

Mill's and Whewell's Competing Visions of Logic*

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While John Stuart Mill was, he tells us, revising his draft of the *System of Logic*

Dr Whewell's *Philosophy of the Inductive Sciences* made its appearance; a circumstance fortunate for me, as it gave me what I greatly desired, a full treatment of the subject by an antagonist, and enabled me to present my ideas with greater clearness and emphasis as well as fuller and more varied development, in defending them against definite objections, and confronting them distinctly with an opposite theory.

What hopes I had of exciting any immediate attention, were mainly grounded on the polemical propensities of Dr Whewell; who, I thought, from observation of his conduct in other cases, would probably do something to bring the book into notice, by replying, and that promptly, to the attack on his opinions.¹

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¹I:231/A 7:3–4. References to Mill's writing by volume and page in the standard edition of Mill's works (1967–1989); the *Autobiography* and the *System of Logic* will be given slashed cites, with *A* chapter:paragraph and book:chapter:section, respectively, following the uniform citation. A biographer of the period endorses Mill's expectations: "Dr Whewell was always unwisely prompt in noticing the criticisms of reviewers" (Todhunter, 1876, vol. i, p. 72).

William Whewell did take Mill's objections seriously, and did reply, although not as rapidly as Mill had hoped, subsequently revising his *Philosophy of the Inductive Sciences* and reissuing it as three differently titled volumes; when he did so, one of these, the *Philosophy of Discovery*, contained a chapter rebutting Mill's objections.²

Because both Whewell and Mill ended up prominent figures in the intellectual world of their day, the back and forth was noticed. As the "Mill-Whewell debate" is remembered today, it was a disagreement between two philosophers of science about both scientific method and the correct analysis of induction. John Stuart Mill, we are told, thought that the scientific method was built around what *we* now call 'induction'—one observes, say, that a number of crows are black, and infers that the next one will also be black—although Mill fortified that basic inference pattern with the "Four Methods" that are even at the present time taught in critical thinking classes, as well as with a number of further methods, specific to particular types of sciences, that are less widely known. Whewell, for his part, is thought of as perhaps anticipating views we associate today with Philip Kitcher, on which scientists adopt theories that unify results across theoretical domains, or perhaps as advancing an ancestor of abduction and inference to the best explanation.³

If the Mill-Whewell debate really had this shape and content, it would be of no more than historical interest. The positions that are attributed to the opponents are familiar, and we do not need archaic Victorian-era formulations to assess their merits. And in general, while a great deal of energy is expended by historians of philosophy over whether the label for a well-understood contemporary position sticks to a dead philosopher—for instance, on whether Hume was an internalist or Aristotle a metaphysical realist—if the dead figure's view really was the well-understood position, we do not need him. Dead philosophers are worth figuring out when their views are *unfamiliar*; it is especially in those cases that we are likely to learn something from them.

²Whewell, 1847; Whewell, 1860, ch. 22, and see also pp. 338–340, 349f, and pp. 470f, which directly respond to arguments in the *System of Logic*; I'll cite the *Philosophy of the Inductive Sciences* as PI, the *Philosophy of Discovery* as PD, the *Novum Organon Renovatum* (1858) as NO, and the *History of the Inductive Sciences* (1857) as HI (for multivolume works, by volume and page).

³For the Four Methods, see VII:388–406/III:viii, and for this sort of appropriation of Mill, Skyrms, 1975, pp. 89ff. For such a consilience-oriented view, see Kitcher, 1981; for instances of this way of reading Whewell, see Forster, 1988, Thagard, 1977b, pp. 15, 17. The resemblance to abduction is not simply coincidence; as Fisch, 1991, pp. 12, 106f, 109f, 131, reminds us, Peirce read and approved of (but did not sufficiently credit) Whewell.

But both the issues at stake in the disagreement between Mill and Whewell, and the positions they took, have been erroneously located in philosophy of science, or so I will suggest here. Much of the common ground between the two of them was indeed the insistence that we must look to the history of science to determine what it is to reason correctly about matters of fact. But in the psychologistic way of thinking that was taken for granted until Frege and Husserl, the study of what it is to reason correctly is logic.⁴ So the dispute is properly characterized as belonging to philosophy of logic, and both the positions and the arguments will be surprising to most turn-of-the-millennium philosophers.

I will first recap Mill's view of induction, sketch the main elements of Whewell's view, and remark on a few parallels between the positions. Then I will turn to the protagonists' respective modes of argumentation. Mill was committed to arguing for his inductivism inductively; Whewell was analogously committed to giving an argument for his notion of induction that conformed to his views about what it was, and I will describe how he attempted to do that. I will turn to the question of why it is that, almost always, Whewell and Mill strike readers as talking past one another. I will argue that this is a misconception, and explain how it was that each of them did their best to provide argumentation that the other party to the dispute would be forced to accept on his own terms.

1

Mill's views in logic and the philosophy of science are in one way a little like his moral and political opinions: it is too easy on first or even second reading to assimilate them to what are mainstream notions today, and so fail to notice his more startling claims. Both to preempt this, and to prepare the way for comparison with Whewell, I'm going to take a slightly unusual route into Mill's inductivism. The so-called New Riddle of Induction, it turns out, was not all that new—although in the middle of the last century Nelson Goodman managed to convey its force more vividly than perhaps ever before. Mill was quite aware that induction doesn't treat all predicates alike, and his understanding of the problem was more nuanced than Goodman's. It is not just that some predicates (like 'green') are treated as legitimately

⁴For Mill's psychologistic rendering of the subject matter of logic, see VII:4–12/Introduction:4–7; at VII:202/II:iii:7, he calls induction “that great mental operation,” at VII:206/II:iii:9, describes logic as “the entire theory of the ascertainment of reasoned or inferred truth” and at VIII:735/V:i:2, he describes his subject matter as “the philosophy of reasoning.”

mediating conclusions drawn on the basis of empirical evidence, and others (like ‘grue’) are not: we treat the predicates we *do* take to be legitimate very differently from one another. (If the New Riddle is new to you, I’ve put a recap in this footnote.⁵)

When a chemist announces the existence and properties of a

⁵Suppose you aren’t bothered by David Hume’s Problem of Induction: you are willing to allow that the future resembles the past. Goodman, 1979, noticed that we haven’t made sense of inductive inference until we have a way of saying *which way* it’s reasonable to expect the future to resemble the past, and I’ll borrow his very famous illustration. Consider a simple induction-by-enumeration:

1. The grass was green this morning.
2. The grass was green yesterday morning.
3. The grass was green in the morning the day before yesterday.
4. The course of nature is uniform. (I.e., we’re not going to be detained by Hume’s objections.)
5. So, tomorrow morning, the grass will be green.

This seems to be more or less in order, leaving to one side worries about whether I’m going to remember to water the lawn. But now, let’s define a new color predicate:

x is *grue* iff x is observed today or earlier, and is green, or x is observed tomorrow or later, and is blue.

That puts us in a position to assemble another simple induction-by-enumeration:

1. The grass was grue this morning.
2. The grass was grue yesterday morning.
3. The grass was grue in the morning the day before yesterday.
4. The course of nature is uniform.
5. So, tomorrow morning, the grass will be grue.
6. If the grass is grue tomorrow morning, it will be blue. (by definition of “grue”)
7. Tomorrow morning, the grass will be blue.

Obviously, our first induction, if not all that strong, was anyway sane, while the second, very similar-looking induction was crazy; and clearly, not both of them can predict the color of the grass tomorrow. Goodman’s problem is to say why.

Since Scarre, 2002, it has been the received wisdom that early nineteenth-century philosophers were unaware of Hume’s skepticism about induction, and thus Mill could not have been responding to it. Whewell’s works remind us that this is very unlikely: gestures at Humean skepticism are frequent, and terse enough to presuppose a widely shared understanding of what it amounted to (e.g., PI 75, PD 222, 333, or HI I:193, where Algezeli’s “deni[al of] the possibility of a known connexion between cause and effect” is called “a prelude...to the celebrated argumentation of Hume”). And we know that Mill read Whewell closely. For a reconstruction of Mill’s solution to Hume’s problem of induction, see Millgram, 2009.

newly-discovered substance, if we confide in his accuracy, we feel assured that the conclusions he has arrived at will hold universally, though the induction be founded but on a single instance. We do not withhold our assent, waiting for a repetition of the experiment. . . Now mark another case, and contrast it with this. Not all the instances which have been observed since the beginning of the world, in support of the general proposition that all crows are black, would be deemed a sufficient presumption of the truth of the proposition, to outweigh the testimony of one unexceptionable witness who should affirm that in some region of the earth not fully explored, he had caught and examined a crow, and had found it to be grey.⁶

We correctly induce more confidently on some predicates than on others. At the limit, we (correctly!) refuse to proceed with inductive inferences driven by certain predicates. The puzzle then is: what makes similarities better and worse bases for induction? And Mill tells us,

Whoever can answer this question knows more of the philosophy of logic than the wisest of the ancients, and has solved the problem of induction. (*ibid.*)

Mill's solution to the problem is very much in the spirit of Goodman's later attempt on it: some types of induction have a better track record than others.⁷ We start out our collective history with the simplest, crudest inductions-by-enumeration: we have observed a great many crows, and found them all to be black; we conclude that the next one will be black as well. Over the course of time, we experiment with more careful and complicated variations on the technique, and, again over time, we come to see (and this is itself an inductive inference) which work best, and which work in which domains. Mill's various Methods—the famous Four Methods, and the further Methods which, over the course of the *System of Logic*, he spells out and argues are suitable for different sorts of sciences—are metainductively warranted *refinements* of those initial and so very crude inductions.⁸

⁶VII:314/III:iii:3; Holland *et al.*, 1986, pp. 241–243, documents that as a matter of psychological fact people do discriminate as Mill says. Mill gives an explanation of the contrast at hand at VII:585f/III:xxii:7.

⁷VII:318–319/III:iv:2. Goodman claimed that the predicates which we correctly allow to drive inductions are better “entrenched”. However, he was never able to spell out satisfactorily what counted as entrenchment; see Zabludowski, 1994, and its sequels.

⁸See VII:188/II:iii:3; VII:567f/III:xxi:2, where Mill concludes “that induction *per enumerationem simplicem*. . . is in reality the only kind of induction possible; since the more

His treatment is only partial, but is ambitious enough to make him, following on that “knows more... than the wisest of the ancients,” no doubt inadvertently immodest.

Two of these refinements deserve special notice. First, Mill holds that all inductive inference proceeds from particulars to particulars: I observe a great many black crows and properly speaking infer *not* the general claim that all crows are black, but that various particular crows which I have yet to observe will prove to be black. The general claim, Mill argues, is not part of any inference, but rather a mnemonic device: a reminder of the evidence I have seen, and a way of encoding it that allows me to keep track of which conclusions about particulars that evidence warrants. Since it is not part of any inference, a syllogism constructed using such a general claim as a major premise cannot count as inference, and as deductive inference was in Mill’s day understood to be captured by syllogisms, Mill was arguing that there is no such thing as deductive inference at all: what appears to us to be syllogistic deduction is in fact a refinement (albeit an important one) of *inductive* technique; not argument, but a way of monitoring and controlling the progress of our inductive arguments. In Mill’s view, *all* inference about matters of fact is inductive.⁹

Second, the more mature sciences can be systematized so thoroughly that results in the science can be derived from a small set of axioms. Like a syllogism’s major premise, the axioms are compressed encodings of the evidence base for the science; even the axioms of apparently apriori sciences such as arithmetic and geometry are, Mill claims, merely encodings of observational evidence and the warrant it provides. When Mill calls a

elaborate process depends for its validity on a law, itself obtained in that inartificial mode”; and compare the remarks made in early editions of the *Logic*, to the effect that “all that is requisite to support the Canons of Induction is, that the generalization which gives the Law of Universal Causation should be a stronger and better induction” (VII:572n/III:xxi:4). At VIII:833/VI:i:1 we are told that “we should never have known by what process truth is to be ascertained, if we had not previously ascertained many truths. . . In scientific investigation. . . the way of obtaining the end is seen as it were instinctively by superior minds in some comparatively simple case, and is then, by judicious generalization, adapted to the variety of complex cases.” At VII:312/III:iii:2, Mill describes induction by enumeration as “the induction of the ancients,” and he attributes the shortcomings in previous theories of induction to “want of sufficient acquaintance with the processes by which science has actually succeeded in establishing general truths.” That is, to arrive at “practical rules, which might be for induction itself what the rules of the syllogism are for the interpretation of induction,” more attention needs to be paid to the track record (VII:283/III:i:1).

⁹VII:183ff/II:iii; VII:159/II:i.2. One can give top-down deductions about what to do, as when a judge applies a law, but even here Mill regards this as interpretation rather than inference proper (VII:193f/II:iii:4; cf. VIII:944/IV:xii:2).

science “Deductive,” we must not be confused; systematized sciences, including “Deductive” sciences, are further and dramatic refinements—once again, warranted by our experience with them over the history of inquiry—of our inductive reasoning.

If logic is, as Mill and his contemporaries understood it, the study of inference, and if there is no deduction as we understand it, but only inductive inference, then a theory of induction *is* a full-featured theory of logic—and that conclusion is borne out in the title, *A System of Logic*, which Mill chose for his *magnum opus*. Mill understood himself to be, first and foremost, a philosopher of logic.

2

Whewell tells us that

Induction is a term applied to describe the *process* of a true Colligation of Facts by means of an exact and appropriate Conception.

An Induction is not the mere *sum* of the Facts which are colligated. The Facts are not only brought together, but seen in a new point of view. A new mental Element is *superinduced*. . . once this is effectually done, the novelty of the conception is overlooked, and the conception is considered as part of the fact. (NO 70f, italics deleted)

Whewell’s central concept, “Colligation,” is not one of our words, so I will walk through the central elements of Whewell’s view in a slightly anachronizing vocabulary. (As I introduce Whewell’s own vocabulary, I will mark it by reproducing his capitalization, but only on the initial use: the modern reader’s patience with the orthography of the time is quickly exhausted.) At any given stage in the development of science, we will find ourselves with a repertoire of Ideas, in terms of which our observations are couched; we will also find ourselves with a collection of Facts. For instance, we observe logs (thus deploying ideas such as that of a physical object); we notice that when you divide a log in two parts, each part is hard to lift, but not as hard to lift as the both of them together; we notice that when you burn a log, the ashes are easier to lift than the log was; we introduce devices such as scales to systematize such observations, and over the course of time perhaps

collect a great many of them.¹⁰

At the ensuing stage (the Epoch of the emerging science), an especially insightful scientist might introduce a new idea, *Weight*. It has proved possible, remarkably often, to spell out the content of an idea formally or semiformally: in the development of geometry, the novel idea is that of Space, whose content is unpacked by Euclid’s axiomatization; analogously, the content of the idea of weight will be given by statements like: any physical object has a weight; weights can be treated arithmetically; weight is conserved. This process of spelling out the content of an idea is the move to a *Conception*. (As someone might say when making this move: we all have an intuitive idea of weight, but now let me explain my conception of it.¹¹) At this point a new science—perhaps called “Barology”¹²—will crystallize around the idea. In this science, the conception of weight will allow one to derive the many and varied lower level facts about scales, lifting logs and so on.¹³

¹⁰I am piecing together a Whewell-like example—as opposed to one of Whewell’s own—in order to avoid both an over-discussed but atypical case, Kepler’s Laws, and some of Whewell’s well-worked out but overly lengthy illustrations. Components of the example I am assembling can be found at PD, Appendix K, HI I:71–72, and Forster, 2011, sec. 5.

¹¹I’m grateful to Richard Healey for this way of putting it; HI II:117 gives a different illustration of how the content of an idea can be unpacked, in this case, that of Universal Gravitation.

¹²Mill’s English rendering of Auguste Comte’s preferred name for it (X:283).

¹³Buchdahl, 1991, takes this to allow him to classify Whewell as a ‘deductivist’ about scientific justification, but this is a mistake evidently arising from exclusive attention to Whewell’s treatments of the physical sciences, in which (as Whewell says of the theory of machine construction) “the determination of the results and conditions of any combination of materials and movements becomes really a mathematical deduction from known principles” (HI II:440). Perhaps this is understandable; Whewell had after all authored the *Mechanical Euclid*. However, when discussing the life sciences and mineralogy (that is, sciences in which natural kinds are picked out by similarity), Whewell points out that their definitions needn’t be extensionally correct: because there are, say, plants that fail to satisfy the correct botanical definition of “rose,” but are roses nonetheless (NO 175; compare HI III:269–271). In a passage that Mill takes the trouble to quote at length, after reminding us that “in the family of the rose-tree, we are told that the *ovules* are *very rarely* erect, the *stigmata* *usually* simple,” Whewell continues, “particulars which are included in a class, though they transgress the definition of it. . . are so contrary to many of the received opinions respecting the use of definitions, and the nature of scientific propositions, that they will probably appear to many persons highly illogical and unphilosophical. But a disposition to such a judgment arises in a great measure from this, that the mathematical and mathematico-physical sciences have, in a great degree, determined men’s views of the general nature and form of scientific truth” (VIII:717f/IV:vii:4). When “the type [of the Rose family] must be connected by affinities with most of the others of its group. . . near the center of the crowd, and not one of the stragglers,” among the facts colligated by the idea, rose, some will not follow deductively from one’s worked out conception. Thus not every such unpacking takes the form of an axiomatization. (Mill, for his part, complains

The test for correctness of an idea is Consilience, that is, the ability to draw together phenomena and theoretical treatments that had seemed qualitatively different enough to belong to distinct subject matters. In another much-discussed illustration, Newton’s theory of universal gravitation drew together the heliocentric theory of Copernicus, the fact that the “Satellites of Jupiter and Saturn revolve according to Kepler’s Laws,” the tides being “produced by attraction of Moon and Sun on Sea,” the “Fall of heavy bodies” and much else.¹⁴ Whewell is quite confident that a theoretical advance whose organizing idea makes possible the consilience of a wide range of different classes of fact will not need to be walked back, and he believes his confidence to be supported by the history of science in its entirety.¹⁵ Although consilience has been the focus of discussion of Whewell’s philosophy of science over the last century, it is important to remember that, for him, it is invoked to spell out—to provide a conception of—the idea he took to be central, namely, colligation. If we focus so exclusively on a secondary or subsidiary concept that we lose track of the idea it is there to elucidate, we will fail to understand what Whewell thought he was doing.¹⁶

that this way of doing things “is inconsistent with Dr. Whewell’s own statement of the fundamental principle of classification, namely, that ‘general assertions shall be possible’” (VIII:721/IV:vii:4).)

¹⁴From the “Inductive Table of Astronomy” in PI II, facing 118; see further discussion of Whewell’s Tables, below.

¹⁵Oddly, since he describes nature’s horror of a vacuum as such a consilience, since rejected (HI II:51). His confidence is both less and more naive than it seems. On the one hand, once we have come to organize an array of facts via the newly-adopted idea, when we encounter an apparent exception to the generalizations the idea seems to warrant, instead of treating it as a counterexample, we will insist that there must be a “disturbing cause”. (E.g., when we encounter an apparent violation of Kepler’s Laws, “in the case of Uranus, a new planet” [PD 453].) That is, Whewell has very much the reasons for confidence in the stability of a consilience-driven induction that Kuhn had for stability of his paradigms.

On the other hand, although the ideas that sciences crystallize around demand the utmost in inventiveness and intellectual acuity, and may take arbitrarily long to enter the scientific vocabulary, we are preequipped to come up with just *these* ideas (as we will shortly see, in something like the way that, for Kant, we already come with just *these* categories of the understanding), and God has arranged the world so that just these ideas will allow us to understand, albeit only ever partially, its real structure: “the human mind can and does put forth, out of its natural stores, duly unfolded, certain Ideas as the bases of scientific truths: These Ideas are universally and constantly verified in the universe: And the reason of this is, that they agree with the Ideas of the Divine Mind according to which the universe is constituted and sustained” (PD 374).

¹⁶Colligation, importantly, is not restricted to the sciences; Snyder, 2011, p. 255, following Todhunter, 1876, vol. i, p. 44, recaps an example from architecture; compare HI I:261–263.

The novel idea, if successfully introduced and spelled out, will induce something like an aspect shift: as the idea is clarified, it gradually becomes obvious that the weight of the smoke *must* be the weight of the logs minus the weight of the ashes.¹⁷ “We do not see [Ideas], we see *through them*” (PI I:40). The hard-won empirical facts, in this case, about which physical objects tip the scales which way, about how hard it feels to lift them and so on turn out, in retrospect, to have a large *necessary* and *a priori* component, in this case, the arithmetic manipulability of the values we use the scales to assign (PD 330): “in the progress of that exact speculative knowledge which we call Science, Facts which were at a previous period merely Observed Facts, come to be known as Necessary Truths” (PD 302). The aspect shift induced by colligation (in this case, of the lower-level facts by the idea of weight) makes what was formerly an exotic Theory, advanced in order to accommodate evidence, into a fact in its own turn, which can now be drawn upon in the search for the next scientific advance.

Whewell more than acknowledged his debt to Kant, and we should see colligation as an historicized adaptation of the Kantian notion of synthesis—not quite as we are taught it today, but as it was received in the nineteenth century—to the philosophy of science.¹⁸ Like Kantian cognitions, which are synthesized into larger cognitions via the addition of a further conceptual element provided by the cognizer, facts are colligated, through the addition of a further idea, into larger facts. (Conversely, just as, in Kant, anything we are aware of is *already* a product of synthesis, *every* fact is Janus-faced—it contains subordinate facts colligated by an appropriate idea—all the way down.) The history of science is the history of progressive colligations; un-

¹⁷PD 344, 472f; remarkably, given the date, this has the sound of pre-Lavoisier chemistry. But Whewell has this much right: these sorts of conservation laws seem obvious in retrospect but not in prospect. Aristotle thought that when you dissolve a drop of wine in a bucket of water, the wine is just *gone*; we think that it *can't* be gone. (*De Generatione et Corruptione* 328a25ff; cf. 322a10f, 33f; however, Ariane Shemmer has objected that this is not so far removed from our current ways of thinking; after all, when you put a little bit of hot water into a lot of cold water, the hot water is not thought to persist, albeit diluted by the cold water—rather, it *turns* cold.)

¹⁸For instance, Whewell allows himself the hyperbole of describing parts of *The Philosophy of the Inductive Sciences* as “almost literal translations of chapters in the *Kritik der Reinen Vernunft*” (PD 335, *sic*); Todhunter, 1876, vol. i, p. 345, mentions Whewell’s reading notes on the first *Critique*. Fisch, 1991, tells the plausible and fascinating story of how Whewell arrived at the Kantian view, not in the first place by reading Kant, but by wrestling with the problems of authoring mathematics and physics textbooks.

When a view is described as an historically-oriented idealism, it is natural to ask whether the guiding light was perhaps not Kant but Hegel. This is unlikely: Whewell did not think much of Hegel at all (PD, Appendix H; Todhunter, 1876, vol. i, p. 207).

like Kantian syntheses, the last two-and-a-half thousand years of these are recorded episodes in our collective intellectual history, and they are *achievements*, each one turning on the introduction of an idea whose content is deeply novel relative to the facts it assimilates; like Kantian syntheses, colligations, or more narrowly, the ideas introduced in the course of colligations, are the source of emergent necessity and aprioricity.

How is that last supposed to be possible? Whewell was quite scholarly about ancient philosophy, and his writings contain Aristotelian moments as well, one of which can serve as a partial explanation. Facts, Whewell suggests, are to ideas as matter is to form (NO 72). Now, when you eat your bread, tomatoes and olive oil, Aristotle thinks, they become specifically human flesh, and when your form relaxes its grip in death, your flesh becomes mere meat: in living beings, matter is dependent on form. Analogously, in colligation, when facts are brought within the ambit of an idea, it metabolizes them. Once the idea of an ellipse is introduced by Kepler, you can no longer see the raw astronomical data as was: you see the points, willy nilly, *as points on an ellipse*. In our toy example of a moment ago, when a log is hard to pick up, you take yourself to be *registering its weight*. More primitively, your sensations are experienced through the lens of the ideas of space, time, substance and so on: so you are no longer able to experience your visual qualia as unlocated, as not yet within a temporal order, as free-floating sense-data rather than, say, the colors of objects. The aspect shift induced by a successful colligation “lead[s] us to regard the views we reject as not only false, but inconceivable” (NO 32f).

The Mill-Whewell debate is often read as a disagreement over what induction is—that is, over what the *word* means—and thus as merely terminological.¹⁹ I am in the course of suggesting that it is a great deal more interesting than that, but to sidestep the distraction, let’s use “induction” for what Mill means by it (just because it is pretty much our own usage), and “colligation” for the intellectual activity that Whewell describes.

¹⁹E.g., “it would have been more convenient if each of the writers had invented a word for himself, and used it in his own sense, instead of disputing as to the proper application of a word involving so much controversy” (Todhunter, 1876, vol. i, p. 231). Whewell himself considers whether the disagreement “is a question merely of words,” and replies that “such questions of definition are never questions of definition merely” (PD 243). Mill agrees: “it is impossible to name [objects] properly except in proportion as we are already acquainted with their nature and properties” (VII:176/II:ii:3); “to determine... what should be, the meaning of a name... requires for its solution that we should enter, and sometimes enter very deeply, into the properties... of the things named” (VII:150/I:viii:7; cf. VIII:672/IV:iv:4).

Perhaps because “reasoning” was at the time so closely tied to deductive inference, Whewell does not begin by announcing himself as a philosopher of logic.²⁰ But that is evidently what he is: he is explaining how inference is correctly conducted on the most challenging and most admired playing field, that of novel and dramatically successful scientific theorizing, which makes him a logician by Mill’s lights. Whewell reserves the term “Logic” for “a system which teaches us so to arrange our reasonings that their truth or falsehood shall be evident in their form,” and once he has explained what he takes induction to be, he goes on to treat of the “Logic of Induction,” understood as analogous to the “Logic of Deduction,” i.e., the syllogistic (NO 105f and Book II, ch. 6, *passim*). So he is a logician by his own lights as well.²¹

3

It is quite normal, when reading Mill’s and Whewell’s responses to one another, to have the sense that they are too far apart to be having a successful conversation. Both to see what is producing that effect, and to help us see past it, let’s do a little compare and contrast.

Both Mill and Whewell are committed to allowing science to ground their views about logic. In Mill’s case, the reason is clear: all inference about matters of fact is inductive, and so if logic is a science, the arguments it appeals to must be inductions from, especially, past cases of successful inference about matters of fact—and what better showcase of past successes could there be than science? (If logic is an ‘art’, Mill thinks, that is, a collection of practical dicta, those dicta must be underwritten by a corresponding science.) We will presently explain why Whewell thought he had to appeal to the history and practice of science, but for the moment it is quite clear that he did: while working on the *Philosophy of the Inductive Sciences*, he was simultaneously paying his dues by writing up his three-volume *History of the Inductive Sciences*, which he described as “a survey of the present state of knowledge, in order to learn from it the best method of philoso-

²⁰Rather than contest the connection, as Mill does, Whewell at one point agrees that “Induction is inconclusive *as reasoning*. It is not reasoning. . . an inductive truth. . . is not *demonstrated*” (PD 454); and he agrees as well that “there is no formula for the discovery of inductive truth” (PD 456).

²¹He once wrote to De Morgan: “I do not wonder at your denying these devices [my *Inductive Tables*] a place in Logic; and you will think me heretical and profane, if I say, *so much the worse for Logic*” (Todhunter, 1876, vol. ii, p. 417, letter of Jan. 18, 1859; I was directed to this passage by Fisch, 1991, p. 199n16).

phizing, and the right view of philosophy,” and of which he insisted that “it [wa]s essential in order to give anything like consistency to [his] views of the method of philosophising.”²²

Neither Whewell nor Mill exhibit much philosophical interest in the workings of deduction. As I have already mentioned, Mill argues that there is no such thing as deductive (properly so-called) inference, and accordingly contents himself with a standard review of the forms of the syllogism.²³ Whewell thinks that, often enough, the content of an idea is spelled out deductively, and allows the deductive reconstruction of the facts it colligates; but he does not seem to think that deductive logic needs its own treatment within the frame of his discussion, which is confined by stipulation to the inductive sciences (HI I:15). I am not entirely sure that this is a principled decision: because settled sciences *become* necessary, the necessity and apriority of deductive logic do not distinguish it from other sciences which Whewell takes to be within the scope of his treatment (and indeed, geometry and arithmetic are both presented as colligations of empirical observations, having to do with land surveying and counting respectively, by the ideas of space and number). I would very much have liked to see Whewell’s colligationist analysis of deduction.

However, deduction is in the background as a model; both Mill and Whewell aspire to present their logic as formally as possible, which in historical context meant, as closely resembling traditional presentations of the syllogistic as they could manage. In Mill, this aspiration accounts for the way he lays out his Four Methods, making them appear much more mechanically applicable than they in fact are. In Whewell, the analogous presentation is found in his “Inductive Tables” of colligations. (These appear in his books as folded inserts, and are well worth a close look.) In these “Tabular arrangements” (NO 115), we see facts at one stage of a science collected together above the theory produced by colligating them, which is in turn collected together, along with others belonging to the same stage, above the theory produced by colligating *them*.²⁴ Whewell thinks of the

²²Todhunter, 1876, vol. ii, pp. 193, 248, letters to Herschel and to Jones, Dec. 4, 1836 and Oct. 6, 1834, respectively; I was alerted to these quotations by Fisch, 1991, pp. 111, 139. See esp. at HI I:viii his insistence that his history is “a basis for the Philosophy of Science. It seemed to me that our study of the modes of discovering truth ought to be based upon a survey of the truths which have been discovered,” and compare his concluding sentence at HI I:338.

²³VII:164ff/II:ii; remarkably, Whewell even seems willing to endorse Mill’s “doctrine, that the force of the syllogism consists in an inductive assertion, with an interpretation added to it” (PD 289, emphasis deleted).

²⁴The terms “theory” and “fact” turn out to be relational: “as the same person is a

Inductive Brackets that mark these collections as logical notation, and says of the graphical presentation that although a table of colligations can be as difficult to work out as a syllogistic presentation of a deductive train of thought, once produced, “it supplies the means of ascertaining the truth of our *inductive* inferences, so far as the *form* in which our reasoning may be stated can afford such a criterion” (NO 106), and is “the Criterion of Inductive Truth, in the same sense in which Syllogistic Demonstration is the Criterion of Necessary Truth” (NO 115).

Beyond this point, the disagreements commence. Whewell thinks that the heart of scientific inference is the invention of the colligating idea; it is overstating his view just slightly to say that once the right idea has been hit upon, the remaining intellectual activity is just mopping up. Mill holds that the content of any claim can be reduced to sense-impressions; thus there can be no irreducible, genuinely novel ideas of the sort that Whewell takes to induce colligations (VII:106f/I:v:7; VII:296/III:ii:4; cf. VII:485/III:xiv:2). Mill allows that inductions may require newly invented concepts or descriptions, of the tamer sort which he allows, but insists that generating them is an operation “subsidiary” to the reasoning proper (VIII:647/IV:i:4). Mill holds that because colligations *collect* already available facts, they are mere summaries or compendia of them, and so not inference at all. He famously compares colligating one’s data to filling in a map on the basis of a sea voyage; when, staring at the completed map, you suddenly realize that you have sailed around an island, notwithstanding the novel concept, you have not actually inferred a new conclusion at all, but merely recapitulated what you already knew (VII:292/III:ii:3).

Whewell takes the task of philosophy of science to include accounting for the *necessity* of scientific truths. Mill was a loyal empiricist, who understood Hume to have shown that necessity takes what we today would call a noncognitivist analysis; that something is necessary is never a *fact*, but at most, an observer’s feeling.²⁵ He had in addition been impressed by Auguste Comte’s doctrine of the three stages of a science: “Speculation he conceives to have . . . three successive stages; in the first of which it tends to explain the phenomena by supernatural agencies, in the second by metaphysical abstractions, and in the third or final state confines itself to ascertaining their laws of succession and similitude (VIII:928/VI:x:8; see X:267–279). But the metaphysical abstractions, such as necessities or forces, are just the super-

father and a son[.] Propositions are Facts and Theories, according as they stand above or below the Inductive Brackets of our Tables” (NO 116).

²⁵He reclaims the word “necessary” for an acceptably low-key use at VII:339/III:v:6.

natural agents with their dramatic personalities effaced, and consequently Mill regarded the alleged necessities for which Whewell was trying to account as superstitious atavisms—“this character of necessity,” he tells us, “is an illusion”—and he dismissed them as side-effects of habituation.²⁶

When we look over the the list of disagreements, we will not be surprised at the impression that their readers have often had, that the parties to the debate were unable to engage one another philosophically. I can testify to the disconnect myself: I read Mill before I discovered Whewell, and came away from the *System* understanding colligations to be something like lists; so little of the Kantian picture in the background was conveyed, that when I did get around to reading Whewell, it was natural to assume that Mill must not have understood him. Moreover, an argument can only persuade one’s opponent if he accepts its premises; Mill and Whewell don’t share enough common ground for the disagreement to be productive—or so it seems.

4

In fact, the disagreement goes deeper even than that. Mill and Whewell differ not only on background views to which their arguments might appeal, but on what would *count as an argument*. And this is on display especially in Whewell’s most mature presentation of his own position.

While there are no doubt various inference patterns at work in the sciences, the important results are arrived at by colligation. To establish this would be an important result in the philosophy of science; philosophy of science is itself a scientific endeavor; we should attempt to arrive at the result by colligation. To do so, Whewell first must identify ranges of facts that are candidates for colligation, and then select the “appropriate” idea (HI I:64)—the one that will effectively colligate them. Colligations are effective when they exhibit consilience; a consilience involves “an hypothesis which, assumed in order to explain one class of phenomena, has been found also to account exactly for another” (HI II:429); so the relevant ranges of facts must be drawn from what before the colligation seem to be different subject matters. The history of science—construed broadly, to include not just the usual suspects, but such varied sciences as botany, crystallography, and “those Sciences which contemplate the universe, the earth, and its inhabitants, with reference to their historical changes and the causes of those changes” (HI II:229)—provides a good deal of the required variation; observations by empiricist and Kantian philosophers provide others. (For

²⁶VII:225/II:v:1, and cf. the manuscript remarks at VII:571/III:xxi:3.

instance, that on the one hand, science is responsible throughout to empirical observation, but, on the other, that the deepest of its results have the look and feel of necessary truth—a pair of claims that look to amount to an antinomy.) Remarkably, the appropriate idea proves to be colligation itself, and it is to be spelled out, that is, converted into a conception, via the very discussion we have so tersely recapitulated.

Once the conception of the idea of colligation is on hand, if the colligation deploying that conception is successful, it will induce an aspect shift. If the history did indeed corroborate the colligationist account, it will have documented episodes in the development of science that proceeded in accord with Whewell’s template; the empirical historical data will have been, as far as the investigators who collected it were concerned, contingent. However, when the aspect shift kicks in, it will become clear in retrospect that progress in science *must* happen via colligation; the historical episodes will be inevitably redescribed in the vocabulary of the conception, and the appearance of a contingent and merely empirical historical record will be permanently lost.²⁷

²⁷Fisch, 1991, pp. 22, 112, 165, 186n64, 193n12, notices that Whewell is going meta—that his argument for colligationist inference is itself a colligation of the history of science—and in general Fisch’s treatment is highly recommended. However, he takes Whewell’s primary colligating concept to be “antithetical knowledge,” which seems to me a (rare) misstep on his part.

When one misses the intended form of the Whewell’s argument, one ends up puzzled by what he takes the connections between the parts of his view to be. For instance, Laudan, 1971, p. 378, complains that Whewell does not “offer any valid argument to support his claim” that consilience entails truth: that “the great characteristic of a true theory [is] . . . that the hypotheses, which were assumed in order to account for one class of facts, are found to explain another class of a different nature” (HI II:436); that “such coincidences, or *consiliences* . . . are the test of truth (HI II:429; cf. 370). Snyder, 2006, pp. 183f, takes the argument to be inductive—as Mill and we ourselves understand the notion—which would be a methodological *faux pas* on Whewell’s part. But allow that necessity does entail truth, for roughly Kantian reasons: because one synthesizes one’s theoretical reconstruction of the world in accord with the idea. The colligation of the history of science by the idea of colligation, as rendered into a conception via the notion of consilience, is to show that scientific advances are driven by consilience, that they consist in colligations of the facts by an idea, and that these colligations induce novel necessities. Since necessities entail truths, consiliences will entail truths; and as the demonstration was itself by colligation of the history of science, we will see the claim borne out in the history of science—in Whewell’s view, inevitably.

We can now see that the deductivist reading of Whewell (note 13, above) must be mistaken for a second reason: the case of most interest to Whewell himself cannot be rendered deductively. Whewell’s own spelling out of colligation is nothing like an axiomatization, and of course it does not entail the reconstructed episodes of the history of science in anything like the way that Euclid’s axioms entail geometrical theorems.

Briefly, on Whewell's view, colligation is inference, and so an argument-by-colligation is being used first to establish that colligations are legitimate arguments, and second, that when the science is taking a major step forward, colligation is the mode of inference. Recall once again that Mill understands all inference about matters of fact (where the laws of logic are included under this heading) to be inductive—as *he* understands induction; it follows that colligation cannot be inference. Whewell and Mill cannot so much as agree on what a legitimate form of argument is; how can we expect them to have a useful exchange of any kind?

5

Although each of them devoted the preponderance of their respective discussions to arguments that would count as properly reasoned by their respective lights, both Mill and Whewell took time out to assemble arguments that conformed to the *other's* canons of argumentation.

Once again, Mill took logic to be an inductive science, and the successes of past science were to be the basis for the meta-induction that would show logic to be inductive. Whewell agreed that the history of science was to be the touchstone, and he had devoted himself to it so thoroughly that when someone said of him that “science is his forte and omniscience. . . his foible,” the tag line stuck.²⁸ So Whewell was in a position to object that Mill's views were not borne out by the track record, a complaint that Mill should have been unable to shrug off.

Mill's Four Methods were supposed to be a refinement of crude inductions-by-enumeration, and their warrant had to be that inductions so conducted had been the stuff of successful science. Whewell asked rhetorically: “Who will tell us which of the methods of inquiry those historically real and successful inquiries exemplify? Who will carry these formulae through the history of the sciences, as they have really grown up; and show us that these four methods have been operative in their formation?” (PD 264) After framing the objection which *he* takes to be the decisive one—that when you apply Mill's Method of Agreement, say, and observe that As are always followed by *as*, your having the concepts ‘A’ and ‘a’ in the first place has been left entirely unexplained—Whewell is going on to argue that even in retrospect, that is, even after we are already equipped with the relevant concepts, we cannot see Mill's Methods at work in the history of science.²⁹ And Whewell

²⁸Todhunter, 1876, vol. i, p. 410.

²⁹In the *System of Logic*, Mill of course had supplied examples of his Four Methods at

had likewise dryly preempted Millian invocations of social science, on the grounds that in its current state we cannot plausibly take it to be part of the record of scientific success (PI I:vii, 7 and PD 269).

Mill dismissed consilience, inference to the best explanation, and the importance of making surprising predictions.³⁰ But Whewell could point back to his having documented one after another scientific revolution that had turned on all of these: most extensively, the Newtonian synthesis, with the wave theory of light and Lavoisier's theory of oxygen as close runners-up (HI II:310, III:119). Where Mill regarded the effort of arriving at concepts in whose terms successful inductions are formulated as not part of the reasoning proper, Whewell drew the lesson from his history of mechanics that "the discoverer [has] to struggle, not for intermediate steps of reasoning between remote notions... but for a clear possession of ideas which [are] near each other, and which he [is un]able to bring into contact, because he [does] not yet [have] a sufficiently firm grasp of them"—that is, that attaining control of the concepts is where the intellectual work is done (HI II:38).

And where Mill followed Comte in treating necessity as a superstition, and consequently in dismissing Whewell's attempts to account for the emer-

work in science, esp. at VII:407–433/III:ix. But he also felt that he didn't have nearly the command of the history that Whewell displayed, or anyhow made a point of saying so: "with [the present writer's] comparatively imperfect knowledge of the various physical sciences, the attempt would have been desperate unless the materials had been brought together, and had undergone a partial elaboration, by [Whewell and Herschel's] more competent hands" (VII:284n/III:i:2). Whewell dismissed Mill's examples with this remark: "I confess that I have no expectation of any advantage to philosophy from discussions of this kind" (PD 269).

Mill's own reply to the putatively decisive objection—that it proves too much—can be found at VII:431/III:ix:6.

³⁰VII:495/III:xiv:4; VII:500/III:xiv:6 (the subsequent pages make it clear that these remarks are directed to Whewell); and here is Mill's belittling take on Kepler's abduction: "boldly guessing that the path [of Mars] was an ellipse, and finding afterwards, on examination, that the observations were in harmony with the hypothesis" (VIII:646/IV:i:3). And Whewell complained that "Mr. Mill... has borrowed the term... *Consilience*, but has applied it in a different manner" (PD 274)—perhaps having in mind Mill's appeal to "the accordance of... *à priori* reasoning and specific experience," at VIII:874/VI:v:6. (For an example of this sort of accordance, called "consilience" by Mill, see VIII:920/VI:x:5.)

I remarked earlier on Mill's "Deductive" method, and it is perhaps in the use he makes of it in his program for social science that his rejection of abduction is most prominently on display. Mill insists on methodological individualism (VIII:879/VI:vii:1), and requires that the axioms of a deductive construction of social science be supplied by an independent and foundational science, psychology (VIII:870/VI:v:5). The motivation is evidently that the laws of association (VIII:852ff/VI:iv:3) can be directly established using the Four Methods, thus obviating the need for an abductive inference to axioms of sociology and accompanying concepts not immediately discernable in sense experience.

gence of necessities in science, Whewell replied that “Comte’s . . . distinction of the three stages of sciences, the theological, metaphysical, and positive, is not at all supported by the facts of scientific history. Real discoveries always involve what he calls *metaphysics*”.³¹ Briefly, Whewell’s complaint was that Mill did not live up to his official views.

Whewell, for his part, was committed to the claim that once the facts had been assembled and placed side by side with the idea that colligates them, the colligation could be seen to be necessary, in something like the way that the conclusion of a valid argument could be seen to be inescapable. (PI II:79ff; Whewell insisted, however, that it might take some time, practice, public discussion, and intelligence to appreciate the force of the considerations supporting the colligation [e.g., HI III:136].) Colligation was being presented as the idea that colligated the facts Whewell had assembled in his survey of the history of science. So Mill made a point of talking through selected episodes in that history without bringing to bear the idea of colligation, and visibly without finding deployment of the idea to be inevitable or compelled.³²

³¹PD 275n19; Mill’s rejoinder, which does not properly represent Whewell’s objection, can be found at VIII:929n/VI:x:8.

³²E.g., VII:464–483/III:xii–xiii; or see Mill’s discussion of Aristotle’s allegedly failed colligation of the facts of motion around the idea of natural motion (VIII:657f/IV:ii:4). Mill pointedly substitutes the empiricist notion of abstraction for Whewell’s colligations (as in the title to Book IV, ch. 2 of the *System*); here are a couple of characteristic remarks: “Dr. Whewell’s . . . Colligation of Facts by means of appropriate Conceptions, is but the ordinary process of finding by a comparison of phenomena, in what consists their agreement or resemblance” (VIII:648/IV:i:4); “hypotheses, or, as Dr. Whewell prefers to say . . . Conceptions” (VIII:647/IV:i:4; cf. VIII:701/IV:vi:3).

One of these episodes was Kepler’s discovery of his Laws, and now would be an appropriate time to explain why I have been sidestepping this case, which has served as an anchor for much of the subsequent discussion. Mill complains that since it was already known that the planets returned to their previous positions, and since all those positions had already been observed, Kepler was merely redescribing already available information, and that no new prediction was being made. Since the application of the idea, ellipse, had been adduced as a colligation (of facts about the positions of Mars), Mill is taking Whewell to be committed to colligations involving no prediction, and this, Mill thinks, cannot be what science is about (although he allows that such redescriptions can be “an advance” [VII:292–297, 303, 317/III:ii:3–4, III:ii:5, III:iv:1]).

This is evidently unfair; Kepler’s colligation is, by Whewell’s lights, very unusual in this respect, and normally much of the warrant of a colligation comes from specifically *surprising* predictions being borne out. Whewell observed that “most scientific thinkers . . . have allowed the coincidence of results predicted by theory with fact afterwards observed, to produce the strongest effects upon their observation; and that all the best-established theories have obtained their permanent place in general acceptance in virtue of such coincidences, more than of any other evidence” (PD 273; see also PI II:62–65). For instance,

Where Whewell took it that what needed to be explained in the history of science was the emergence of one after another necessary truth, Mill insisted that the only phenomenon in question was merely psychological rather than logical: that once a theory becomes entrenched, alternatives to it become, as a matter of psychological fact, inconceivable. And the empiricist doctrine of the association of ideas fully accounts for that sort of inconceivability; after a while, you are simply so used to thinking about things one way that you can't think about them any other way (VII:361/III:v:11; VII:242/II:v:6).

Whewell took it that the key step in a scientific advance is the introduction of a novel idea whose content is cashed out as a structure of general claims (in the paradigmatic case, Euclidean geometry, as definitions and axioms). Mill insisted, to the contrary, that while inductions in the sciences typically are cast as having general conclusions, in principle the general claim is an optional step: the real inference proceeds from particulars to particulars, and the syllogistic monitoring can be skipped.³³ When it is,

“the great Newtonian Induction of Universal Gravitation,” Whewell observes, “pointed out an interminable vista of new facts, too minute or too complex for observation alone to disentangle, but capable of being detected when theory had pointed out their laws, and of being used as criteria or confirmations of the truth of the doctrine” (HI II:136; or again compare HI III:22, where Whewell remarks that “in order to give [a theory] full confirmation, it was to be considered whether any other facts, not immediately assumed in the foundation of the theory, were explained by it; a circumstance which, as we have seen, gave the final stamp of truth to the theories of astronomy and optics”).

When Mill insists that “Dr. Whewell’s theory of the logic of science. . . pass[es] over altogether the question of proof” (VII:304/III:ii:5), Mill is ignoring Whewell’s explication of consilience, which he had explicitly presented as the mode of proof appropriate to colligation. Kepler’s colligation is unusual in that respect also: it fails to produce consilience of facts from what were formerly regarded as disparate subject matters. (All of Kepler’s data were after all observed positions of a single planet.) It is perhaps worth emphasizing that consilience was felt even at the time to be the central element of the Whewellian justification for a colligation; that much is clear from Thagard, 1977a, reporting on Darwin.

³³Thus, the point of the note at VII:287/III:ii:2 is that inductive inference is not confined to scientists, and that if nonscientists draw “direct inferences,” we can conclude that “the intermediate stage of a general proposition” is dispensable, and thus the additional concepts alleged to be involved in that stage are unneeded as well. Compare his insistence that “if reasoning be from particulars to particulars, and if it consist in recognising one fact as a mark of another, nothing is required to render reasoning possible, except senses and association: sense to perceive that two facts are conjoined; association, as the law by which one of those two facts raises up the idea of the other” (VIII:664/IV:iii:2).

As Snyder, 2006, p. 106, observes, Mill thought he needed to show that there was no added value provided by the mind to experience, and so he argued that Kepler’s colligating concept would be available in principle in the experience of a properly positioned observer: if Mars left a trail of glowing foam behind it, someone floating in space opposite and suitably distant could see that its orbit was elliptical. Snyder describes the move as “this strange argument of Mill’s” (p. 105), but if we keep in mind that his objective was to

the concepts deployed in the induction must be those that can be directly applied to the particulars, and cannot have built into them the sort of content that Whewell insisted on in his colligating ideas. And in any case, Mill noted, concepts are being added to one's raw perceptions *whenever* one describes what one sees, and no one ever called most of *that* "induction".³⁴

Mill is making a point of not getting Whewell's point, because in Whewell's intellectual world, that should itself count as an argument against the colligating idea (in this case, the idea of colligation at the center of Whewell's logical theory).³⁵ Mill's uncharacteristic posture of obtuseness is his ingenious shot at providing an argument whose force his opponent will have to allow.

6

It is not my purpose here to try to resolve the disagreement I have been describing; I am in any case not sure that there is a point, a century and a half after the event, in declaring one or the other of the parties the winner. But I think that their exchange can serve both as a model of philosophical conduct and as a cautionary reminder.

The Mill-Whewell debate was a dramatic illustration of challenges posed by philosophy of logic, and of what it can look like to try to meet them. Views in logic are views about what counts as an argument. A thoughtful philosopher of logic—and both Mill and Whewell were that—will realize that the arguments one advances for one's logical views ought to conform to the strictures those views imply. But then a philosopher with opposing views is unlikely to see them as arguments at all, and to think that the question at issue has been violently begged. On the other hand, if you try to satisfy your opponent, and the arguments you give fail to conform to your own views about what a successful argument is, why you should count them

resist Whewell's views of colligation, it should not seem strange at all.

³⁴VIII:645/IV:i:3; since Whewell insists that colligation goes all the way down, Mill's point is question begging if it is meant as a rebuttal of Whewell's claim, but entirely in order if what he wants to show is that you needn't see the phenomena Whewell's way.

³⁵Of course, Mill allowed himself "quite content to use Dr. Whewell's *term* Colligation" (VII:305/III:ii:5, my emphasis)—refusing, however, to let it mean what Whewell had wanted it to.

Snyder, 2006, pp. 173, 182, notices that Mill "did not directly address the issue of consilience," and if that were all there were to it, it would have been a grave oversight, and his use of Kepler would have been (for reasons covered in note 32) patently unfair. But Mill was handling it in what was the dialectically most effective way possible: namely, by doing without it entirely.

as support at all?

Both Mill and Whewell faced up to the problem by providing two distinct types of argument for their logical views. Each developed arguments that by their own lights were satisfactory (inductivist arguments, in Mill's case; colligationist arguments, in Whewell's). But each also produced arguments tailored to their opponent's views about argumentation (complaints about Mill's inductive use of the history, in Whewell's case; a performance meant to show that Whewell's colligating idea was not demanded by his data, in Mill's). It is perhaps not the most principled solution; as it so happened, each of the parties thought that his twin modes of argumentation both delivered the same conclusion, but what is one to say when that sort of convergence proves unavailable? Nevertheless, it behooves philosophers today to be alert to the problem to which Mill and Whewell were responding, and—if no better way of solving it comes to mind—at least to do what they did: produce arguments both for oneself and for one's opponent, and if necessary, formally very different arguments.

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