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HISTORY, DISCOVERY AND INDUCTION: WHEWELL ON KEPLER ON THE ORBIT OF MARS*

William Whewell's stature among philosophers has slipped considerably since the late 1960s and early 1970s, when he was widely portrayed as providing an alternative to positivist philosophy of science. Partly as a result of shifting philosophical fashion and partly because the shortcomings and idiosyncracies of the historicist approach have become clearer, philosophical interest in Whewell now tends to focus on his discussion of particular issues such as the role of consilience in theory choice rather than on his more general conception of scientific inquiry. Yet in redressing the balance we are in danger of losing sight of the gains that Whewell undoubtedly made. There remains much to be said for his contention that philosophy of science should be rooted in a close examination of actual scientific practice. And many of his specific insights concerning scientific discovery have still to be fully assimilated by philosophers of science.

As is well known Whewell takes Kepler's discovery of the elliptical orbit of Mars to be an exceptionally clear example of scientific induction. While recognizing that Kepler's "works are . . . extremely curious and amusing", Whewell contends that they "are a very instructive exhibition of the mental process of discovery". These works, he argues, "exhibit to us the usual process (somewhat caricatured) of inventive minds: they rather exemplify the *rule* of genius than (as has generally been hitherto taught) the *exception*" (1857/1967, I, p. 318). In particular he regards Kepler's discovery as deserving special attention since it illustrates especially well the crucial element in all induction, namely the introduction of a new conception. For him Kepler is remarkable if only because he clearly "apprehended that colligation of facts which is the main business of the practical discoverer" (1860/1971, p. 121).

Whewell accepts the standard conception of "induction . . . as the process by which we collect a *General Proposition* from a number of *Particular Cases*" (1847/1967, II, p. 48). But he rejects the assumption that "the general proposition results from a mere juxta-position of the cases" on the grounds that "there is a New Element added to the combination by the very act of thought by which they are combined".

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"The peculiar import of the term *Induction*", he insists, is that "some Conception is introduced, some Idea is applied, as the means of binding together the facts, and thus producing the truth" (pp. 49—50). However "*Induction* . . . was originally or anciently employed", the plain fact of the matter is that "in every inference by Induction, there is some Conception *superinduced* upon the Facts" (p. 50).

In the special case of Kepler's discovery we should think of "the Invention of the Conception [of the ellipse as] the great step in the *discovery*", Kepler's main achievement having been to "superinduce" this geometrical conception on Tycho's data (p. 51). To appreciate the character of the discovery, we need to recognize that Kepler was the first to have "bound together the particular observations of several places of Mars by the notion, or . . . the *conception*, of an *ellipse*, which was supplied by his own mind" (1860/1971, p. 253). This was "an essential element in his Induction": before his investigations the facts were "detached, separate, lawless"; afterwards they were "connected, simple, regular" (p. 254). As in every discovery a "Conception . . . which did not exist in any of the observed facts" had been introduced, "a Principle of Connextion" had been supplied, a "String" on which to hang "the Pearls" had been provided (1847/1967, II, p. 48).

Whewell thus stands foursquare against Mill's suggestion that the sole induction that Kepler performed was to infer from observed positions of the planet to unobserved ones. Whewell agrees that Kepler inferred that the planet "would continue to revolve in [the] same ellipse" and that "during the time which intervened between two observations [the planet's positions] must have coincided with the intermediate points of the curve" (see Mill 1843/1973, p. 293 and Whewell 1860/1971, p. 248). But he is unequivocally opposed to Mill's further suggestion that these inferences constituted "the only real induction concerned in this case". In his view the inferences isolated by Mill contributed little if anything to Kepler's fundamental achievement. They required no special expertise and would have been made routinely by any competent astronomer at the time.[1]

According to Whewell, Mill and like-minded philosophers overlook the very element in scientific discovery that requires special talent, the invention of appropriate theoretical conceptions. It is, he argues, a mistake to think that the facts determine their own interpretation and even more so to suppose that scientists obtain their conclusions by generalizing from unconceptualized facts. In cases of induction such as

Kepler's the relevant inference is to the conclusion that *A*s should be classified as *B*s, not merely to the conclusion that all *A*s are *B*s (given that some of them are). When Kepler found that Tycho's data could be colligated by means of an ellipse, he had — says Whewell — in effect already made the various inductions that Mill singles out for special discussion. As he puts the point, the Keplerian colligation included all that Mill mentions since "a continuous line, a periodical motion, are implied in the term *orbit*" (1860/1971, p. 248).

In arguing that Kepler "superinduced" a conception on Tycho's data, Whewell does not deny that the ellipse law describes the phenomena. On the contrary, he explicitly holds that "the orbit of Mars is a Fact — a true Description of the path" (1860/1971, p. 250). In his view the "description of ellipses" was fundamental to the Keplerian inference (1847/1967, II, p. 51) and the theory that Kepler finally adopted was "a mere representation of the motions and distances as they were observed" (1857/1967, I, p. 331). Indeed Whewell is even willing to go along with Mill's much criticized suggestion that a person with "adequate visual organs and a suitable position" would be able to see the ellipse in the phenomena (see Mill 1843/1973, p. 297 and Whewell 1860/1971, p. 249).[2]

Where Whewell and Mill differ is with regard to the propriety of calling the ellipse law an induction given that it merely describes, represents, sums up the phenomena. The butt of Whewell's criticism is Mill's conclusion that Kepler "found the expression only, not the inference" (see Mill 1843/1973, p. 294). For Whewell finding the expression — i.e. the description — was itself a matter of inference. "There is", he maintains, "no validity discoverable in the distinction which Mr. Mill attempts to draw between 'descriptions' like Kepler's law of elliptical orbits, and other examples of induction" (1860/1971, p. 252). "Description [of the sort involved in Kepler's discovery] is a kind of Induction, and must be spoken of as Induction, if we are to speak of Induction as the process by which science is formed" (p. 249).

To understand what Whewell is driving at here, it is important to remember that he takes uninterpreted observations to be nondescript. "In whatever manner facts may be presented to the notice of a discoverer", he tells us, "they can never become materials of exact knowledge, except [in the event that] they find his mind already provided with precise and suitable conceptions by which they may be analyzed and connected" (1847/1967, II, p. 23). "The fact of the elliptical orbit was not the sum of the observations *merely*; it was the sum of the observations, *seen under a new point of view*" (1860/1971, pp. 256—257). In other words Mill erred when he argued that "conceptions do not develop themselves from within, but are impressed from without" since these "must have both origins, or they cannot make knowledge" (p. 258). Even someone with "adequate visual organs and a suitable position" would — according to Whewell — still be obliged to contribute something from his or her own mind in order to see the planet's orbit as an ellipse.[3]

It is undoubtedly a difficult question "whether or not abstract orbital paths are found in astronomical data" (Butts 1968, p. 28). But some of the mystery can perhaps be dispelled by noting that the positions of planets define ellipses in much the same way as sets of stars define constellations. While Whewell is not always clear about these matters, what he presumably means is that an ellipse can properly be said to be in (and not in) the phenomena in exactly the same sense that a certain figure can properly be said to be in (and not in) the night sky. When we say that a planet has an elliptical orbit or that a group of stars forms a constellation, we do not project new facts into the world but see old ones in a new way. In both cases what we say about the world depends on the concepts that we employ but it is the world itself that determines whether and to what degree what we say is true.[4]

Be this as it may, Whewell can hardly be faulted for failing to recognize the ingenuity involved in scientific discoveries like Kepler's. When reading Whewell one never feels — as one does reading some philosophers — that such discoveries are relatively straightforward affairs, that they are more a matter of good fortune than of intellectual brilliance. It is one of his constant themes that discovery involves "inventive talent" (1847/1967, II, p. 41), that "at each step of the progress of science, are needed invention, sagacity, genius" (1847/1967, I, viii). "A facility in devising hypotheses", he insists, "is so far from being a fault in the intellectual character of a discoverer, that it is, in truth, a faculty indispensible to his task" (1847/1967, II, p. 54). It is not enough to recognize that "the discovery of new truth requires . . . minds careful and scrupulous in examining what is suggested"; we must also acknowledge that "it requires, no less, such as are quick and fertile in suggesting" (pp. 55—56).

Unsurprisingly then Whewell looks askance at the idea of "a logic of induction" construed as a set of general principles for devising new theories. In his view "an Art of Discovery is not possible" and "we may hope in vain, as Bacon hoped, for an organ which shall enable all men to construct scientific truths, as a pair of compasses enables all men to construct exact circles" (1847/1967, I, p. viii). For besides taking it to be indisputable that "no maxims can be given which inevitably lead to discovery", Whewell also maintains that "in every inductive inference, an act of invention is requisite" and that "Induction mounts by a leap which is out of the reach of method" (1847/1967, II, pp. 20, 51 and 92). Indeed Whewell's ultimate position would seem to be that there are no methods for deriving laws from data apart from special methods appropriate to special circumstances (compare Book XIII on the "Methods Employed in the Formation of Science").[5]

Should we therefore conclude that Whewell is — as is commonly assumed — a proponent of the hypothetico-deductive method?[6] Given his emphasis on hypotheses as "guesses" (p. 41), it is tempting to assume that he is. However it is also true that much of what he says runs counter to the deductivist conception. Contrary to Venn, "Whewell's account of the Inductive process [does not resolve] itself into making guesses, and then justifying these guesses by subsequent deduction" (1907/1972, p. 356). Nor is it plausibly regarded — as Ducasse would have it — as "a comprehensive and systematic theory of induction . . . in terms of the so-called Newton method of Hypothesis — Deduction — Verification" (1966, p. 217). In actual fact Whewellian induction is less easily characterized and very much more subtle.

Whewell does not think of scientists as merely showing that particular hypotheses fit the available data, nor does he use the expression "inductive inference" solely to indicate that such a fit has been achieved. In his view scientists do more than deduce observational consequences and check to see whether or not they agree with the facts. Rather they should be seen as proving that hypotheses hold, as establishing that they are true, even as demonstrating their necessity. In this regard at least Whewell is closer to the inductivist who takes inductive inference to be a technique for deriving general conclusions from premises about particular individuals. He does not have anything unusual in mind when he speaks of induction as "the genuine source of all our real general knowledge" (1847/1967, II, p. 47). He is not a deductivist in disguise.[7]

In the final analysis Whewell remains faithful to the Baconian view that "true knowledge is . . . obtained from Facts by Induction" (1847/1967, I, p. vi). To appreciate his position we need to remember that he holds that whenever a new law of nature is discovered "a number of facts . . . are brought into a point of view in which order and connexion becomes their essential character" and "it is seen that each fact is but a different manifestation of the same principle; that each particular is that which it is, in virtue of the same general truth" (1834/1984, p. 315). For all his emphasis on conjecture and supposition, Whewell still takes the "Formula for the Colligation of Facts by Induction" to be "'The Several Facts are exactly expressed as one fact if, *and only if*, we adopt the Conception and the Assertion' of the inductive inference" (1847/1967, II, p. 90).

In the particular case of Kepler's discovery, the relevant "induction" was not to the conclusion that the elliptical-orbit hypothesis is consistent with Tycho's data but to the law itself, Kepler's conclusion having been "the true doctrine, that the planet's path is an ellipse" (p. 42). When Tycho's data had been colligated under the conception of an ellipse, "Kepler's *laws* of the elliptical motion of the planets were established [and] immediately became the *facts* on which the mathematicians had to found their mechanical theories" (1847/1967, I, p. 48). Indeed Whewell even endorses Kepler's claim — which he made in response to a challenge posed by Ramus — to have constructed "an astronomy without hypotheses" (1857/1967, I, p. 331). In Whewell's opinion "this was not saying too much".

These observations notwithstanding, it may still seem that Whewell should be regarded as a deductivist if only because he fails to provide an account of inductive inference. While it is true that he holds that hypotheses are proved, it is also true — as has often been argued — that he has little if anything to say concerning what counts as a proof of a hypothesis. In this regard at least Mill was right: "Dr. Whewell's theory of the logic of science [passes] over altogether the question of Proof" (1843/1973, p. 304). As Buchdahl has observed "there is in Whewell no provision for forms of argument which involve the drawing of conclusions, or the detaching of such 'conclusions' from the evidence [and what is] more extreme even, there is no attempt to assess in any way quantitatively or quasi-quantitatively the inductive probability of any hypothesis" (1971, p. 350; see also Butts 1973, p. 56).

Nor is it helpful to recall that Whewell introduces his "inductive tables" under the rubric of "the logic of induction". True, he avers that "the analysis of doctrines inductively obtained, into their constituent facts, and the arrangement of them in such a form that the conlusiveness of the induction may be distinctly seen, may be termed the *Logic of Induction*" (1847/1967, II, p. 82). But the arrangement of doctrines that he envisions does not by any stretch of the imagination supply "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" (p. 83). Moreover it should not be forgotten that when challenged on the question of whether inductive tables actually constitute a "logic", Whewell allowed that they did not and retorted that this was "*so much the worse for logic*" (see Todhunter 1876, II, p. 417).

Nonetheless it would be wrong to conclude that Whewell has nothing of importance to say about discovery and to treat his position as tenable only insofar as it accords with strict deductivism. While Whewell does frequently seem to be attempting to reconcile the irreconcilable, his remarks are undoubtedly informed by a definite vision of how scientific discoveries are made. Understanding them in the way he intended — as complementary to historical investigations — it is not difficult to see why he considered the issue of the "logic of induction" to be unimportant even as he insisted that scientists prove their results. Indeed there is no need to look further than his analysis of the "Inductive Epoch of Kepler" in *The History of the Inductive Sciences* to appreciate what he had in mind.

In the discussion of the Keplerian discovery in the *History*, which is considerably more detailed than the discussions of *The Philosophy of the Inductive Sciences* and "Mr. Mill's Logic", Whewell outlines some of the more important steps leading to Kepler's final conclusion. Interestingly he does not argue that Kepler made his discovery by superimposing a conception on Tycho's observations but instead focuses on the manner in which Kepler deployed these observations in the course of developing his new view of planetary motion. Here Whewell's main object is to relate the discovery to the wider framework in which Kepler pursued his astronomical investigations and to clarify the various technical considerations that prompted him to revise his inital assumptions and ultimately to accept the elliptical-orbit law itself.[8]

Whewell starts by noting that "the occasion of the discovery of [Kepler's] laws was the attempt to reconcile the theory of Mars to the theory of eccentrics and epicycles [and] the event of it was the complete overthrow of that theory, and the establishment, in its stead, of the Elliptical Theory" (1857/1967, I, pp. 324—325). Then he goes on to detail the "repeated struggles" that led Kepler to conclude that "the path of a planet is [not] a perfect circle", to adopt "the supposition of the oval", to resort to the use of an ellipse to simplify calculation, and finally to appreciate that "he might take another ellipsis, exactly intermediate between the former one and the circle, and that this must give the path and motion of the planet" (pp. 327—329). In a nutshell then Whewell takes Kepler to have "combined the observed geocentric places with successive modifications of the theory of epicycles, till at last he was led, by one step after another, to change the epicyclical into the elliptical theory" (p. 325).

Here what Whewell is describing is not a single "act of colligation" nor a single inductive inference but rather a long and highly complex investigation. Although he undoubtedly holds that Kepler's final colligation was of immense importance, it is the argument leading up to it that receives the bulk of his attention. In his view it is not the specific interrelationships of the Keplerian law and Tychonic data that require investigation but rather the character of the inquiry that led Kepler from the data to the law. To appreciate what Kepler achieved we must attend to the process whereby he arrived at his result as well as to the nature of the result itself. In this and similar discoveries what is needed is not a logical analysis (in terms of the abstract propositional content of the laws and data in question) but a historically-oriented analysis (in terms of scientists' actual procedures).[9]

Whewell does indeed hold that Kepler "made nineteen hypotheses with regard to the motion of Mars, and calculated the results of each" (1847/1967, II, p. 42). But he does not take him to have engaged in nineteen separate inquiries. Rather the opposite: according to Whewell the various stages in Kepler's long struggle were linked together and the whole investigation comprised a single "train of researches" (1857/1967, I, p. 326). As he interprets the historical situation Kepler's formulation of the ellipse law would have been arbitrary and ill-motivated in the absence of the results (both positive and negative) of the astronomer's early inquiries. Instead of thinking of Kepler as having found the ellipse law after a number of unfortunate false starts, we should think of him as having found it because he had thoroughly explored other hypotheses and figured out why these fell short. Here as elsewhere "the discoverer [had] constantly to work his way onwards by means of hypotheses, false and true" (1847/1967, II, p. 59).[10]

In this respect the difference between Whewell's view and the empiricist view often attributed to Mill could hardly be greater. While allowing that the ellipse hypothesis provided a compendious summary of Tycho's observational data, Whewell rejects the suggestion that Kepler summarized them by adding them together. For him it is a fundamental error to assume that Kepler obtained the elliptical-orbit law this way if for no other reason than that Tycho's data had to be interpreted, seen from a point of view, organized in a coherent fashion, reduced to an appropriate form. The calculations that Kepler made were of a quite different order from those envisioned by partisans of the strict empiricist point of view. The ellipse law was not the result of a straightforward process of summation but rather the culmination of a long and complex theoretical investigation.[11]

Bearing these remarks in mind it is not difficult to see why Whewell took the criticism that he had not supplied principles of inductive validity to be beside the point. He would not have seen the need for such principles for the simple reason that he regarded scientific discovery as a process of careful thought and detailed analysis. The clear assumption underlying his observations is that scientists reason their way to their conclusions, that their specific acts of colligation take the form they do because they are informed by a deep understanding of the various theoretical issues involved. It is not for nothing that Whewell compares the discovery of laws with the deciphering of codes (see for instance his 1834/1984, p. 315). Nor is it by chance that he constantly refers to the indispensibility of "sagacity" for the development of science and fruitful colligation (see for instance his 1847/1967, II, p. 40 and p. 55).[12]

This is particularly clear in Whewell's discussion of the ellipse law, which is largely devoted to detailing Kepler's reasons for revising his earlier views and for embracing new ones. "At every step", he observes, "he [i.e. Kepler] endeavoured to support his new suppositions by what he called, in his fanciful phraseology, 'sending into the field a reserve of new physical reasoning on the rout and dispersion of the veterans': that is, by connecting his astronomical hypotheses with new imaginations, when the old became untenable" (1857/1967, I, p. 325). Moreover it should not be overlooked that Whewell maintains that Kepler's discovery of "the right figure was a matter requiring research, invention, resource" (1860/1971, p. 254) and that he draws attention to the trouble that Kepler took to record "the notions by which he had been led to invent or to entertain [the various suppositions that he had made]" (1857/1967, I, p. 326).[13]

Whewell means what he says when he speaks of scientists as learning from their mistakes. Unlike philosophers such as Karl Popper for whom errors merely provide information about what is not the case, Whewell recognizes that scientists can also glean information from them concerning what is the case, that they frequently learn something positive from their mistakes. In particular he reminds us that early astronomers were able to advance their investigations considerably by determining where the errors of inadequate hypotheses lay. While "the *Doctrine of epicycles* . . . was erroneous", he argues, "it was . . . of immense value to the progress of astronomical science; for it enabled men to express and reason upon many important truths which they discovered respecting the motion of the stars, up to the time of Kepler" (1847/1967, II, pp. 60—61). Indeed Whewell even suggests that "all who discover truths, must have reasoned upon many errors" (p. 56).

Nor should the importance that Whewell accords to discussion and disagreement in science be forgotten. For him scientific progress depends in large measure on the clash of ideas and on the ability of scientists to negotiate their way among conflicting demands. "Discussions of Ideas" can, he tells us, be crucial for scientific advance in that they constitute "the Method (if they may be called a *method*) by which the Explication of Conceptions is carried to the requisite point" (1847/1967, II, p. 376).[14] In his view it is a simple but profound fact that "controversies make up a large portion of the history of each science; a portion quite as important as the study of facts; and a portion, at every stage of science, quite as essential to the progress of truth" (1860/1971, p. 255). Furthermore along these lines it should be remembered that the emergence of truth from the clash of ideas is one of the central themes of Whewell's famous account of "The Transformation of Hypotheses in the History of Science" (see pp. 492—503).

It is true that Whewell confounds the contexts of discovery and justification, that he was — as Butts succinctly puts it — "both historically and spiritually a preReichenbachian" (1973, p. 56). But this does not mean that he was committed to an untenable form of psychologism. Quite the reverse: his failure to separate the two contexts should put us on our guard concerning the import of Reichenbach's distinction. If nothing else, Whewell's close analysis of Kepler's discovery provides us with an incentive to look twice at the common assumption that there is a logical gulf between induction construed as a process of discovery and induction construed as a mode of proof (see also my 1985, section

VI). One might even argue that Whewell establishes — albeit inadvertently — that there is a sense in which discovery and justification go hand in hand and that there is a viable alternative to the kind of rigorous anti-psychologism that has long been the vogue.

What I am suggesting then is that Whewell's approach to the issue of discovery differs both from the approach of the traditional inductivist and from that of the modern-day deductivist. From Whewell's standpoint each of the two sides is partially right and partially wrong. On the one hand he rejects the inductivist's assumption about the existence of canons of proof even as he insists that scientists reason to their conclusions. On the other hand he takes exception to the deductivist's contention that reasoning in science is always from (rather than to) hypotheses even as he embraces the thesis that there is no general "Art of discovery". By concentrating on the process or "method" of reasoning to conclusions, he is able to straddle the fence and to account both for the element of creativity, invention and genius in science and for the fact that the discovery of new hypotheses is — to use a phrase of Hanson's — often a "reasonable affair" (1958, p. 71).

One reason why philosophers tend to overlook views such as Whewell's is that they assume that scientists must argue either from data to laws or from laws to data and they forget that scientific conclusions are normally obtained from a vast range of premises. Whewell certainly did take Kepler to have reasoned from Tycho's data but he did not for one moment assume these to have been his only premises, his view being that the final Keplerian colligation rested on a wide range of theoretical, conceptual, mathematical and methodological assumptions. Whewell's picture of Kepler was — as he himself put it — one of an individual proceeding "from observations by means of hypotheses" (1847/1967, II, p. 222). To understand Kepler's achievement we need to keep firmly in mind the point — stressed by Whewell — that he uncovered the planet's true orbit by revising the body of background information that he began with (using Tycho's data and various physical and mathematical principles).

It is also in this light that we should understand the "inductive tables", on which Whewell sets so much store. He does indeed refer to these tables loosely as constituting a logic of discovery but he also expressly states that they represent "the elements and order of [scientists'] inductive steps, [not] the whole signification of the process in each case" (1847/1967, II, p. 77). To read these tables properly, we must consider them in conjunction with the detailed historical accounts of the *History*. For what they in fact schematize is not a set of principles of "inductive validity" but rather Whewell's earlier historical analyses of the steps of specific discoveries (compare Butts 1973, p. 77). In Whewell's eyes the entries comprise summary statements of the starting and finishing points of especially significant scientific investigations and should be thought of as being mediated — as Whewell himself notes in connection with Kepler's discovery — by a variety of further physical assumptions (see the inductive table for astronomy in Whewell 1847/1967, II, between pp. 118 and 119).

Furthermore we can now better appreciate Whewell's insistence that Kepler "established" the laws of the elliptical motion of the planets. The critics are right to point out that the ellipse law cannot be established as an "impregnable fact" merely by noting that it is more accurate and simpler than the epicyclic hypothesis (compare Wilson 1974, p. 246). But this is fully compatible with Whewell's view that Kepler had proved the ellipse law and even with his contention that he had constructed "an astronomy without hypotheses". For it was no part of Whewell's account that Kepler never made use of hypotheses, still less that the ellipse law would never require modification. What he was mainly concerned to argue was that Kepler could justifiably claim to have provided a proof of the law given Tycho's data and a host of auxiliary assumptions. For Whewell, Kepler was justified in thinking of the ellipse law as nonhypothetical because he had shown how it can be derived, because he had established it without relying on assumptions "merely postulated and not proved" (p. 252).[15]

In this paper I have focused on those parts of Whewell's discussion in which he attends most closely to actual scientific practice. This is in line with his expressed aim of developing a philosophy based on a "systematic and regular" study of science and its history rather than "casually and arbitrarily" (1847/1967, I, p. 12). But it is also true that some of what Whewell says appears to have been shaped more by *a priori* philosophical considerations than by a study of science. Thus it is far from obvious that the various Kantian and Platonic elements that permeate Whewell's thinking were — or can be — derived from his historical analyses. And one might certainly query the status of his views (discussed under the rubric of "the fundamental antithesis of philosophy") concerning the inseparability of thoughts and things, theories and facts, ideas and sensations. Doubtless it would be foolhardy to suppose that the various concessions to his philosophical preconceptions that Whewell made in the course of his deliberations were always minor or beside the point.

Nonetheless it seems clear that the theme that I have extracted from Whewell's discussion is central to his thinking about scientific inquiry. Even granting that he compromised his initial vision of a philosophy of science subservient to science and its history, it can hardly be denied that his thinking continued to be informed by his analyses of particular historical cases. The important thing to bear in mind is that Whewell's general philosophical remarks about scientific discovery cannot be detached from his remarks about discoveries such as Kepler's without making them seem ill-motivated, confused, even incoherent. When considering his conception of discovery (and induction) it is crucial that the discussion of the *History* be accorded at least as much attention as the discussion of the *Philosophy* itself.

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NOTES

* I am pleased to have the opportunity to dedicate this paper to Robert Butts on the occasion of his sixtieth birthday. While preparing it I have been often reminded of how much Butts has done to alert us to the importance (and limitations) of Whewell's philosophy of science, Hopefully some of what I have learned from studying his work is reflected in what follows.

[1] Compare Venn (1907/1972, p. 355): "Whewell, in fact, almost ignores the generalizing element in our inductions; not of course that he would deny its existence, but rather because he takes for granted that it would be sure to follow as a matter of course".

[2] Note also that it is beside the point that the Martian orbit cannot be seen to be elliptical since it is not exactly an ellipse and even if it were, it would be too close to being circular to be detected as an ellipse (compare Venn 1907/1972, p. 354 and Ducasse 1966, pp. 214—215). The question at issue is not whether the Martian orbit describes an ellipse exactly but whether it describes one within specifiable limits and whether it could conceivably be seen as so doing.

[3] Thus I take Whewell's treatment of induction to foreshadow Norwood Russell Hanson's account of the "logic of discovery" (see especially Hanson 1958, chapter 4). Whewell's view that Kepler saw Tycho's data from a "new point of view" is much the same as Hanson's view that he discovered a pattern in these data by "pull[ing] together [the data] into a geometrically intelligible pattern" (p. 83). Interestingly Hanson takes Whewell's account of Kepler's discovery to be quite different from his own even though he recognizes that Peirce — whose views he endorses — regarded discovery as "begin[ning] always with [a Whewellian] colligation" (quoted by Hanson, p. 88).

[4] Compare Ducasse's discussion of Whewell's comparison of colligations with cryptograms (1966, p. 202). Ducasse is surely right to note that a new conception is no more an additional fact than is the key to a cryptogram, but presumably the relevant comparison is between the conception and the message embedded in the cryptogram (rather than the key). Also it should be borne in mind that Whewell holds that it is no more possible to discern an ellipse in Tycho's data without an appropriate conception than it is possible to find Kepler's law in his book without a knowledge of Latin (1860/1971, p. 257).

[5] Significantly Whewell devotes the bulk of his discussion of the "General Rules for the Construction of the Conception" to a historical account of Dulong and Petit's investigation of the law for cooling bodies (see 1847/1967, II, pp. 389—395). In fact he not only fails to supply "general rules", he freely admits that "we cannot give rules which will be of much service" (p. 395).

[6] Compare Buchdahl 1971, p. 345, Butts 1968, p. 17, Ducasse 1966, p. 217 and Venn 1907/1972, p. 356. It is perhaps also worth recalling here that the view championed by Hanson (see note 3) is often held to reduce to a form of deductivism and that Hanson himself explicitly states that Whewell's account of Kepler's discovery is "little better than the modern hypothetico-deductive account" (1958, p. 84).

[7] In formulating this point I have benefited from Buchdahl's discussion of the deductivist and inductivist approaches in his 1971, section I (even though I disagree with his interpretation of Whewell as a deductivist). Also I might note here that Butts observes that the standard view of Whewell as a hypothetico-deductive theorist overlooks distinctive features of his methodology (see his 1973, pp. 66ff) and that Ducasse is on record as holding — in apparent opposition to his characterization of Whewell as a proponent of the Newtonian method — that Whewell regards scientists as "*proving* true or false each hypothesis thought of, in its turn" (1966, p. 215).

[8] Whewell's account might be criticized — e.g. on the grounds that it fails to do justice to Kepler's physical arguments (see Wilson 1974, p. 247) — but this does not detract from his discussion of the philosophical issues involved. More complete and more accurate historical accounts can be found in Aiton 1969, Koyré 1973 and Wilson 1974.

[9] Compare Buchdahl's contention that Whewell's "emphasis is all on 'process'" (1971, p. 350), Butts's observation that "Whewell viewed science as an historically developing process" (1973, p. 57) and Strong's point that Whewell employs "the term 'induction' to cover the *modus operandi* of discovery" (1956, p. 231). (Also compare Stoll 1929, chapter IV, which is devoted to "The Process of Discovery".) Furthermore in connection with the present point it is of interest that both Peirce and Hanson treat Kepler's discovery as a process even as they attempt to reduce it to a single "retroduction" (see Peirce 1960, p. 31 and Hanson 1958, p. 76).

[10] It is, I believe, in this context that Whewell's discussion of consilience and other "tests of hypotheses" needs to be considered. According to Whewell these are important aids for "working onwards" in that they constitute "some of the most general . . . processes by which, in certain cases, the discovery of the laws of nature may be materially assisted" (1847/1967, II, pp. 59—60).

[11] See also Ducasse (1966, p. 214): "Whewell would acknowledge that the idea of ellipticity as applied to the orbit of Mars does summarize our observations in the sense that it states them all at once, but not in the sense that it was obtained from them by a

mere process of summation". In passing I should also mention that Mill's position is considerably more sophisticated than usually supposed. For he not only allows that Kepler tried a number of hypotheses before obtaining the correct one, he also explicitly states that his guesses were "skilful" and not merely "lucky" since he was "abounding in knowledge and disciplined in intellectual combinations" (1973, p. 297).

[12] The point here is similar to one to which Ducasse alludes, namely that Whewellian colligations are like diagnoses (see his 1966, p. 215). Unfortunately Ducasse does not pursue this suggestion but adopts the implausible view that to diagnose is to state "that what one perceives is such as it would be if a certain conjecture were true". In addition it should be kept in mind — as Niinuluoto has stressed — that "Whewell's theory of induction is . . . related to the idea of analysis as an 'upward movement'" (1977, p. 292). (Compare in particular Whewell's discussions in his 1847/1967, II, pp. 382—383 and pp. 389—395.)

[13] Here again Whewell foreshadows Peirce and Hanson, for whom Kepler "never modified his theory capriciously, but always with a sound and rational motive for just the modification he makes" (Peirce 1960, p. 31; see also Hanson 1958, p. 84). (In addition compare Whewell's discussion of Dulong and Petit's investigations referred to in note 5. Here too Whewell's emphasis is on the process of reasoning that led to the discovery.) Moreover I should mention that my argument runs counter to Wilson's claim that Peirce was the first to recognize that Kepler's discovery depended on a "succession of reasonings, testings, choices, and exclusions" (see his 1974, p. 249).

[14] In her discussion of Whewell, Stoll observes that it is strange that he discusses the explication of conceptions before the colligation of facts (1929, pp. 68—69). However if the present view is correct, it made good sense to Whewell to have discussed the explication of conceptions first if only because such explication may figure prominently in the development of correct colligations.

[15] Hence I disagree with Wilson's contention that Whewell misunderstands Kepler's response to the Ramean challenge (1974, p. 247). On my account Whewell's reading is similar to the one that Wilson attributes to Kepler himself (see pp. 251—254).

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