exactly right!

Let $T$ be a theory that is simple, unified and predictively accurate. Let $G$ be the statement “God exists, and he manipulates the world such that it appears that $T$ is true but that $T$ is not true.” Then we should not use the predictive success of $G$ to warrant the existence of God (Sober, 1990). On the other hand, it does not follow that we must be entirely agnostic about the differences between $T$ and $G$. It depends on what $T$ says and how well it is supported by the consilience of inductions. If $T$ is very conservative in only asserting the existence of the causes needed to explain consiliences, suitably limited and restricted to the conditions under which the consiliences have been observed, without making further assertions that are unwarranted (such as the indivisibility of particles), then we may be warranted in believing that $T$ is true. In such a situation, we would have reason to believe that the last clause of $G$ is false, and therefore that $G$ is false.

Traditional forms of realism, according to which science aims at the truth or approximate truth of theories, are tied to the exact linguistic content of the theories of science. Real scientific theories tend not to be conservative in what they say. They are imbued by the cultural or religious dogma of their inventors, and they too readily carry the vestiges of past metaphysical traditions. So, any form of realism that ties itself to the linguistic content of actual scientific theories is vulnerable to powerful antirealist arguments, like those found in van Fraassen (1980) or Laudan (1984).

We therefore propose that realists have every reason to break away from these traditional forms of realism, which takes the linguistic content of theories too seriously. In their place, realists should adopt an operational form of realism, which says that the world must be a certain way in order for the theory to operate successfully. Having freed realism from unwanted constraints, we are in danger of leaving realism without anything precise to say. For instance, how is operational realism any different from fictionalism, which also agrees that our theories are successful because the world is some way. Our answer must come in two parts. First, there are cases in which operational realism will agree with fictionalism. It was a convenient fiction to say that the particles we today call atoms are indivisible. The existence of an ether through which light waves propagated was a convenient fiction. However, operational realism does not concede that all scientific entities are convenient fictitious under every possible description. What operational realism recommends is that we provide a guide to the description of reality, or in other words, a guide to the correct interpretation of nature. The new realism is not opposed to linguistic realism in the strong sense of denying that language can ever be used to describe nature.

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17 We owe this point to Elliott Sober, personal communication.
Yet, the question remains: What are these guides for deciphering the world? This is where we propose that Whewell is best understood as an operational realist who sees the consilience of inductions as guiding us towards the “language of truth”.

16. What Does Consilience Really Tell Us?

Almost all Whewellian scholars read Whewell as a very naïve realist when Whewell says of the consilience of inductions:

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 supposition could, after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unseen 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 the truth resides. (Butts, ed., 1989, p. 153)

The quick reading of Whewell has him saying that any theory, such as Newton’s theory of motion, that has proved to be consilient, must be true in its entirety. There is certainly plenty of independent evidence to show that Whewell did think that Newton’s was fully true. In that we agree that Whewell was wrong. However, it would be hasty to conclude that Whewell thinks that any consilient theory must always be true in its entirety. If the atomic hypothesis says that atoms are indivisible, then Whewell can recommend withholding belief from that portion of the theory even if it is otherwise highly consilient.

We recommend the more conservative reading of Whewell. To motivate this interpretation, we need to make two points clear about the context in which this quoted passage appears. The first is that Whewell is addressing a general skepticism he has about the truth of scientific hypotheses, and in an effort to rescue science from complete skepticism he allows that:

...hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error. The object of such inventions is to bind together facts which without them are loose and detached; and if they do this, they may lead the way to a perception of the true rule by which the phenomena are associated together, even if they themselves somewhat misstate the matter. (Whewell, in Butts, ed., 1989, p. 149; Whewell’s emphasis.)

That is, hypotheses may lead to a perception of the true rule if they associate together phenomena that are truly connected, even though the hypotheses are false. For:

...if our scheme has so much of truth in it as to conjoin what is really connected, we may afterwards duly correct or limit the mechanism of this connection. If our hypothesis renders a reason for the agreement of cases really similar, we may afterwards find this reason to be false, but we shall be able to translate it into the language of truth. (Whewell, in Butts, ed., 1989, p. 149.)

Whewell’s idea is that false hypotheses have some truth in them if they associate what is truly connected. The problem is to extract that element of truth. That is the purpose of Whewell’s tests of hypotheses, and in particular it is the function of the consilience of inductions. Therefore, when he talks of the consilience of inductions impressing us with the truth of our hypothesis, he may be referring only to the causal hypothesis that explains the consilience of lower-level inductions. Unlike foundationalists, who require inferences to be based on incorrigible premises, Whewell allows that false hypotheses may thereby lead to a true hypothesis about the truth in the false hypotheses. So, when he says “That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from that being the point where the truth resides,” Whewell is not implying that a consilience must point to a final theory that is true in every detail, but only that it truly points to where the truth resides.

The “language of truth” to which Whewell refers, is the language of causes (Butts, ed., 1989, p. 330):

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. The coincidence of propositions inferred from separate classes of facts, is exactly what we noticed in the Novum Organon Renovatum (b. ii. c. 5, sect.3) as one of the most decisive characteristics of a true theory, under the name of the Consilience of Inductions.

This is Whewell’s principle of common cause.18 In this principle, Whewell sees himself as adopting

18 This is quite different from Reichenbach’s principle of common cause (Reichenbach, 1956), which claims,
Newton’s first rule of philosophizing as his own—that we are not to admit other causes of natural things than such as both are true, and suffice for explaining their phenomena (Ibid., p.325). Our suggestion is that the appropriate way to amend Whewell’s naïve realism would be to say that no other causes be admitted beyond those that are both necessary and sufficient to explain the consilience of inductions.

If Whewell sees theoretical causes as being defined by the consilience of inductions, and the same effects must always point to the same cause, then each causal hypothesis has the same claim to truth. Therefore, two alternative explanations of the same consilience will have an equal claim to truth even if these explanation are couched in quite different vocabularies, arising from distinct scientific traditions. This somewhat surprising viewpoint is exactly what we find in Whewell. In an article published in 1851 called “Of the transformation of hypotheses in the history of science” (Butts, ed., pp.251-262), Whewell puzzles over how it could be rational for Cartesians to tenaciously adhere to their vortex theory of gravitational forces, until finally Fontenelle dies seventy years after the publication of Newton’s Principia. Whewell’s explanation is that the earlier doctrine was gradually brought nearer and nearer to the new theory, so that if, in fact, the transformation had fully succeeded, then the Cartesian theory would have proved to be true. That is, the Cartesians were not automatically doomed to fail in their quest for truth by their entrenchment within a different scientific tradition. Referring to Mill, Whewell says:

This view of the manner in which rival theories pass into one another appears to be so unfamiliar to those who have only slightly attended to the history of science, that I have thought that it might be worth while to illustrate it with a few examples.

It might be said, for instance, by such persons,… “Either it is false that the planets are roughly, that correlated variables, where neither causes the other, must arise as the effects of a common cause variable. It is almost universally acknowledged that Reichenbach’s principle is refuted by some quantum mechanical correlations (Bell, 1964). A noticeable feature of the quantum mechanical correlations is that there are no separate explanations for the occurrence of each variable. There are no separate inductions, and therefore no consilience of inductions, for which Whewell’s principle would posit a common cause. Whewell’s principle is not, therefore, subject to the same counterexample.

Of course, for someone who has just finished a major treatise on logic, Mill (1874, pp. 219-220) is unable to make sense of Whewell’s claim except on the supposition that Whewell is an instrumentalist. We know that he is not. So, how should his view be understood? Our conjecture is that Whewell is an operational realist. As in the case of Earman’s instrumentalists and realists, the truth in the theory is determined by predictive power that it exhibits. If the Cartesian mathematicians had been able to transform their theory so that it matched the predictive accuracy of Newton’s theory, then it would have contained the same truth. The extraneous part of the Cartesian theory, which asserts the existence of vortices emanating from the sun, could have been quietly dropped, just as explanations of gravity were tried and abandoned by the Newtonians themselves. Or the vortex hypothesis could have proved useful, if it had lead to other consiliences, in which case it would have been enthusiastically adopted by the Newtonians as their own. With hindsight wisdom, we can say now this scenario is implausible, yet it was not automatically irrational for the Cartesians to have assumed otherwise.

For Whewell, the search for truth does not consist of testing rival theories within a common deductive framework, deciding which one merits the best evaluation, and then believing the winner. Instead, the search for truth is an ongoing process of inductions, consilience of inductions, unification and simplification, followed by further inductions, consiliences and simplification. Truth in science is not found by inventing and deductively testing rival theories within a static linguistic framework. Rather, the language of science must be carefully developed by colligation, restricted, limited and corrected by the

19 See the opening quote of the previous section. A clear statement of Whewell’s rejection of the argument that it would be “safer and wiser to confine ourselves to the investigation of the laws of phenomena” is found in Butts (ed., 1989) on p. 183, and after.
consilience of inductions, wherein the extraneous and erroneous parts of our language are eliminated by the constant convergence of science towards simplicity and unity. Whewell followed Kant in maintaining that it was to "whereas for them meaning was found with the text, fers from his seventeenth century predecessors; (p.241) points out, Whewell’s use of this analogy dif-

ibly and unity. When the process is complete, and the ideal is reached, are we entitled to say that science is correct interpretation of nature. Somewhat poetically, we might say that Whewell has turned Kuhn on his head. For Whewell, scientific “revolutions” might be better referred to as scientific “reve-
lations,” and the variance of language is not obstacle to scientific realism but the very mechanism by which it operates.

Whewell’s realism has a conditional form; that if the processes of scientific discovery are properly and completely implemented, and the language of science is the true language of nature, then our best theories will be true. Unfortunately, Whewell also appears to have believed that Newtonian mechanics had already reached this ideal, and we happily agree that Whewell was wrong about that. This mistake is unfortu-
nate given that he frequently cites this example, for it makes him look like a naïve realist. But do we there-
fore conclude that Whewell believed that a completely accurate description of theoretical causes must always result from every consilience of inductions? The most cited quote from Whewell in this connection is: “No example can be pointed out, on 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.” (Butts, ed., pp.154-155) But again, there is a subtle ambiguity here. Perhaps all that Whewell means is that there is no consilience that has later to be discovered to be a mere coincidence arising by pure chance. If deciphering the meaning of a consilience is like deciphering the meaning of an ancient text, then the testimony is always true even if the reading is wrong.21 As Fisch (1985, footnote 5, p.241) points out, Whewell’s use of this analogy differs from his seventeenth century predecessors; “whereas for them meaning was found with the text, Whewell followed Kant in maintaining that it was to be superimposed upon it; meaning was read into the text, rather than out of it.”

A less favorable interpretation would be that Whewell is not allowing that there have every been any mistakes in the description of theoretical causes. Does Whewell really believe that there can be no corrections or limitations to the description of causes arising from the reiteration of inductive processes to still higher levels of generality? Remember that, in section 9, we quoted Whewell as describing the consilience of inductions as “the testimony of two wit-
tesses in behalf of the hypothesis;” and then con-
cluding that “in proportion as these two witnesses are separate and independent, the conviction produced by their agreement is more and more complete.” “More and more complete” does not mean “com-
plete.”

Furthermore, Whewell complains that this is one feature in the construction of science to which Mill does not ascribe “its due importance”; namely to “that process by which we not only ascend from particular facts to a general law, but when this is done, ascend from the first general law to others more general; and so on proceeding to the highest point of generalization.” (Butts, ed., pp.297-298) It may well be that Whewell is unduly optimistic about when this highest point is reached, but it is nevertheless clear that Whewell allows that at each ascent, the general proposition may reach back deductively to refine and modify the description of the facts at the lower level (section 13).

As to whether Whewell sees this process as ever ending, remember that he complains about Mill’s “hope of the efficacy of Deduction, rather than Induction, in promoting the progress of Science; which hope, so far as the physical sciences are concerned, appears...at variance with all the lessons of the his-
tory of those sciences.” Whewell takes the different view, where by “looking at the state of physical sci-
ence, we see that there are still a vast mass of cases, in which we do not at all know the causes, at least in

their full generality; and that the knowledge of new causes, and the generalization of laws already known, can only be obtained by new inductive discoveries.” (Butts, ed., p. 301) Surely, this allows that future inductive discoveries may correct the very best theories that science has to offer at the present time.

The dispute between Whewell and Mill began with a question about whether conceptual innovation is essential to scientific induction. It has now expanded into a dispute about the representational role of language in science. Mill has a rather impoverished view of language, whereby it merely serves as a fixed receptacle for deduction, while Whewell adopts a dynamical view of scientific realism in

20 See Glymour (1980) for some interesting examples, which fit nicely with Whewell’s description of the process in our opinion.

21 What nature says is always true ( unlike ancient texts). The issue is whether the message can be read at all, or whether it is correctly read.
which conceptual change is an essential element. The dispute between them is far more than a terminological debate about whether the colligation of facts should be called induction. The debate has reached into the very heart of contemporary philosophy of science; into the nature of scientific progress, into the evaluation of theories, and into the methods that make the achievements of science possible.

Therefore, we have come full circle. We began with a disagreement between Mill and Whewell over the form of science, and we have arrived at their disagreement over the matter of science. As Whewell might say, form and matter are always inseparable elements of the same object. So too in this instance. For Whewell the matter of science is the discovery of true causes, and the form is the introduction of the concepts by the mind, which through successive stages of refinement, testing, and correction, will come to represent the true causes. Mill has the more modern point of view. We leave it to the reader to mull over the pros and cons on each side.

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