Studies in History and Philosophy of Science 41 (2010) 26–40

Contents lists available at ScienceDirect

Studies in History and Philosophy of Science

journal homepage: www.elsevier.com/locate/shpsa

Whewell's tidal researches: scientific practice and philosophical methodology

Steffen Ducheyne

Centre for Logic and Philosophy of Science, Department of Philosophy and Moral Sciences, Ghent University, Blandijnberg 2, B-9000 Ghent, Belgium

ARTICLE INFO

Article history:
Received 7 August 2008
Received in revised form 13 August 2009

Keywords:
William Whewell
Tidology
History of HPS
Scientific methodology
Whewell papers

ABSTRACT

Primarily between 1833 and 1840, William Whewell attempted to accomplish what natural philosophers and scientists since at least Galileo had failed to do: to provide a systematic and broad-ranged study of the tides and to attempt to establish a general scientific theory of tidal phenomena. I document the close interaction between Whewell's philosophy of science (especially his methodological views) and his scientific practice as a tidologist. I claim that the intertwinement between Whewell's methodology and his tidology is more fundamental than has hitherto been documented.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

1. Introduction

Whewell's philosophical, historical, and scientific project has recently regained scholarly interest: a new edition of his collected work, edited by Richard Yeo, appeared in 2001 (*CW*)[1] and between 2005 and 2008 three important monographs on Whewell fell from the press (Wettersten, 2005; Snyder, 2006; Reidy, 2008).[2] Michael S. Reidy's monograph is the first of its kind: it focuses on contextualising Whewell's tidal research.

Primarily between 1833 and 1840, William Whewell[3] attempted to accomplish what natural philosophers and 'scientists' (a neologism he coined in 1833) since at least Galileo[4] had failed to do: to provide a systematic and broad-ranged empirical study of the tides and to establish a general scientific theory of tidal phenomena.[5] Eventually, a royal medal would be awarded to him and John W. Lubbock in 1837 for their joint tidal research.[6] According to R. Robson and Walter F. Cannon, Whewell 'effectively founded these studies as an on-going scientific enterprise along lines which seem quite familiar today' (Robson & Cannon, 1984, p. 184). Whewell was one of the key figures in the 'spatial turn' in tidology, which led to a worldwide collaborative research-project of tidal phenomena between maritime states (Yeo, 1993, pp. 164–169).

Both Snyder's and, especially, Reidy's recent monographs break with the frequently upheld view that Whewell was mainly an obser-

E-mail address: steffen.ducheyne@ugent.be

1 See the 'Manuscripts' and 'Collected works' sections of the references for the meaning of the abbreviations used throughout this paper.

2 See Ducheyne (Forthcoming a) for extensive review of these monographs.

3 Valuable contextualisation of Whewell can be found in: Fisch & Schaffer (1991), Snyder (2006), Yeo (1979, 1993, 2009).

4 In the *Fourth Day* of the *Dialogo* (1632), Galileo offered a geo-kinetic explanation of the tides (Galilei, 2001). An early version of this theory was written in 1616 in a piece entitled *Discorso del flusso e reflusso del mare*. To Galileo's mind, the tides were a definite physical proof that the earth moved (see furthermore Ducheyne, 2006, pp. 453–459; Palmieri, 1998; Naylor, 2007).

5 For an excellent overview of the development of nineteenth-century theories of tidal phenomena, see especially Darrigol (2003). On the history of the study of the tides in general, see Cartwright (2001), and Deacon (1971).

6 It should be stressed that, although Whewell's tidal papers were published under his name, they were the joint outcome of the collaborative work, made possible by the British Association for the Advancement of Science and the British Admiralty, of various observers, seamen, calculators and tide table makers, and that before Whewell's publications a renewed interest for tidology was awoken in Great Britain (Reidy, 2008, pp. 81–86, 90–121). According to Whewell, tidal research (and science by extension) was hierarchically structured: the 'scientist' did the theoretical processing (theory construction and refinement) of the data provided by 'subordinate labourers' of 'less elevated pretensions' (ibid., pp. 15–16, 98–235, 238, 254, 270). These labourers occasionally made active contributions. Reidy has documented Thomas G. Bunt's, Daniel Ross's, and Thomas Bywater's crucial roles in Whewell's tidal research (ibid., pp. 204–228).

0039-3681/$ - see front matter © 2009 Elsevier Ltd. All rights reserved.
doi:10.1016/j.shpsa.2009.12.011

ver and critic of science. According to Richard Yeo, Whewell was essentially a meta-scientist, or looker-on-science: that is, he created for himself 'a role as the critic and reviewer, adjudicator and legislator of science' (ibid., p. 8).[7] With respect to the tides more specifically, Yeo noted that Whewell 'felt inadequate in not being able to push beyond careful observation to an advance in "hydrodynamical theory"' (ibid., pp. 54–55). Menachem Fisch dismissed the importance of Whewell's tidal studies for his philosophical-methodological views (Fisch, 1991b, pp. 58–59). Joan Richards's opinion on the matter is closely aligned with Yeo's, for she claims that Whewell 'was more an observer [of science] than a participant [in science]' (Richards, 1996, p. 235). Yeo stresses that Whewell's contributions in mineralogy and tidology 'were important, but neither met his own criteria for truly significant advances in science, and they did not compare with those of leading men of science he counted among his friends' (Yeo, 1993, p. 54; cf. 2009). Yeo strengthens his thesis of Whewell as a looker-on-science by pointing to the fact that 'he did not consider himself a major scientific discoverer' (Yeo, 1993; cf. 2009) and that he failed to establish the new hydrodynamics required to tackle the problem of the tides (Yeo, 1993, pp. 54–55).[8] However, from both observations it follows only that Whewell did not see himself as a *great* scientist, not that he did not see himself as a scientist *tout court*. Moreover, to claim that Whewell was primarily an observer of science and not a participant is somewhat unfair in light of his numerous scientific papers on the tides (see Reidy, 2008, pp. 126–127). Granted, his tidal research did not establish an adequate theory that could explain all tidal phenomena; however, Whewell surely thought that the process of collecting a body of trustworthy data and that exploring the equilibrium-hypothesis (in its standard or modified version) that potentially could explain these data, constituted genuine progress in tidology. Laura J. Snyder points out that Whewell, in view of his tidal researches, 'had first-hand knowledge about the methods of empirical research' (Snyder, 2006, p. 150), and that 'both current scientific practice and the history of science were important to Whewell in developing his philosophy of science' (ibid., p. 151). Moreover, in his recent book Michael S. Reidy stresses the importance of Whewell's tidal researches for the methodological views he developed in his *Philosophy of the inductive sciences* (1840):

> His early work on tidology also taught him valuable lessons concerning the discovery process, including the difficulty of connecting facts with theory, the disparate ways of testing those theories, and the proper methods of data analysis and representation. (Reidy, 2008, p. 14; cf. p. 155)

More specifically, Reidy has called attention to the connection between Whewell's tidal research and his discussion of the 'Special Methods of Induction Applicable to Quantity' in *Philosophy of the inductive science*: that is, the methods of Means, Least Squares,[9] Residues, and Curves[10] (Reidy, 2008, pp. 182, 220, 245).[11] Reidy has convincingly shown that the quantitative methods were standard exercises in Whewell's tidology. Reidy (2008) did not show, however, that Whewell's views on scientific methodology changed over time in view of his tidal research; neither did he engage much in Whewell's philosophy of science. By doing so here, I provide additional substance to Reidy's recent suggestion that the sections on the Special Methods in the *Philosophy of the inductive sciences*[12] rendered explicit the changed views that Whewell came to from his studies in tidology.[13] Though agreeing to a large extent with Snyder's and Reidy's recent findings, I seek to go beyond their claims. So far, no *systematic* and *detailed* attempt has been made to connect Whewell's philosophy of scientific methodology with his scientific practice.[14]

In this essay, I attempt to trace the close intertwinement between Whewell's philosophical views on scientific methodology and his actual scientific practice as a researcher of tidal phenomena. I shall begin my study (see Section 2) by putting Whewell's tidology in the context of physical astronomy; that is, his tidology will be put within the context of the Newtonian theoretical framework—a point that has escaped the attention of previous commentators (Deacon, 1971; Reidy, 2008; Ruse, 1976, 1991).

Next, it will be shown in Section 3 that Whewell's thoughts on scientific methodology in the early 1830s (as expressed in his 1830–1833 notebooks on induction) were still quite rudimentary. I show that his views on scientific methodology changed significantly between the early 1830s and 1840 (see Sections 3 and 4). This change did not involve Whewell's abandonment of his earlier views on methodology, but rather refers to a more sophisticated level of detail and elaboration of his later views vis-à-vis his early views. As a welcome side-effect, my analysis of Whewell's notebooks on induction further points to the difficulties of Menachem Fisch's 'erotetic reconstruction' of Whewell's intellectual

[7] The term 'looker-on-science' might be used rightfully for the early Whewell. Compare 'There is another point of view which occurs to us lookers on, who, not making a single experiment to further the progress of science, employ ourselves with twisting the results of other people into all possible speculations mathematical, physical, and metaphysical' (Whewell to Herschel, 1 November 1818: *CW*, Vol. 16, p. 29). Note that Yeo uses this letter to strengthen his 'looker-on science' thesis (Yeo, 1993, p. 54).

[8] Yeo points out that Whewell admitted that 'there is nothing of such a stamp, that what I have attempted, as entitles me to be considered an eminent man of science' (Whewell to Murchison, 18 September 1840: *CW*, Vol. 16, p. 286; cited in Yeo, 1993, p. 55).

[9] The Method of Least Squares is a variation of the Method of Means. It is helpful in establishing the most probable law by selecting that law of which the sum of the squares of errors is as small as possible.

[10] In a crossed-out section of the printer's copy of the *Philosophy of the inductive sciences* Whewell wrote in a more cavalier moment that the Method of Curves is 'the true way of discovering the laws of nature by which they are produced' (WP, R.6.18^9(6), fol. 52^r).

[11] On the Method of Means, see Whewell (1835), p. 84; (1837b), p. 231; *PIS*, Vol. 5, pp. 403–408. On the Method of Curves, cotidal lines and T. G. Bunt's tide-recording device, see Whewell (1833), pp. 147, 149, 157; (1837a), p. 76; (1838a), p. 250; *PIS*, Vol. 5, pp. 395–403; Whewell (1848), p. 24. On the Method of Residues, see Whewell (1831b), pp. 401–402; WP, R.18.11^8, fol. 137^r; Whewell (1834b), pp. 24, 26, 27, 29–31, 33, 43; Whewell to Lubbock, 30 October 1835: *CW*, Vol. 16, 229–230; Whewell (1837a), pp. 76–77; (1838b), p. 236; *PIS*, Vol. 5, pp. 409–412.

[12] Here I use the second and most commonly cited edition of the *Philosophy of the inductive sciences* (1847). The parts that are relevant to Whewell's 'Special Methods of Induction Applicable to Quantity' are identical in both editions—apart from some small text-editorial changes (Whewell, 1840b, pp. 542–559; *PIS*, Vol. 5, pp. 395–412). Book XI, 'Of the construction of science', of the first edition (Whewell, 1840b, pp. 169–277) is identical to Book XI of the second edition (*PIS*, Vol. 5, pp. 3–118). The printer's copy of the first edition of the *Philosophy of the inductive sciences* is preserved at the Wren Library (WP, R.6.18$^{8–9}$). It contains some crossed-out sections that didn't make it to print. In the second edition more titles and subtitles were added. The 'Special Methods of Induction Applicable to Quantity' reappeared in *Novum organum renovatum* (the third edition of *Philosophy of the inductive sciences*, 1858) without relevant changes (*NOR*, pp. 202–219).

[13] Note that at places in the *History of the inductive sciences* where Whewell discussed taking the means of observed values (one occurrence: *HIS*, Vol. 1, p. 109), residual phenomena (eight occurrences: *HIS*, Vol. 1, pp. 231, 237, 242; Vol. 2, pp. 259, 388, 505, 539; Vol. 3, p. 39), and curves (thirty-one occurrences: *HIS*, Vol. 1, p. 447; Vol. 2, pp. 24, 29, 37–38, 56–57, 75, 94, 99, 109, 112, 163–164, 168, 243, 313, 330, 336, 338, 349, 353, 381, 386, 405, 418, 457, 486, 521–522, 422–423; Vol. 3, pp. 72, 74, 108, 117), he remained fairly superficial on these issues: he never came close to a detailed, methodologically relevant discussion. In his tidology, the Special Methods are used constantly and are explicitly described in the methodologically advanced way of his later *Philosophy of the inductive sciences*.

[14] Michael Ruse has suggested that scientific activity inspired Whewell's philosophy, but he did not further elaborate on this (Ruse, 1976, p. 252). One important caveat from the outset: I do not assume that *only* tidology helped to develop his methodological views. Other contemporary scientific disciplines (for example optics, crystallography, photistics, thermotics, political economy, etc.) may have contributed to this as well. This remains to be further documented, but cannot be undertaken here. In my own defence, I stress that tidology was the only scientific discipline to which Whewell contributed over such a long time-span and to such a level of engagement. That Whewell's tidology and his philosophy of science interacted has been asserted by others, yet the details of this interaction have not been documented—leaving such claims at a level of high generality.

development, according to which his early textbooks on mechanics were the starting point of an attempt to explain the structure of excellent science, and Whewell developed a philosophical position in which both his Baconian and Langrangian inclinations could be reconciled.[15] As Fisch's reconstruction has been found untenable[16], the quest for a more accurate reconstruction is still open.[17] Here, I want to study if and how Whewell's tidal studies contributed to the development of his later and more sophisticated views on methodology.

Hereafter, I turn to Whewell's tidal research proper (see Section 4). In Section 4.1, I document his tidal research. Next, I argue that not only his historical studies of science[18] but also his tidal research offered him a concrete means to develop and refine his methodological views (see Sections 4.2 and 4.3). His philosophy of science inspired his scientific practice and vice versa.[19] This will be shown by focussing on Whewell's attempt to put equilibrium-theory to the test, his comments on the criteria for useful hypotheses, and the occurrence of consilience of inductions in tidology. Contrary to my claims on the Special Methods, I do not claim that Whewell's methodology (with regard to theory-testing, criteria for fruitful hypotheses, and consilience) was the outcome of his tidal research: both constantly interacted and it is hard to tell cause from effect. In the same section a discussion of Whewell's 1848 Bakerian Lecture is also provided—an important methodological paper missed by Reidy in his recent monograph. Next to this, an issue that has baffled previous commentators is resolved: namely, Whewell's preference for equilibrium-theory. It may be shown that Whewell's commitment to equilibrium-theory can be explained by his own methodological considerations on what counts as a useful hypothesis. Past commentators, who did not systematically study the interaction between Whewell's methodology and tidology, have missed this point.

2. Tidology as a branch of physical astronomy

In his first papers on the tides, Whewell reflected on the state-of-the-art knowledge of 'tidology', that is, the study of (the laws of) the tides.[20] Whewell remarked that, although a lot of progress in bringing theory and evidence in accordance with each other had been made by Newton (1999 [1726], pp. 874–880), Bernoulli (1738), Brémontier (1809), the Webers (Weber & Weber, 1825), Russell (1838), Fourier (1818), Cauchy (1827), Kelland (1840, 1844), and Airy (1841), no one had yet been able to explain tidal phenomena in their particulars (*HIS*, Vol. 2, p. 57). Imagine, Whewell wrote, that our current astronomical knowledge perished 'by some great natural or moral convulsion' and that only a few general notions, such as universal gravitation, concerning astronomy remained, but that 'the resources of mathematical art', 'the collected stores of observation', and 'the habit and apparatus of observing (Whewell, 1834b, p. 15; 1836a, p. 1) were swept away.[21] Our current knowledge of the tides, Whewell continued, was similar to this hypothetical state of affairs: there was no systematically arranged body of tidal data and no theoretical synthesis of the phenomenon of the tides on a par with physical astronomy, that is:

> It has not been shown, by any author, that the general course of the effects produced upon the tides, by the changes of position and distance of the heavenly bodies, is such as, according to the mathematical reasoning, it ought to be. (Whewell, 1834b, p. 17)

Up until the mid nineteenth century, the laws connecting the tides with the motions and distances of the sun and the moon were not known for any single port. Moreover, as Whewell lamented:

> Our philosophers assert, without hesitation, that this phenomenon is the result of the law of universal gravitation of matter; *yet no one has hitherto deduced, from this law, the laws by which the phenomena are actually regulated* with regards to time and place. (Ibid., p. 15; my emphasis)

Whewell's concern fits nicely with Isaac Newton's dual methodology (see Ducheyne, 2005a,b). In Query 31, Newton wrote:

> As in Mathematics, so in Natural Philosophy, the Investigation of difficult Things by the method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For hypotheses are not to be regarded in experimental Philosophy ... By this way of Analysis we may proceed from Compounds to Ingredients, and from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particular Causes to more general ones, till the Argument end in the most general. This is the Method of Synthesis: And the method of Synthesis consists in assuming the Causes discover'd and establich'd as Principles, and by them explaining the phaenomena proceeding from them, and proving the Explanations. (Newton, 1979, pp. 404–405)[22]

Natural philosophy thus proceeds along two types of demonstrations: the first *from effects to causes* (the analysis); the second *from causes to effects* (the synthesis). After he had quoted Newton's famous analysis–synthesis distinction in the *Philosophy of the inductive sciences*, Whewell noted that the Newtonian analysis consists of 'exact *observation* and *measurement*', '*decomposition* of facts', '*selection* and *explication* of the appropriate conception', and the '*colligation* of facts', while the Newtonian synthesis consists of 'those steps of *deductive reasoning*, proceeding from the conception once assumed, which are requisite for the comparison of its consequences with the observed facts' (*PIS*, Vol. 5, p. 278). The analysis, that is, the derivation of the (primary) cause of the tides, had been provided, for Newton had established by the theory of universal gravitation that the attractive forces of the sun and the moon produce a tide-generating force. However, it needed to be shown

[15] As developed in Fisch (1991b), Chs. 2 & 3; (1991a). On the meaning of erotetic reconstruction, see Fisch (1991b), pp. 11–16.

[16] For devastating criticism of Fisch's portrayal of Whewell as a 'Langrangian-Baconian', see Becher (1980), pp. 14–34, and especially (1992).

[17] Much of my recent work on Whewell is intended to provide an alternative reconstruction of his philosophical-methodological development. Whewell's tidal researches are but one part within that story. A second part, which is covered in Ducheyne (Forthcoming a,b), is Whewell's appropriation of specific Kantian elements in his epistemology—I do not, however, conceive of Whewell as an (orthodox) Kantian in the way that Robert E. Butts has portrayed him (Butts, 1965). A third and final part, which remains to be carried out, is to assess what Whewell learned methodologically from studying the history of science.

[18] Cantor claims that Whewell turned to the history of science in order to test his theory of induction (Cantor, 1991, pp. 69, 71). According to Whewell's own statements, his *History* and *Philosophy* were composed simultaneously (*HIS*, Vol. 1, p. 16). In a letter to Richard Jones on 6 October 1834, Whewell wrote: 'I write at the same time two Books, one of history, and one of philosophy, and when I find myself, in the course of my historical researches, becoming metaphysical and transcendental, I open *Book two*, in which all these things fall into their places' (*CW*, Vol. 16, p. 193).

[19] See Ruse (1991), p. 87. Again Ruse's claim is not based on a detailed account of Whewell's tidology.

[20] His tidal papers are briefly documented in Todhunter's account of Whewell's writings (*CW*, Vol. 15, pp. 75–88); see furthermore Ruse (1976); and Becher (1991). On Whewell's tidology see Deacon (1971), pp. 251–275; and Reidy (2008), pp. 122–271.

[21] A draft of Whewell (1836a) is preserved at WP, R.6.20[23(61)].

[22] It is worth observing that Whewell copied exactly this quote in one of his notebooks on induction (*c.* 1830–1833: WP, R.18.17[10], pp. 41, 43).

how the law of universal gravitation could account for concrete tidal phenomena. Newton's account was a rude approximation of the matter (*HIS*, Vol. 2, p. 135). According to Whewell, the synthetic counterpart in tidology was simply lacking: the contributing or counteracting causes were largely unknown (Whewell, 1831a, pp. 166–168). Newton's theory, as it stood, had virtually no explanatory value in tidal research and was frequently inconsistent with observation (Reidy, 2008, p. 11). In fact, 'the only way in which the assumptions [i.e. the explanation given for the tides by universal gravitation] could be justified would be our finding, from observation, that the laws of the facts are such, or nearly so, as these calculations give' (Whewell, 1834b, p. 16). Whewell's intention was thus to call attention to local and specific conditions that, in conjunction with the law of universal gravitation, produce the broad myriad of tidal phenomena. If such 'initial conditions' or 'contributing causes' could be treated systematically, then we could establish a true and complete synthesis of the tides; that is, we could deduce tidal phenomena from universal gravitation. It was this goal that Whewell in the early 1830s sought to accomplish in tidology.

3. Whewell's thoughts on the process of induction before his tidal research

In a letter to Richard Jones written in 1833, Whewell claimed that the phenomena of the tides could nicely be accommodated by his philosophy and methodology of inductive science:

> This being so, I am meditating the returning forthwith and in earnest to my beloved Induction. I have been employed all the term hitherto upon a thumping[23] paper of mine on the Tides, which I intend to be a step of some consequence in the theory. I wish I could explain to you how useful my philosophy is in shewing me how to set about a matter like this, and how good a subject this one of the Tides is to exemplify it. (Whewell to Richard Jones, 31 October 1833, *CW*, Vol. 16, p. 172)

Now, what were his views on induction at that time? In what way did he (or could he) think that his 'beloved Induction' was useful for tidal research? The answers can be found, I argue, in Whewell's notebooks on induction (composed between 1830 and 1833; classmarks: WP, R.18.17[5–15]). These notebooks on induction were written 'with the object of discovering what are the processes by which their advance to this state of completeness has been ↓brought about↓, the conditions by which advance was secured, the faculties of man which it has called into plan' (WP, R.18.17[8], fol. 1r, entry dated December 1833). Whewell sought to establish the conditions under which science is successful (ibid., fol. 4[r]) and to renovate Bacon's ideas on induction (ibid., fols. 6[r], 7[r], 8[r]). He stressed that our ability to know the natural world and to make inductive generalisations depends on universal principles (for example the notions of space, time, and cause) that reside in the constitution of the mind and that regulate all our perceptions. Whewell wrote: 'Phænomena are incapable of being received without being subordinated to regulative and interpretative conceptions' (WP, R.18.17[15], fol. 56[r], dated 1831–1832). Knowledge thus implies both passive and active thought: 'collection of impressions' and 'the operations of the reason' (WP, R.18.17[8], fol. 19[r]). The actions of the mind work on impressions provided by the senses (ibid., fol. 36[v]). Whewell noted that by using language 'we do not expose our impressions only, but expose them modified and transformed by the operations of our thoughts' (WP, R.18.17[7], p. 23, dated 1830–1833), so that human minds are 'perpetually exercising a formative and productive power' (ibid., p. 24), which is 'exercised upon the rude material' (ibid., p. 41). Such principles, which 'are part of the original furniture of the common or unsystematic reason' (ibid., p. 14) and which spell out '↓universal↓ and familiar modes of contemplating objects' (ibid., p. 18), have been brought to light and systematised during the course of human history. According to Whewell, 'sound and real physical science consists <u>in apprehending a general fact of observation by means of ↓distinct↓ ideas'</u> (ibid., p. 61, also p. 63). Whewell warned that he did not use the term 'idea' in its customary sense, and noted that 'the ideas of which I have to speak are general notions of relation, connexion, dependence, by which ↓such↓ conceptions are combined with one another' (ibid., p. 61). Whewell sought to unravel 'the general fundamental convictions and laws' underlying human reasoning and science (WP, R.18.17[8], p. 12). His aim was to show how these laws or principles gave rise to sound scientific knowledge:

> Our object is to ascertain the ↓general↓ laws which govern the formation and progress of knowledge in the largest sense; And the course which we purpose to follow leads us to examine their ↓laws↓ in the first place, as they have operated in those branches of human knowledge which are more peculiarly termed *Sciences*, and in which the certainty and progressive character of our knowledge are most striking and incontestable ... Science may be ↑for our purpose↑ described as <u>speculative knowledge of general truths</u>. (Ibid., p. 84)

In a very schematic way Whewell pointed out what the 'Steps in the Method of Induction' are. According to his first attempt at classification, the process of induction consists of four consecutive steps:

I. Primary Induction from Particulars.
II. Initiation of Primary Induction.
III. Successive Generalisations.
IV. Redescent to particulars from Principles established by Induction.

(WP, R.18.1711, fol. 1[r], dated 1830–1833)

Steps I to III refer to the analytical part of science: the establishment of general principles.[24] In steps I and II we make inductive generalisation from particulars. By combining different generalisations from particulars we arrive at successive generalisations—that is, generalisations with a larger domain of application—and, ultimately, at the most general principle. Step IV refers to the synthetic part: the derivation of other particulars (originally not included in the analysis) from the most general principles we have established. Whewell only further commented on step I: he noted that this step presupposes 'Regulative Conceptions' or 'Conditions of Inductivity' such as space, time, motion (a combination of space and time), order, cause and effect, resemblance, opposition-contrariety, and elementary composition (WP, R.18.17[11], fol. 1[r]). In another notebook, we can trace Whewell's second and somewhat more developed—but still very sketchy—attempt at classifying the process of induction (this entry is dated 22 July 1831). There he divided the process of induction in 'Experimental physics or Sciences of Experiment'[25] as follows:

[23] Whewell's first paper on the tides counted ninety pages.

[24] Cf. Whewell's later statements in *On the philosophy of discovery* (1860: *PD*, p. 184).

[25] On sciences of classification (which he contrasted with sciences of experiment) Whewell noted: 'We have some sciences of <u>thoughts</u> where the conceptions are rather interpretative than regulative—that is they do not present to us the facts w[ch] we consider, as necessarily together bound in space, time', that is, 'they do not present them to us as being necessarily thought of in a certain way' (WP, R.18.17[15], fol. 50[r]).

1. Common observation and Collection of Phenomena/Instances, occasional occurrences
2. decomposition of phenomena and Perception of simpler connexions
3. Insulation of facts and Terminology[26] (technical)
4. Insulated experiments [i.e. systematic experiment and measure of insulated facts (WP, R.18.1715, fol. 39ʳ)]
5. Induction 1 Laws of Phenomena
6. Induction 2 Causes of Laws

(Ibid., fol. 39ᵛ)

According to the division suggested by Whewell, the process of induction proceeds as follows. The first step is observation of particulars. Such observations then suggest various ways of decomposing phenomena mentally into simpler relations. On step 2, the 'Decomposition of Facts', he added that at this stage the decomposition is conjectural, but once we come to experiment we no longer decompose phenomena in our thought but in reality (ibid.). Facts are decomposed 'either into ↓conclusions↓, or into simpler connexions' (ibid.). He also wrote 'If this conjectural law is false, try another, and alter the terms if necessary' (ibid.). In the next step, we give technical terms to these decomposed phenomena. To test whether a mental decomposition corresponds to reality we have to perform a systematic experiment and thus to quantify the components of the phenomenon under consideration. If this decomposition turns out correct then we have established a law of phenomena. Once we have further investigated the laws of phenomena it is possible that we will be able to penetrate further into the causes of these empirical regularities. In steps 5 and 6 Whewell distinguished between induction of laws of phenomena (induction 1) and induction of laws of causes (induction 2).[27] On step 6 he noted that 'The highest step of science is the knowledge of causes' (WP, R.18.17[15], fol. 43ʳ). It is reasonable to suggest then that, when Whewell wrote to Richard Jones in 1833, he thought that proceeding along these six consecutive steps could be useful to deal with the problem of the tides. Careful study of Whewell's notebooks on induction reveals that his views on scientific methodology were still quite rudimentary before he actively embarked in tidal research.[28] It is precisely my claim that Whewell's active involvement in tidology contributed in arriving at the more detailed and elaborated methodological views he spelled out in the *Philosophy of the inductive sciences*.

In his early notebooks, Whewell's thoughts about induction were still very much under construction and quite vague. Therein he did not elaborate much on the details of the process of induction. Neither, did he raise important methodological issues such as hypothesis-testing, consilience of inductions, or the quantitative methods involved in induction. In the *Philosophy of the inductive sciences*, that is, after the period in which most of his tidal research appeared in print, this would change.

In his 1830–1833 notebooks Whewell thus gave attention to both inductive method and the (regulative) conceptions provided by the mind,[29] *pace* Fisch, who claims that Whewell only became concerned with epistemological issues after the appearance of the first edition of the *History of the inductive sciences* and that his transcendental turn appeared between 1837 and 1839 (Fisch, 1991a, pp. 37, 62, 64–65). Yeo has also pointed to Whewell's 1820s interest in matters of inductive philosophy (Yeo, 1993, p. 62), as did Harvey W. Becher, who points out that:

> Whewell, at the latest, from 1814, read, discussed, and understood Locke, Berkeley, and the Scottish Common Sense School; at the latest, in the mid-1820s encountered Kant, and from the first, based his mechanics on a division between contingent and a priori truths'. (Becher, 1992, p. 382)

Furthermore, in view of such regulative conceptions, Fisch's claim that Whewell's six-step scheme of the inductive method was 'orthodox Baconian' (in the sense of hardcore empiricist; Fisch, 1991a, p. 54) needs to be taken *cum grano salis*, for (1) Bacon was hardly a hardcore empiricist himself and (2) the content of Whewell's 1830–1833 notebooks and his review of Herschel's *Discourse* belie such reading.[30] This further renders Fisch's erotetic reconstruction of Whewell's philosophy doubtful. Also, Fisch's contention that in 1834 Whewell introduced a radically new concept of induction—induction as *superinduction* (Fisch, 1991a, pp. 58–59)—is hampered by the fact that Whewell in his review of Herschel's *Discourse* wrote that induction 'does *more* than Observation, inasmuch as she not only collects facts, but catches some connexion or relation among them' (Whewell, 1831b, p. 379; cf. Whewell's letter to Richard Jones on 19 February 1832, *CW*, Vol. 16, p. 141; Yeo, 1989).

4. Connecting Whewell's tidal research and his philosophical methodology[31]

In 1833 Whewell declared to John Herschel that he was going 'to do something about the tides' and asked him to send his opinion on whether the propagation of a tide-wave as a hydro-dynamical phenomenon could be accepted as an approximation to a real case 'on the *common suppositions*' (i.e. by equilibrium-theory; Whewell to Herschel, 14 January 1833, *CW*, Vol. 16, p. 153). In the same year Whewell's first research paper on the tides was published, and with this paper his 'hunt' for the tides began. In his first papers on the tides, he began sketching the problem at stake and pointed to the insufficiency of the available tidal theories. Whewell commented that the lack of a proper theory of the tides derives from the virtual absence of unified and interpreted data—according to Whewell, uninterpreted data only led to confusion (Becher, 1991, pp. 6–7)—and from the problematic presuppositions underlying contemporary tidal theories. In Section 4.1 below a brief outline of Whewell's main contributions to tidology is provided; in Sections 4.2 and 4.3 I shall connect Whewell's tidology with his philosophical-methodological work. In 4.3 Whewell's views of the theoretical status of equilibrium-theory and theory confirmation are discussed.

---

26 The significance of the introduction of technical terminology, Whewell noted, had already been stressed by John Herschel in his *Preliminary discourse* (1831: Whewell, 1831b, p. 390).

27 This distinction can further be found in WP, R.18.17[5], p. 25 (entry dated 1832) and in WP, R.18.17[15], fol. 46ʳ. In WP, R.18.17[10], p. 17, Whewell distinguished between '1 Observations of Phænomena 2 Phænomenal Laws—3 Physical Laws'. Later, in the *History of the inductive sciences*, he contrasted formal sciences with physical sciences.

28 Cf. Cantor's view that by the mid 1830s Whewell had achieved 'an early but not fully worked-out version' of his theory of induction (Cantor, 1991, p. 69).

29 For a defence of such reading of these notebooks, I refer the reader to Ducheyne (Forthcoming b).

30 Related points are made in Wettersten (1993), pp. 495–499; and Snyder (1999), pp. 532–539, 546–550.

31 See Whewell (1833, 1834a,b, 1835, 1836a,b,c, 1837a,b, 1838a,b, 1839a,b, 1840a,b, 1848, 1850, 1851).

4.1. Whewell's 'hunt' for the tides[32]

With respect to lack of observational data, Whewell commented that the specifics on how universal gravitation causes the tides are absent:[33]

> even up to the present day this general explanation has not been pursued into its results in detail, so as to show its bearing on the special phenomena of particular places,—to *connect* the actual tides of all different parts of the world,—and to account for their varieties and seeming anomalies. (Whewell, 1833, p. 147; my emphasis)

He complained that few data are publicly accessible, since most of them have been 'kept as secrets, and handed down as private property from one generation to another' (Whewell, 1834b, p. 16). As the history of science had shown (especially the development of astronomy), the rendering public of observational tables and the confrontation of data with theory were the speediest ways to establish a true theory (ibid., p. 40; cf. Reidy, 2008, pp. 230–232, 242–243). Whewell commented as follows:

> And thus the study of the tides might be pursued, and, to do the subject justice, ought to be pursued, in the same manner as the study of the other provinces of astronomy: that is, constant and careful observations should be made of the phenomena; and, as fast as they are made, should be reduced and discussed at the public expense; so as to test the accuracy of the tables already obtained, and to supply the means of making them still more accurate. (Whewell, 1838b, p. 232)

He noted that although some local tidal phenomena had been studied, 'no one appears to have attempted to trace the nature of the *connexion* among the tides of different parts of the world', leaving our knowledge of the tides 'very imperfect and doubtful' (Whewell, 1833, pp. 148, 219; my emphasis). In other words, tidal observations have not been properly generalised (ibid., p. 148). Unravelling the universal patterns of the tides was Whewell's main interest (Deacon, 1971, p. 256). Collecting accurate observations of tidal phenomena was paramount in order to deduce the proper theory of the tides. The larger the bulk of observations, the more accurate the harvested results will be. As an example of this, Whewell pointed to Lubbock's 1831 paper on the tides, where 'above 13,000 observations, extending through nineteen years' were collected (*PIS*, Vol. 5, p. 407).

Whewell's aim was to make the first steps towards unravelling the empirical laws of the tides, before making any assertions about the (contributing) causes producing the tides. In one of his notebooks (1835), he wrote that the tides 'are not a normal specimen because the general course is known' (i.e. the primary cause by the force of gravity), but still we have 'to trace the <u>laws of phenomena</u> [i.e. the local contributing causes] as if the <u>laws of causation</u> were not known—and having got laws of certain phenom. we get universal phenom' (WP, R.18.11[14], fols. 37[v], 38[r]). Whewell compared the present state of knowledge of the tides to the pre-Newtonian state in astronomy:

> When we consider the enormous accumulation of observed phenomena and empirical laws which preceded the discovery of the true principles of the heavenly motion, we may easily suppose that we are only on the outset of what we have to do, in order to obtain the same success with regard to the tides. (Whewell, 1834b, p. 40)

As we have seen, Whewell stated in his notebooks on induction that the establishment of empirical laws precedes the investigation of causes. In the *Philosophy of the inductive sciences* he upheld the same distinction between empirical and causal inductions: scientific laws are either empirical laws, or 'Laws of Phenomena' (which teach us what takes place); or causal laws, or 'Theories of Causes' (which explain why it takes place[34]) (*PIS*, Vol. 5, pp. 95–106, 336; cf. *NOR*, pp. 118–128). Few branches of science are able to unravel the causes of things. In manuscript material dated between ca. 1837 and 1840 relating to the *History of the inductive sciences*, Whewell wrote that in order to tackle the problem of the tides we should

> ascertain by an analysis of long series of observations, the effect of changes in the time of transit, parallax, and declination of the moon, and thus ... obtain the laws of phenomena; and then ... proceed to ↓investigate↓ the laws of causation. (WP, R.18.13[2], fol. 278[v])

Accurate tide tables—the earliest ones were produced for the ports of Liverpool and London—formed the necessary point of departure for establishing a theory of tidal phenomena; only careful observations could yield insight into patterns, and accidental causes could be filtered out by 'taking so great a number of observations', that 'the effects thus produced will depend upon the depth of the ocean, the form of its shores, and other causes, of which it is impossible to estimate the result *à priori*' (Whewell, 1834b, pp. 17–18; cf. p. 43 and also 1836c, pp. 238–336, 290). Initially Whewell was quite optimistic in this respect (cf. Becher, 1991, p. 14) as can be seen from a letter concerning the tides in the port of London where he wrote to John W. Lubbock in 1833:

> I shall get formulæ which will represent your tables very well, and I am persuaded that I can calculate tide tables from my formulæ, which will agree with observation as well as any extant tables or better. (Whewell to J. W. Lubbock, 31 October 1833, *CW*, Vol. 16, p. 169)[35]

Likewise, in his first paper on the tides (1833), Whewell noted:

> If, with the opportunities which now exist, observations are for the future made with due attention to the circumstances of real importance, we may in *a very few years* be able to draw a map of cotidal lines[36] *with certainty and accuracy*. (Whewell, 1833, p. 148; my emphasis)

[32] Although I cannot offer here a detailed chronology of Whewell's papers on the tides, the following subsection is, I claim, representative of his tidology. I shall focus on those aspects that are most relevant to the claims I seek to argue for.

[33] In an undated newspaper article reporting on a lecture of Whewell at the Bristol Institution, an anonymous journalist wrote: 'By this means [tide tables and cotidal lines] the phenomena are *discussed*, and the rules which they follow extracted from them; and this investigation exemplifies on important step in science, which may be called the *determination of phenomena*. Next follows a higher step,—the *determination of causes*. The causes of the phenomena, in this case, we do not doubt to be the attraction of the sun and moon; but to trace from the theory the effects of these causes, and to show that it agrees with the detail of the phenomena, is a task so complicated and arduous, that it has not been executed' (WP, R.6.20[6]).

[34] Whewell later gave the following examples of causes: substance, force and polarity (*NOR*, p. 247, Aphorism LXI).

[35] This was shortly after the publication of Lubbock (1832). The paper was read on 17 November 1831.

[36] Whewell probably derived the expression 'cotidal lines' from Thomas Young's 'contemporary lines', and suggested the term in 1831 to Lubbock (Reidy, 2008, pp. 162, 194; Deacon, 1971, p. 258; Marmer, 1928). Alexander von Humboldt's work on isothermal lines (Humboldt, 1817) might also have been a source of inspiration. For a discussion of the significance of Whewell's graphical method in statistics and economy, see Maas & Morgan (2002). Cotidal lines thus represented the ridge of the tide-wave at a place, that is, that protuberance of the water upon the surface of the ocean which moves along the seas and brings high tide (and low tide) at the time the elevated (or depressed) parts reach that place (Whewell, 1833, p. 149). Correspondingly, cotidal lines at successive moments represent the successive positions of the tide-wave. The great advantage of such lines is that they could trace the general patterns of the tides that are not easily traceable from tide tables alone.

In his paper on the tides at Liverpool (1836), he noted that the obtained measures pointed directly to a very simple law of the tides, 'namely, that the tide at any place occurs in the same way as if the ocean imitated the form of equilibrium corresponding to a certain antecedent time' (Whewell, 1836a, p. 2). It is highly likely, Whewell suggested, that other ports 'might be represented in a similar manner' (ibid., p. 6). Equilibrium-theory thus expresses 'with very remarkable exactness, most of the circumstance in my results' (ibid., p. 2; cf. Whewell, 1838b, p. 233); moreover, he added,

> notwithstanding the great irregularities to which the tides are subject, the results of the *means* of the large masses of good observations agree with the formulæ with a precision not far below that of other astronomical phenomena. (Whewell, 1836a, p. 2)

In 1837, he claimed to have established 'a rule, based on equilibrium theory, agreeing with the observations to an extraordinary degree and precision', and also that in some cases the '[diurnal] inequality assumes a very remarkable form, so as materially to disguise the general circumstances of the tides, and to explain other causes in which the usual features are entirely obliterated' (Whewell, 1837a, p. 75).

In his 1834 paper on the tides at the port of London and his 1836 paper on the tides at the port of Liverpool, Whewell tried to determine how the time of high-water and the height of the water are affected by the declinations and parallaxes of the sun and moon by equilibrium-theory. Equilibrium-theory states that the attractive forces of the sun and moon cause the ocean to approach the shape of a spheroid with its major axis (approximately) aligned so that the greatest elevations of water occur one below the moon and the other on the opposite side of the earth (Deacon, 1971, pp. 252–253). Here he relied on Lubbock's 1832 paper, in which Lubbock had found a formula that accounted for the lunar inequalities: that is, for both the correction of the moon's declination (the semi-menstrual inequality), owing to the moon's changing angular distance from the earth's equator (which depends on the moon's distance from the sun), and the correction of the moon's parallax, owing to the fact that the moon's distance from the earth is not constant[37] (Whewell, 1834b, pp. 19–27). Also the time of high-tide does not follow the moon's transit by the same interval at every period of lunation. Therefore, Lubbock and Whewell did not consider the commonly used vulgar establishment: the time of high-water on the day of new and full moon at a place. They considered instead the corrected establishment: the average of all time-intervals between high-tide and the moon's transit for any whole numbers of a half-lunation, that is, the period required for the moon to pass from a position of maximum angular distance north of the equator to a position of maximum angular distance south of the equator (or vice versa; Whewell, 1833, p. 163).[38] The corrected establishment is thus basically the vulgar establishment freed from the age of the tide and the lunar inequalities.[39] Lubbock's equation for the semi-menstrual inequality stated the following relation: $\tan 2\ (\theta' - \lambda') = -[h\ \sin 2\ (\varphi - \alpha)/h'\ \cos 2\ (\varphi - \alpha)]$, where λ' is the mean interval of the tide and transit, θ' is the correct interval, φ is the solar time of the moon's transit, α is a constant quantity (for London 2 hours) and h and h' are the elevations of the spheroid due to the moon and the sun. This formula assumes that the waters of the ocean approach nearly the form in which they would appear in equilibrium under the action of the sun and the moon, and that the pole of the fluid follows the pole of the spheroid of equilibrium at a certain distance (Whewell, 1834b, p. 35). It also assumes that the earth and the moon are at rest (WP, R.6.20[24], fol. 102[r]). Whewell then went on further to incorporate the solar corrections (and the heights of high tide) (Whewell, 1834b, p. 34; Reidy, 2008, p. 152).[40] The tide tables for Liverpool and London suggested a confirmation of Whewell's formulae (Whewell, 1834b, p. 34; 1836b, p. 131). The results showed, Whewell claimed, that

> notwithstanding the great irregularities to which the tides are subject, the results of the *means* of large masses of good observations agree with the formulæ with a precision not far below that of other astronomical data. (Whewell, 1836a, p. 2)

By comparing the initial results of the port of London with those of the port of Liverpool, the formula for the port of London could further be tested and improved on (ibid., pp. 1–2). More precisely, Whewell had shown that the tides at the ports of London and Liverpool subsumed under the same mathematical formula, once different constants for λ' and α are filled in. Such constants (magnitudes and epochs) are not derived from equilibrium theory and can only be established empirically as they differ from location to location (Whewell, 1837a, pp. 76–77). This put an end to the hope that tide tables for one port could be used to determine the tides at another port by simply adding or subtracting a constant interval (Reidy, 2008, p. 175).

Once Whewell had collected tidal data for several coasts in Great Britain and Ireland he moved on to acquire the times and heights of high- and low-water on a global scale. To that end, directions were given to make observations throughout the world—in a research paper published in 1836 he reported on 'this large experiment' (Whewell, 1836c; cf. 1834a). These observations were, as Whewell stressed, made for the most part 'under the direction of intelligent officers and men of science' (Whewell, 1836c, p. 289). His aim was to deduce corrected cotidal lines, that is, lines that connect places at which high-tide occurs at equal times, from this vast collection of data. For the reduction of the data he had used, according to his own testimony (Whewell to Herschel, 10 June 1836, *CW*, Vol. 16, p. 242), John Herschel's Method of Graphical Interpolation[41] and the Method of Means. The obtained data however showed that the cotidal lines of the North Sea are much distorted, resulting in rotary systems of tide-waves—a further sign that equilibrium-theory alone was insufficient (Whewell, 1836c, p. 298).[42] Moreover, the results for the Atlantic were so complicated that he abandoned any attempt to trace cotidal lines for the oceans as a whole (see Figs. 1 and 2) (Deacon, 1971, p. 263; Reidy, 2008, p. 181). Around that period Whewell wrote to Herschel: 'the longer I attend to the subject [i.e. the tides], the more cautious I become in generalising' (Whewell to Herschel, 4 December 1836, *CW*, Vol. 16, p. 247). Instead of trying to correct the cotidal lines any further, he started a worldwide study on the diurnal inequality, that is, the difference between the heights of the two high-tides (or of the two low-tides) on the same day (Whewell, 1837a; Reidy, 2008, p. 208). Calling attention to the daily inequality was of utmost importance:

---

[37] The moon is closest to the earth at perigee where the tidal effect increases, and farthest at apogee where the tidal effect decreases. Similar effects occur when the earth is in aphelion and perihelion.

[38] Whewell took over this term from Lubbock, who derived it from Lalande's *établissement* (Cartwright, 2001, p. 111). Cf. Whewell (1834b), p. 19; (1836b), p. 131; (1840c), p. 256.

[39] Lubbock's original formula did not include the correction for the moon's parallax.

[40] In a letter to David Forbes on 7 March 1836, Whewell wrote on the solar corrections: 'This almost completes the list of corrections, and I have also been tolerably successful in showing their connection with the forces; but this problem remains to be solved as one of hydrodynamics' (WP, O.15.47[48(2)]).

[41] On this matter see Hankins (2006). This method is identical to the Method of Curves (*PIS*, Vol. 5, p. 399).

[42] This was already implied by the data gathered in Whewell (1835). There he concluded that the discrepancies between theory and observation 'make it clear that we cannot correctly use the tide tables of one place to determine the tides of another, by adding or subtracting a constant interval, as is often done' (ibid., p. 86).

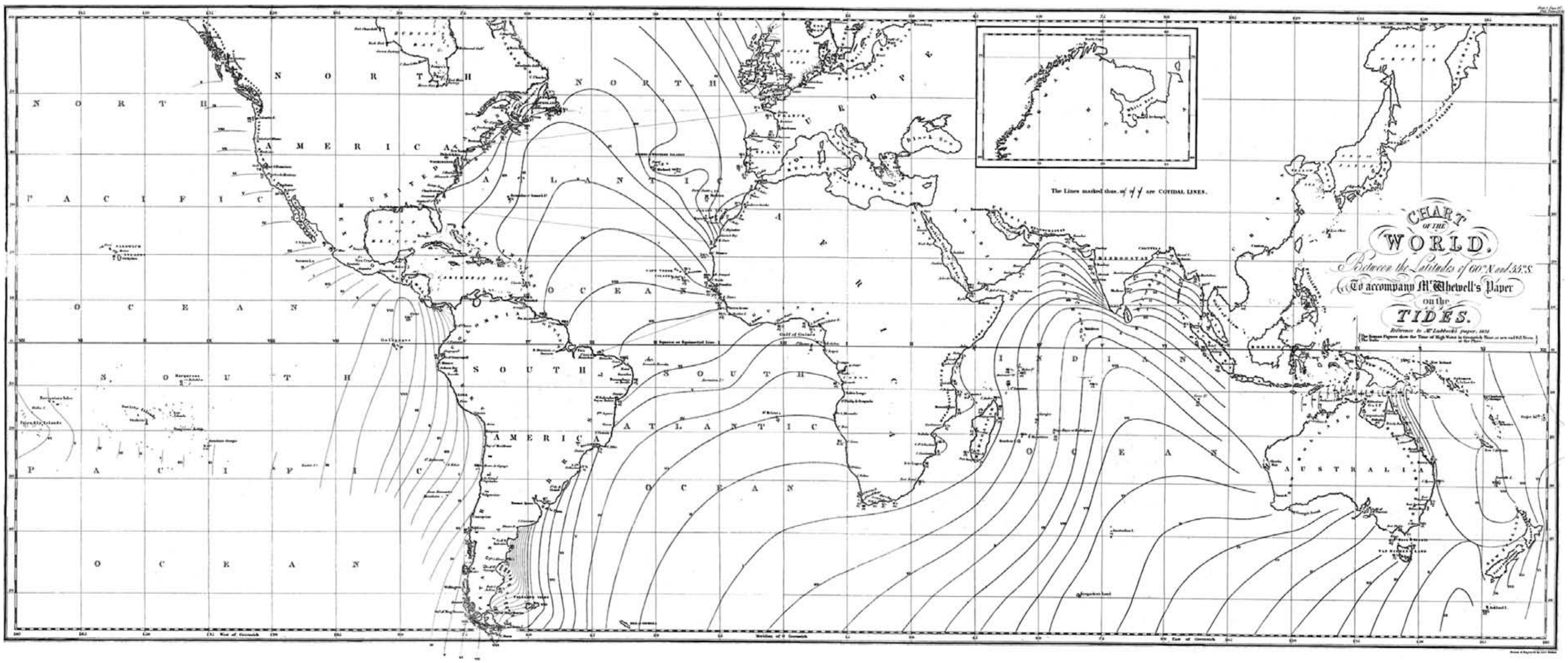

Fig. 1. Given the shape of the cotidal lines and the fact that the cotidal lines in the East of the map occur earlier than those in the West, a progression of waves is suggested in a North-Westerly direction from New Zealand all the way up to Norway. In his *De fluxu et refluxu maris* (ca. 1611) Francis Bacon had suggested a global progressive tide moving in a northwardly direction (Snyder, 2002, p. 82). Cotidal lines had enormous predictive powers as they predicted not only tidal patterns along shores but also in the deep ocean. Map taken from Whewell (1833). Courtesy of The Royal Society.

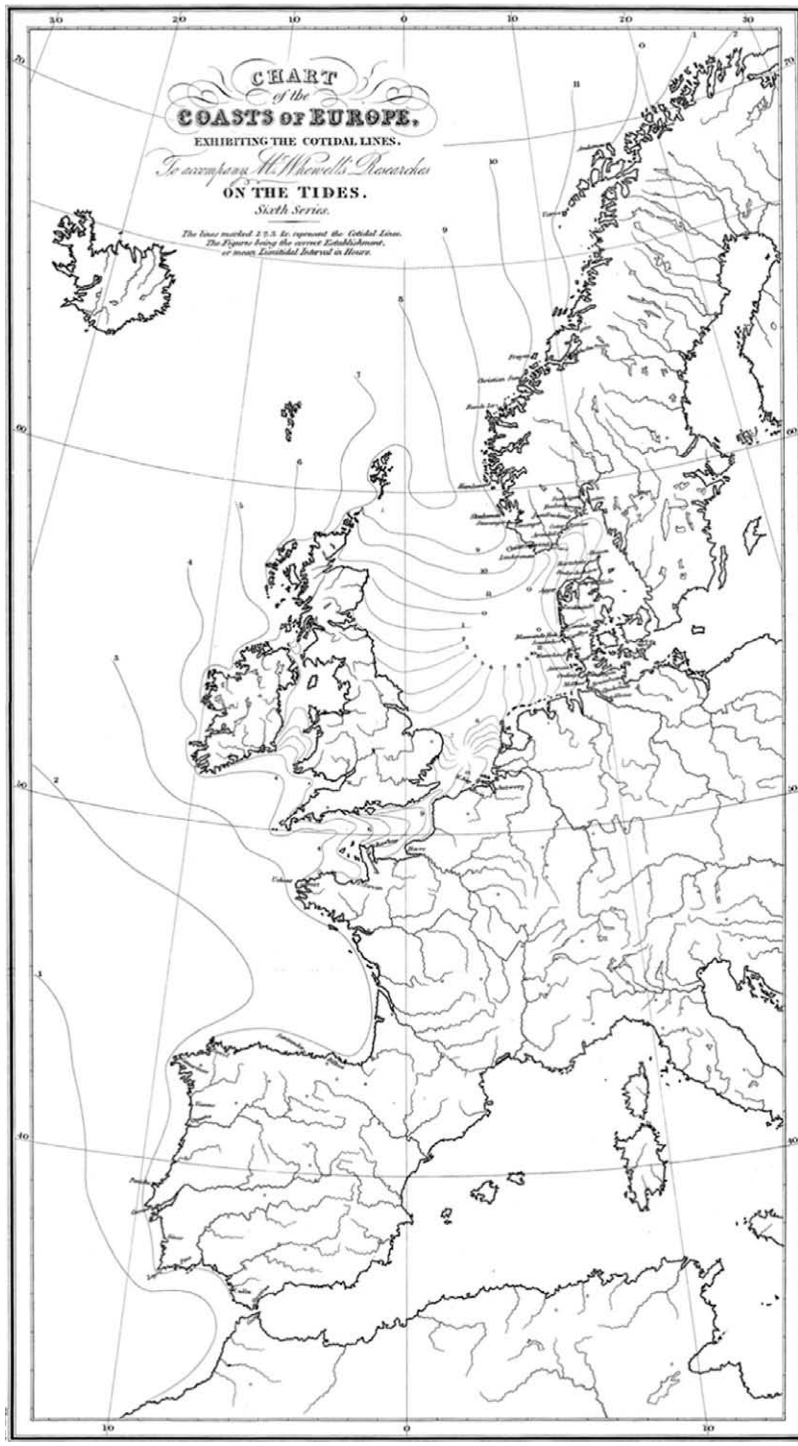

Fig. 2. In view of the presence of complex rotary systems (here depicted for the 'German Sea', as the North Sea was called at the time), Whewell rejected his progressive wave interpretation. Rotary systems implied that at the centre of such a system there was *a point of no tide*. Such a *point of no tide* was found in 1840 near the centre of the smallest rotary system on the map (*CW*, *HIS*, II, p. 471). Whewell never published a corrected global cotidal map. Map taken from Whewell (1836c). Courtesy of The Royal Society.

by emphasising its locality, Whewell made it clear that local observations were badly needed (compare his statement that 'The peculiarities of the tides in each country are such as to make each shore a study by itself' (Whewell, 1837b, p. 233). Therefore, he called attention to the need to make tidal investigations 'a national work in civilised maritime states' so that 'our best generalisations will be collected from results obtained in separate parts and combined' (ibid).[43]

[43] Needless to say, understanding the seas in the Victorian age was of vital economical, geo-political, and strategic importance. This is masterfully documented in Reidy (2008).

4.2. The 'construction of science'

Whewell's method of reducing tidal phenomena into more manageable components nicely fits with his methodological views as described in Book XI, entitled 'Of the construction of science', of Volume 2 of the *Philosophy of the inductive sciences*. From the Fundamental Antithesis of Philosophy,[44] which states that knowledge always involves both 'Thoughts and Things',[45] it follows that the establishment of proper, that is, scientific, knowledge is based on 'clear and certain facts' on the one hand and on appropriate conceptions that are applied to those facts on the other hand (*PIS*, Vol. 4, p. 4). The progress of science, according to Whewell, was only possible by the fruitful combination of metaphysics and experience: 'the metaphysical is a necessary part of the inductive movement' (ibid., p. ix). Consequently, the progress of science has its place in observation, in appropriate ideas (which regulate our active operations of the mind; ibid., p. 66), and in the union of the two (ibid., p. ix). The starting point of the process of induction is, according to Whewell, the decomposition of facts:

> We resolve complex appearances which nature offers to us, and the mixed and manifold modes of looking at these appearances which rise in our thoughts, into limited, definite, and clearly-understood portions . . . The Decomposition of Facts into Elementary Facts, clearly understood and surely ascertained, must precede all discovery of the laws of nature. (Ibid., Vol. 5, pp. 33–34)

This process results in the introduction of technical terms by which such 'Elementary Facts' are described (e.g. altitude, declination, refraction, etc.) (ibid., p. 34; *NOR*, pp. 257–345). Once we have settled the terminology we can begin measuring such decomposed facts. And indeed, when studying the tides, Whewell reduced the complex motion of the oceans (and the relevant changes accompanying it) into less complex and more easily determinable components, such as the heights and times of high-water or the positions and motion of the sun and the moon. To such elementary facts technical names were given ('vulgar' or 'corrected establishment', 'parallax', or 'declination') and they were subsequently measured at specific ports at specific times. The next steps in the formation of science are the Explication of Concepts and the Colligation of Facts, respectively (*PIS*, Vol. 5, p. 5; cf. *NOR*, pp. 29, 30–49, 59–69). The former refers to the fact that Ideas (such as the ideas of space, time, cause, number, etc.) are transformed into special modifications, so called 'conceptions', of those ideas (such as force, circle, squared number, genus, etc.) which are then applicable to particular facts (*PIS*, Vol. 5, pp. 5–6). In this process we clarify our ideas, that is, we render them more concrete (ibid., p. 18). In the course of scientific research we try to 'unfold' conceptions 'so as to bring into clear view the elements of truth with which they are marked from their ideal origin' (ibid., p. 6). Colligation of Facts occurs when several separate facts are bound together by the same conception (ibid., p. 36; cf. *NOR*, p. 70, Aphorism VIII).[46] Induction is not merely the sum of the individual facts (as Mill would claim): known facts are seen from a novel point of view, which did not exist in any of the observed facts previously (*PIS*, Vol. 5, pp. 49, 85; cf. *NOR*, pp. 71–72 and also *PD*, p. 20). When Kepler discovered the elliptic orbit of planets, for instance, he applied the concept of ellipse to the motion of Mars. As Whewell put it, a conception is *super-induced* upon the facts (*PIS*, Vol. 5, p. 50; cf. *NOR*, p. 74). As we have seen, Whewell was particularly interested in unfolding the connexion between tidal observations. A 'cotidal line' was the conception by which he hoped that tidal observations could be bound together.[47] While the under-labourers merely collected tidal data, the 'scientists' colligated them.

4.3. Theory-testing and confirmation

Although there is no doubt of Whewell's immense appreciation of Lubbock's work, he wanted to go beyond what Lubbock was doing (Reidy, 2008, pp. 130–133, 152, 165, 167). Instead of pursuing long-time local observations, Whewell wanted to obtain short-time comparative simultaneous data from around the world, which could then serve as a means to establish plausible theoretical generalisations. Confronting theory with data was a vital goal of his tidal research (Ruse, 1976, p. 233). According to Whewell, tidal theories had not been properly tested: 'the laws which these methods imply have not yet been compared with theory' (Whewell, 1834b, p. 16 n.). Whewell endorsed the view that in our attempt to explain the tides, we should combine 'the hydrostatic effect of the currents with the laws of transmitted undulations' (Whewell, 1833, p. 227).

Hypotheses are easily devised, but not easily confirmed. The process of testing and confirming a theoretical hypothesis takes decades, if not centuries (e.g. universal gravitation). Without proper testing and verification, a colligation of facts has only the status of a hypothesis (*PIS*, Vol. 5, p. 44). According to Whewell, testing hypothesis is a step-by-step process:

> we resolve the most general truths in to their constituent parts; and these again into their parts; and by testing, at each step, both the reality of the asserted ingredients and the propriety of the conjunction, we establish the whole system of truths. (Ibid., p. 80)

If we are successful in this respect our hypothesis has been 'penetrated, infiltrated, and metamorphosed by the surrounding medium of truth, before the merely arbitrary and erroneous residuum has been finally ejected out of the body of permanent and certain knowledge' (Whewell, 1856, p. 146; this paper was presented in 1851). When testing the equilibrium-theory Whewell proceeded in exactly this way: the formula for the semi-menstrual inequality devised by Lubbock and the cotidal lines were based on equilibrium-theory, and thus could serve as an indirect way to confirm or falsify equilibrium-theory. In his first tidal reports, Whewell set out to confront this formula with observation. When it was confirmed from the observations of the port of London, he immediately went on to test it for the port of Liverpool. The generalisations for both ports matched, and thus a consilience of inductions was established that gave Lubbock's formula and equilibrium-theory extra credit. The ability of a hypothesis to provide consilience of inductions is a test of its truth, according to Whewell (Laudan, 1981; Fisch, 1985b). In the history of science only two theories displayed an extraordinary capacity to establish (the strong version of) consilience of inductions: universal gravitation and the wave theory of light (*HIS*, Vol. 2, pp. 310, 328, 341, 429, 459, 464; Vol. 3, p. 22).

Whewell distinguished between two versions of consilience of inductions: (1) a *strong version*, which refers to the unification (or 'jumping together') of two inductive generalisations involving classes of facts of *different kinds*; and (2) a *weaker version*, which

[44] Since it is not my aim here to provide a new interpretation of the contents of Whewell's philosophy in general, I shall not deal with this issue *in extenso*. However, it might be pointed out that I have provided a novel interpretation of Whewell's ideas on the relation between Fundamental Ideas, Axioms and Scientific Laws in general, and his concept of necessity in particular, in Ducheyne (Forthcoming b,2009). On Whewell's philosophy, see Butts (1965), Fisch (1985a, 1991b), Morrison (1997), Snyder (1994, 1999, 2006).

[45] Cf. 'Without Thoughts there could be no connexion; without Things, there could be no reality' (*PIS*, Vol. 4, p. 18).

[46] Whewell's attempt at 'tidal colligation' is discussed in Reidy (2008), pp. 193–194, 243–245.

[47] Cf. Reidy's claim that for Whewell a cotidal map was a 'unifying tool' (Reidy, 2008, p. 166; cf. pp. 193–194).

refers to the unification of two inductive generalisations involving classes of facts of the *same kind* (*PIS*, Vol. 5, p. 65). The jumping together of the data obtained for the ports of London and Liverpool constituted a consilience in the weak sense, as both were based on littoral data. A consilience of the strong type would refer to the jumping together of littoral and oceanic data—a requirement shown to be impossible in Whewell's later tidal research. Proper scientific theories are the result of such a process of successive generalisations, and tend increasingly toward simplicity.[48] However, as it turned out, Whewell's cotidal lines were rendered doubtful in view of the worldwide observations on which he later reported. In manuscript material dated between ca. 1837 and 1840 relating to the *History of the inductive sciences*, Whewell recorded: 'The hypotheses which connect facts in space are most easily devised; yet even then are often difficult. but are needed. Ex. Cotidal Lines' (WP, R.18.10$^{20(3)}$, fol. 8^{r}). In the same manuscript material he wrote: 'The fear of hypothesis leads to inaction: the better philosophy leads to such experiments as may show what is the true hypothesis' (ibid., fol. 8^{v}). As an obvious sneer at Newton's *hypotheses non fingo*, he noted 'Hypotheses may be framed to connect measured phenomena' (ibid., fol. 11^{r}; *NOR*, p. 82). According to Whewell, clearly conceived hypotheses can be useful to arrive at 'the true rule', that is, the rule that is consistent with *all* observed facts (*PIS*, Vol. 5, p. 60; cf. *HIS*, Vol. 1, p. 141). This, as we have seen, was frequently brought up by Whewell's tidal papers. In his *Philosophy of the inductive sciences* he stressed the importance of conjectural leaps in scientific practice:

> To try wrong guesses is, with most persons, the only way to hit the right. The character of a true philosopher is, not that he never conjectures hazardously, but that his *conjectures are clearly conceived,*[49] *and brought into rigid contact with facts*. (*PIS*, p. 55; my emphasis; cf. *NOR*, p. 80)

He added to this that a true philosopher should 'abandon his invention as soon as it appears that it does not agree with the course of actual occurrences' (*PIS*, Vol. 5, p. 56). This is exactly what Whewell did in his post-1836 reports on the tides, and more explicitly in his Bakerian Lecture (see below).[50]

Besides collecting and generalising data (which results in establishing empirical laws), Whewell was mostly interested in comparing obtained data with theory and in potentially establishing the correct theory. With respect to the theoretical apparatus required to tackle the problems of the tides, Whewell noted that there were two different approaches available: (1) equilibrium theory as developed by Newton, and especially Bernoulli, who 'have assumed the form of the fluid spheroid, under the influence of the sun and moon, to be the form of equilibrium' (Whewell, 1834b, p. 16); and (2) oscillatory theory as developed by Laplace, who has treated 'the tides as a problem of the oscillations' (Whewell, 1833, p. 147) (while supposing the whole globe to be covered with water of a uniform depth). On the former option he noted that the waters of the seas cannot be considered at rest, 'and therefore the form of the surfaces is not that of equilibrium' (ibid., p. 218; cf. Whewell, 1836c, p. 304), and that Laplace's theory is undoubtedly

> the correct view of the real operation of the forces;[51] but it does not appear that in this way he has obtained any consequences to which Newton's mode of considering the subject did not lead with equal certainty and greater simplicity. (Whewell, 1833, p. 147)[52]

'[I]t is physically, not only possible', wrote Whewell,

> but certain, that each oscillation in each series is affected by those which precede it in the same series, and affects those which succeed it, so that their relative magnitude is different from what it would otherwise be. (Whewell, 1834b, p. 43)

Although mathematicians (including Laplace) have tried to show that *some* laws of fact agree with the measurements predicted by theory, no one has so far shown that 'the general course of the effects produced upon the tides, by the changes of position and distance of the heavenly bodies is such as, according to the mathematical reasoning, it should be' (ibid., p. 17).

Whewell was quite aware that the standard equilibrium theory was 'not the true theory, but a very inaccurate and insufficient substitute for it, which we are compelled to adopt in consequence of the extremely imperfect state of the mathematical science of hydrodynamics' since the 'tides are a problem of motion, not of equilibrium of fluids; and we can never fully explain the circumstances of the phenomena till the problem has been solved in its genuine form' (Whewell, 1836b, p. 134; cf. 1838b, p. 233).[53] Nevertheless, using an incorrect though clearly conceived working hypothesis that is at least to some degree based on observation, could be useful to suggest a better one (Whewell to Airy, 18 January 1843, *CW*, Vol. 16, p. 307). In 1836 he wrote:

> The laws of the tides, thus empirically obtained, may be used either as tests of the extant theories, or as suggestions for the improvement of those portions of mathematical hydraulics on which the true theory must depend. (Whewell, 1834b, p. 19)

The problem of the tides is a problem 'not of hydrostatics, but one of hydrodynamics. But the extreme difficulty of a hydrodynamical problem of such complexity and generality, as this must be, long frightened analysts away from it.' (WP, R.18.10^{4}, fol. 5^{r}).

Let me now clarify why Whewell thought that pursuing equilibrium-theory rather than hydrodynamics as it stood was the best option to track the true theory. This point has often baffled scholars. For instance, Micheal Ruse noted that:

> To be honest, the reason why Whewell took this course [of opting for equilibrium-theory] was probably in major part due to personal inadequacy; he admitted that he lacked the mathematical skills demanded by the hydrodynamical approach. (Ruse, 1991, p. 96; cf. 1976, pp. 235–236)

[48] This was also highlighted in Whewell's historical studies (cf. his 'Inductive Tables' (*HIS*, Vol. 1, pp. 10–11)).

[49] Whewell, for instance, noted that Hipparchian epicycles and eccentrics are clearly conceived hypotheses, as they provide a resolution of the apparent motions of the heavenly bodies (*HIS*, Vol. 1, p. 140). Without the strict examination and successful analysis of the apparent motions of the celestial bodies, the real arrangement would not have been discovered (*HIS*, Vol. 1, p. 143).

[50] In *Novum organum renovatum* Whewell later recorded that a scientist 'allows no natural yearning for the offspring of his own mind to draw him aside from the higher duty of loyalty to his sovereign, Truth' (*NOR*, p. 81).

[51] In his first tidal paper, Whewell considered several 'derivative waves' (and their interference) produced by the presence of islands, bays, sea arms, and canals. These derivative waves were not 'affected at all by the direct action of the sun or the moon' (Whewell, 1833, pp. 150–156).

[52] Laplace's tidal computations were very laborious. On his tidal research, see Reidy (2008), pp. 50–56.

[53] On the occasion of Whewell's obtainment of his royal medal, an anonymous composer wrote: 'The present state of theoretical hydrodynamics throws very little light upon the causes of these curious phænomena. In order to see the mechanical reasons for the forms and distribution of the cotidal lines, it would be necessary to solve the problem not only of the motion of a wave in a *canal* of variable depth, but also in a *basin* of variable depth and given form, a problem hitherto unattempted' (Account of Mr Whewell's researches of the tides, 1838, p. 253).

This ad hoc explanation holds no grounds, for it can be shown that Whewell's preference for equilibrium-theory was motivated by his views on theory testing. Neither is Ruse's observation correct that Whewell 'did not extend his discussion of models, for he gave no real guide-lines for when they should be used and when abandoned, and which model rather than another should be preferred' (Ruse, 1976, p. 235).[54] Whewell never thought that Laplace's account is appealing since it involves laborious computations; but worse, the hypothesis on which his solution is based affects the results 'so as to make them differ altogether from those of the real case' (Whewell, 1833, p. 35). Margaret Deacon commented as follows on Whewell's preference for equilibrium theory:

> He did not however believe that Laplace's work as it stood offered a way of explaining the tides at large that did the Newton–Bernoulli equilibrium theory. Apart from his tidal and other original studies Whewell was one of the first historians of science and he understood that a hypothesis which could be examined empirically was likely to be a more rewarding field of inquiry than a theory which could not develop, even though he felt that the ultimate answers must lie in that direction. (Deacon, 1971, p. 258)

Whewell noted that 'Tide tables were never, I believe, calculated upon Laplace's theory, and thus never fairly brought to the test' (WP, R.18.13^2, fol. 278^v).[55] Furthermore, Laplace's theory rests on 'arbitrary hypotheses' (Whewell, 1836b, p. 134): the supposition that the earth is uniformly covered with water and hence does not take into account the existence of continents (by contrast, Newton's theory leaves the depth of the oceans open); neither does it enable us to collect from it anything about the depth of the motion (moreover, it remained unclear what the mechanical principle is by which the tides are dependent on the depth of the ocean) (Whewell to Lyell, 5 March 1835, *CW*, Vo. 16, p. 207). Laplace also introduced the precarious assumption that in a system of bodies, in which periodical forces act, the state of the system is periodical like the forces (*HIS*, Vol. 2, pp. 92, 195). Later, in a letter to Airy in 1843, Whewell added that Laplace's theory thus required 'some general conjectural reasoning to bridge over the gap between the mathematical hypothesis and the case of nature' (Whewell to Airy, 2 March 1843, *CW*, Vol. 16, p. 311). Snyder has rightfully called attention to an important, and often neglected, aspect of Whewell's account of theory-testing: haphazardly framed hypotheses—that is, hypotheses that are by no means inferable from the data at hand—cannot pass as candidates for Whewell's consequentialist confirmation tests (prediction, consilience, and coherence) (Snyder, 1997a, pp. 598, 585–588; 1997b, pp. 167–176; 2006, pp. 171–175).[56] A hypothesis worthy of consequentialist testing should (1) be *clearly conceived*, that is, it should resolve the phenomenon involved into limited and definite portions (*PIS*, Vol. 5, p. 33), and *inferable from* the actual observations, that is, 'not connected with them [actual observations] by other arbitrary and untried facts' or, in other words, 'close to the facts' (ibid., p. 276, cf. p. 387; cf. *NOR*, p. 183); furthermore, it should (2) yield a *colligation*, derived from certain Fundamental Ideas, which binds these observations together while assigning a common property to them (*PIS*, Vol. 5, p. 45; cf. *NOR*, pp. 67–68). Furthermore, such property should also be projectable to yet unobserved facts—for an adequate hypothesis should explain all phenomena (*PIS*, Vol. 5, p. 62). For Whewell scientific knowledge involved the combination of inductive discovery and deductive justification (Ruse, 1976, p. 231).[57] On Laplace's account neither of the conditions mentioned above obtain: his account is not 'close to the facts' (it simply explains the form and depth of the seabed away, instead of attempting to account for such factors) nor does it make any predictions about new phenomena/evidence (Laplace had *accounted* for some observations at Brest, but had not *predicted* new data) (*HIS*, Vol. 2, p. 191). Hence, Whewell's criticism of Laplace's theory: it did not make predictions of phenomena we have not yet observed, that is, it did not have forward-looking capacity.[58] Newton's theory of universal gravitation has such capacity:

> it 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. (Ibid., p. 136)

A fruitful hypothesis should be able not only to explain the facts we hitherto observed, but also to foretell phenomena that have not yet been observed (ibid., p. 62).[59] Note also that equilibrium-theory is a corollary of universal gravitation, the most severely tested and confirmed theory in the history of science—obviously a theory Whewell would not easily give up on. As tidology was a part of physical astronomy, it was inconceivable for Whewell not to cast the problem of the tides in its genuine Newtonian framework (while leaving open the possibility that other theoretical elements needed to be added in order to get at the details of tidal phenomena).

By contrast, equilibrium-theory rests on the assumption that 'a fluid will always tend to the condition of equilibrium, though the circumstances of the case prevent its ever reaching that condition; a very just and reasonable assumption' (Whewell to Airy, 18 January 1843, *CW*, Vol. 16, p. 307; cf. WP, R.18.13^2, fol. 280^r). So while equilibrium-theory supposed a *tendency* towards equilibrium (*HIS*, Vol. 2, p. 195; a tendency that could be and in fact is disturbed by

[54] In his doctoral dissertation, Reidy explains Whewell's preference for equilibrium-theory by pointing to his views on the history of astronomy: in contrast to Laplace's account, Newton's theory allowed the construction of (tide) tables, an activity that was crucial in the sequel of inductive epochs (Reidy, 2000, pp. 374–375). While the explanation Reidy gives is basically correct (for example *HIS*, Vol. 2, pp. 161, 194), it contains but a part of the answer: Whewell's philosophy of science, as I show in what follows, also needs to be taken into account in the explanation of Whewell's preference for equilibrium-theory.

[55] Although Laplace compared his theory with observation, he never made predictions directly from theory (Reidy, 2008, p. 53). Whewell had already made this complaint in 1833 (Whewell, 1834a, p. 665).

[56] Coherence refers to the capacity of a (worthy) hypothesis to bind together observations without ad hoc modifications of the theory.

[57] Although Whewell distinguished between the initial moment of generation of a scientific proposition and the later moment of justification, he seems to have opposed the separation between the context of justification and the historical and psychological origins of such proposition (Schickore, 2006, p. 62).

[58] That theory-selection is a forward-looking enterprise is the main lesson of Newton's fourth rule of philosophising (Schliesser, 2005). Whewell commented on Newton's fourth rule of philosophising in *PD*, pp. 196–198; and *PIS*, Vol. 5, pp. 291–292. He noted: 'The realy [sic] valuable part of the Fourth Rule is that which implies that a constant verification, and, if necessary, rectification, of truths discovered by induction, should go on in the scientific world' (*PIS*, Vol. 5, p. 291).

[59] Cf. '*It is a test of true theories not only to account for, but to predict phenomena*' (*NOR*, p. 70, Aphorism XII).

additional parameters[60]), Laplace's theory assumed an unrealistic idealisation: that the earth is uniformly covered by a world ocean. Whewell was, however, doubtful whether equilibrium theory alone could provide the correct theoretical apparatus for tidal phenomena: he believed that equilibrium theory '*in conjunction with the laws of waves*, so far as we knew those waves' could result in a truer theory (Whewell to Airy, 22 February 1843, *CW*, Vol. 16, p. 309). In a paper entitled 'An essay on the theory of the tides' that was read on 11 November 1839, he noted:

> It is well known that though the *equilibrium theory* of the tides ↓as given by Bernoulli and others, when applied with certain modifications, account[s] for several of the phenomena, yet that taking the general progress of the tides into account ↓this theory↓ is irreconcilable with numerous facts, while at the same time it has no right on any mathematical grounds hitherto adduced to be considered now as an approximation to the truth. (WP, R.6.20[24], fol. 102[r])[61]

In the same year he noted that the first approximation 'has little or no real value' and that the other approximations are mere additions to the first (ibid., fol. 131[r]). In a paper written in 1837, Whewell noted that the novel 'mathematical hydraulics on which the true theory must depend' is yet to be established (Whewell, 1834b, p. 19). For almost a decade Whewell published little on the tides. In a letter to David Forbes in 1838, he wrote that he wished to wrap up his tide papers 'for there really is no end of the work to which they lead', and that as far as the hydro-dynamics involved was concerned he preferred to leave this 'to bolder and stronger mathematicians' (Whewell to Forbes, 2 April 1838, *CW*, Vol. 16, p. 269). In the *Philosophy of the inductive sciences* Whewell noted that tidology can at present not be advanced 'because we cannot solve the requisite problems in the Integral Calculus' (*PIS*, Vol. 5, p. xxiv). According to Whewell, not only was a new mathematical apparatus lacking, but also a new conception by which hydrostatics and hydrodynamics could be combined in a single conception. In a letter to Lubbock on 2 February 1839, it seems that Whewell almost gave up on the tides: 'I myself cannot long continue to give to it the attention which I have long done, and I suppose you must be nearly in the same situation' (WP, O.15.47[227]). In a letter to Lubbock sent seven days later, he announced that he had no immediate intention of writing a 'general view' about the tides, as he admitted that he did not see his way well enough (Whewell to Lubbock, 9 February 1838, *CW*, Vol. 16, p. 277).

In later years, Whewell's scepticism about the theoretical adequacy of his cotidal lines (which were based on equilibrium-theory) became more and more apparent. In his Bakerian Lecture (published in 1848), in which he looked back on his earlier work on the tides, he commented as follows:

> When I wrote my first memoir on the subject, our knowledge of the tides of that ocean [i.e. the Pacific] was so imperfect, that I did not even venture upon a first approximation to the cotidal lines. And I have since seen reason to believe that, not only for that ocean but for all large seas, the method of drawing cotidal lines which I formerly adopted, is very precarious. (Whewell, 1848, p. 1)

Moreover, he added,

> I [now] conceive all attempts to draw such lines *across* a wide ocean by means of observations on its shores, must be altogether worthless. This applies beyond doubt to the Pacific Ocean and probably, taking other reasons into account, to the Atlantic as also. (Ibid., p. 2)

The data at hand had rendered it very implausible that the tides in the Atlantic and Pacific could be conceived to be brought by a progressive wave travelling round the world that follows the moon (as the scheme of cotidal lines assumed) (ibid., p. 5). First of all, cotidal lines might be disturbed, as to obtain a convex form, by 'stationary undulations', that is, free undulations 'depending on the dimensions of the fluid only' (ibid., p. 3).[62] Furthermore, it is possible that a stationary undulation may be produced by cotidal lines revolving round a fixed centre, a so-called 'point of no tide' (ibid., p. 5; Whewell, 1836c, p. 299),[63] or 'amphidromic point'[64] as this was later called. Such 'derivative waves' disturb the cotidal waves (Whewell, 1836b, pp. 149–156). Observations in the Atlantic and in the English Channel showed that no universal pattern in terms of cotidal lines existed (Whewell, 1836c). Cotidal lines must be modified substantially in order to accommodate the phenomena (Whewell, 1848, pp. 3–4). Whewell pointed out that the forms of these cotidal lines are exaggerated 'in order to make them confirm to our observations, so that lines near the shore are made clear and almost parallel to each other' (Whewell, 1836c, p. 294). Correspondingly, Whewell now stressed that they were 'mere *geometrical diagrams*, not lines marking the progress of a wave by motions of the particular perpendicular to the line of the wave' (Whewell, 1848, p. 9; 1851, p. 28). In other words, he broke with his earlier (realistic) stance on cotidal lines. In his first paper on the tides, he had indeed tried to trace the course of the cotidal lines according to which the tide is actually propagated in the ocean (Whewell, 1833, p. 156). On the apparent irregularities of the cotidal lines on the west coast of America, he noted in 1833

---

[60] Whewell compared the tendency of the rate of profits of agriculture and the rate of other employments to balance each other with the tendency to tidal equilibrium: 'Supposing the preceding postulates to be true [i.e. the postulates of equilibrium and price], the problems in which they are applied are much simplified by assuming such an equilibrium to obtain: but along with this simplification we incur a necessary and perpetual, and, it may be, a very considerable deviation from the circumstances of actual fact. In reality, this equilibrium is never attained: probably in most cases it is never approximated ... We are to recollect therefore, that even if our principles were exact, deductions from them made according to the method we are now following, would give us only a faint and distant resemblance of the state of things produced by the perpetual struggle and conflict of such principles with variable circumstances. Such deductions however would probably have some resemblance, in the general outline of their results, to the true state of things. They would offer us a *first approximation*: and in difficult problems of physics, it is precisely by such a simplification as this, that a first approximation is obtained. Thus in investigating of the problem of the tides, we have a very complex case of the *motion* of a fluid: but Newton's mode of treating the question was, to consider what would be the form of *equilibrium* of the ocean, acted upon by the forces which produce the tides: and this solution of the problem, though necessarily inexact, was accepted as the best which could easily be obtained. The investigations of Laplace and others who have since treated the problem on its true grounds, as a question of hydrodynamics, have shewn Newton's solution explains rightly the main features of the phenomenon ... The quantities which we neglect must be of an inferior *order* to those which we take into account; otherwise we obtain no approximation at all. We may with some utility make the theory of the *tides* a question of equilibrium, but our labour would be utterly misspent if we should attempt to consider on such principles the theory of *waves*' (Whewell, 1831a, pp. 166–167). As in tidology, Whewell advised against premature generalisation and promoted the collection of a *substantial* body of political-economical data before theory construction: 'The most profitable and philosophical speculations of Political Economy are however of a different kind: they are those which are employed not in reasoning *from* principles, but *to* them: in extracting from a wide and patient survey of facts the laws according to which circumstances and conditions determine the progress of wealth, and the fortunes of men' (ibid., p. 197; see furthermore Henderson, 1996; Hollander, 1983; Maas, 2005, Ch. 3). While Whewell accepted that in tidology the dominant cause, which delivers the basis for a first approximation, had been successfully established by Newton, in political economy he advised to begin with inductively tracing the dominant cause of wealth.

[61] In this paper he conceived of the motions of the tides as taking place in a basin of uniform depth, and approached them mathematically in terms of vertical slices. The differential equations derived by Laplace proved impossible to integrate without the introduction of some implausible assumptions (Reidy, 2008, p. 53).

[62] Whewell had already used the term 'stationary undulation' in 1839 (WP, R.6.20[24], fol. 109[r]).

[63] William Hewett, by a sounding device of his own making, confirmed Whewell's predicted point of no tide in 1840 (Reidy, 2008, pp. 186–187).

[64] Rollin A. Harris coined this term, meaning 'running' (*dromos*) around (*amphi*-) in ca. 1904 (Cartwright, 2001, p. 121).

that these were at the time the simplest forms he could trace from phenomena, and that they 'may very probably be in reality simpler than they are here represented' (ibid., p. 214; cf. p. 235). Nevertheless, Whewell continued, graphs of cotidal lines remain helpful in ascertaining patterns of regularity, and could assist in obtaining laws from imperfect data (*PIS*, Vol. 4, pp. 396–397; cf. Hankins, 2006, pp. 617–622). Cotidal lines may still be used

> to represent, in the first instance, the results of the tide observations made at a series of places in the same seas; nor does it appear that there can be at present devised any better method of bringing tide observations into geographical combination. (Whewell, 1848, p. 9)

They are intended 'to average out irregularities in the observations and to be able to distinguish the various components of the tides visually' (Robson & Cannon, 1984, p. 183). Whewell concluded his Bakerian Lecture by pointing out that the difficulties with the tides suggest 'the necessity of some new mode of conceiving that motion; a subject which I shall not here pursue' (Whewell, 1848, p. 29). This new mode would be a combination of equilibrium-theory and the theory of transmitted undulations.

5. Conclusion

Summarising, we might say that on an empirical level Whewell attempted to systematise and unify tidal data by means of tide tables and visual modes of representation. On a theoretical and methodological level, he made serious attempts to test how well equilibrium-theory, of whose limitations he became well aware, could be reconciled with extant data.

Here I have argued that: (1) Whewell's tidology and philosophy of science interacted in a fundamental way (which becomes obvious when we focus on issues such as: theory-testing, the use of hypotheses, the criteria of their usefulness, and consilience of inductions), (2) his tidal research was the source of inspiration for his 'Special Methods of Induction Applicable to Quantity', and (3) Whewell's tidal research helped him to develop and refine his philosophical-methodological ideas in a significant way. Of course, that is not to say that only his tidal research did so. Whewell was a many-sided man with varying scientific interests and it is likely that other branches of science contributed to this process as well. As noted above,[65] taking these other disciplines into account cannot be undertaken here, and I leave it to bolder and stronger scholars to point out their significance.

Acknowledgements

The author is grateful to The Master and Fellows of Trinity College, Cambridge for their permission to quote from the Whewell papers; and especially to Jonathan Smith, Archivist at the Wren Library, and his staff, for their kind assistance during a research-stay in Cambridge during the spring of 2008. The author is highly indebted to the audience of the international conference Induction: Historical and Contemporary Approaches (Ghent University, 8–10 July 2008), organised by Erik Weber and myself, for some implicit lessons on matters of presentation of the material discussed in this paper; to Laura J. Snyder for useful suggestions and comments; to Michael S. Reidy, who enthusiastically commented on an earlier version of this paper; and, of course, to the anonymous referees for valuable feedback. Finally, the author kindly acknowledges the Research Foundation, Flanders (FWO-Vlaanderen) for financially supporting his postdoctoral research fellowship.

[65] See n. 14.

References

Manuscripts

WP = Whewell papers, Wren Library, Trinity College, University of Cambridge. (*Convention for the transcriptions*: words between arrows pointing downwards refer to additions inserted from above; words between arrows pointing upwards refer to additions inserted from below. Some notebooks have been numbered as books, others with folios—I have maintained the original pagination/foliation. Unless otherwise specified, all text-editorial features are as in the original. The same holds for all of Whewell's tidal papers referred to.)

Collected works

CW = Whewell, W. (2001a). *Collected works of William Whewell* (R. Yeo, Ed.) (16 vols.). Bristol: Thoemmes.
HIS = Whewell, W. (2001b). *Collected works of William Whewell, Vols. 1–3. History of the inductive sciences* (R. Yeo, Ed.). Bristol: Thoemmes. (First published 1837)
NOR = Whewell, W. (2001c). *Collected works of William Whewell, Vol. 6. Novum organum renovatum* (R. Yeo, Ed.) Bristol: Thoemmes. (First published 1858)
PD = Whewell, W. (2001d). *Collected works of William Whewell, Vol. 7. On the philosophy of discovery* (R. Yeo, Ed.). Bristol: Thoemmes. (First published 1860)
PIS = Whewell, W. (2001e). *Collected works of William Whewell, Vols. 4–5. The philosophy of the inductive sciences* (R. Yeo, Ed.). Bristol: Thoemmes. (First published 1840)

Other texts

Account of Mr Whewell's researches of the tides. (1838). *The Army and Naval Chronicle*, *n.s.*, *6*(1 January–30 June), 252–255.
Airy, G. B. (1841). Tides and waves. In *Encyclopedia metropolitana, Vol. 5* (H. J. Rose, & H. J. Rose, Eds.) (pp. 291–396). London: John Joseph Griffin & Co.
Becher, H. W. (1980). William Whewell and Cambridge mathematics. *Historical Studies in the Physical Sciences, 11*, 1–49.
Becher, H. W. (1991). William Whewell's odyssey: From mathematics to moral philosophy. In M. Fisch, & S. Schaffer (Eds.), *William Whewell: A composite portrait* (pp. 1–30). Oxford: Oxford University Press.
Becher, H. W. (1992). The Whewell story. *Annals of Science, 49*, 377–384.
Bernoulli, D. (1738). *Hydrodynamica, sive De viribus et motibus fluidorum commentarii*. Strasbourg: Johann Reinhold Dulssecker.
Brémontier, N. T. (1809). *Recherches sur le mouvement des ondes*. Paris: Firmin Didot.
Butts, R. E. (1965). Necessary truths in Whewell's theory of science. *American Philosophical Quarterly, 2*, 161–181.
Cantor, G. N. (1991). Between rationalism and romanticism: Whewell's historiography of the inductive sciences. In M. Fisch, & S. Schaffer (Eds.), *William Whewell: A composite portrait* (pp. 67–86). Oxford: Oxford University Press.
Cartwright, D. E. (2001). *Tides: A scientific theory*. Cambridge: Cambridge University Press.
Cauchy, A. (1827). Théorie de la propagation des ondes à la surface d'un fluide pesant d'une profondeur indéfinie. *Académie des Sciences de l'Institut de France, 1*, 1–123.
Darrigol, O. (2003). The spirited horse, the engineer, and the mathematician: Water waves in nineteenth century hydrodynamics. *Archive for the History of the Exact Sciences, 58*, 21–95.
Deacon, M. (1971). *Scientists and the sea 1650–1900: A study of marine science*. London/New York: Academic Press.
Ducheyne, S. (2005a). Newton's notion and practice of unification. *Studies in History and Philosophy of Science, 36*, 61–78.
Ducheyne, S. (2005b). Newton's training in the Aristotelian textbook tradition: From effects to causes and back. *History of Science, 43*, 217–237.
Ducheyne, S. (2006). Galileo's interventionist notion of 'cause'. *Journal of the History of Ideas, 67*, 443–464.
Ducheyne, S. (Forthcoming a). Fundamental questions and some new answers on philosophical, contextual and scientific Whewell: Some reflections on recent Whewell scholarship and the progress made therein. *Perspectives on Science*.
Ducheyne, S. (Forthcoming b). Kant and Whewell on bridging principles between metaphysics and science. *Kant Studien*, *100*.
Ducheyne, S. (2009). Whewell, necessity and the inductive sciences: A philosophical-systematic survey. *South African Journal of Philosophy, 28*, 333–358.
Fisch, M. (1985a). Necessary and contingent truth in William Whewell's antithetical theory of knowledge. *Studies in History and Philosophy of Science, 16*, 275–314.
Fisch, M. (1985b). Whewell's consilience of inductions: An evaluation. *Philosophy of Science, 52*, 239–255.
Fisch, M. (1991a). A philosopher's coming of age: A study in erotetic intellectual history. In M. Fisch, & S. Schaffer (Eds.), *William Whewell: A composite portrait* (pp. 31–66). Oxford: Oxford University Press.
Fisch, M. (1991b). *William Whewell, philosopher of science*. Oxford: Clarendon Press.
Fisch, M., & Schaffer, S. (Eds.). (1991). *William Whewell: A composite portrait*. Oxford: Oxford University Press.

Fourier, J. (1818). Note relative aux vibrations des surfaces élastiques et au mouvement des ondes. *Bulletin des sciences par la société philomatique de Paris*, 129–135.

Galilei, G. (2001). *Dialogue concerning the two chief world systems* (S. J. Gould, Ed.; S. Drake, Trans.). New York: The Modern Library.

Hankins, T. L. (2006). A 'large and graceful sinuosity': John Herschel's graphic method. *Isis, 97*, 605–633.

Henderson, J. P. (1996). *Early mathematical economy, William Whewell and the British case*. Lanham, MD: Rowman & Littlefield.

Herschel, J. F. W. (1831). *A preliminary discourse on the study of natural philosophy*. London: Longmans.

Hollander, S. (1983). William Whewell and John Stuart Mill on the methodology of political economy. *Studies in History and Philosophy of Science, 14*, 127–168.

Humboldt, A. von. (1817). Des lignes isothermes et de la distribution de la chaleur sur le globe. *Mémoires de physique et de chimie de la Société d'Arcueil, 3*, 462–602.

Kelland, P. (1840). On the theory of waves. *Royal Society of Edinburgh, Transactions, 14*, 497–545.

Kelland, P. (1844). On the theory of waves, Pt. 2. *Royal Society of Edinburgh, Transactions, 15*, 101–144.

Laudan, L. (1981). *Science and hypothesis, historical essays on scientific methodology*. Dordrecht: D. Reidel.

Lubbock, J. W. (1832). On the tides of London. *Philosophical Transactions of the Royal Society of London, 122*, 379–416.

Maas, H. (2005). *William Stanley Jevons and the making of modern economy*. Cambridge: Cambridge University Press.

Maas, H., & Morgan, M. (2002). Timing history: The introduction of graphical analysis in 19th century British economics. *Revue d'histoire des sciences humaines, 72*, 97–127.

Marmer, H. A. (1928). On cotidal maps. *Geographical Review, 18*, 129–143.

Morrison, M. (1997). Whewell on the ultimate problem of philosophy. *Studies in History and Philosophy of Science, 28*, 417–437.

Naylor, R. (2007). Galileo's tidal theory. *Isis, 98*, 1–22.

Newton, I. (1979). Opticks; or, A treatise of reflections, refractions, inflections and colours of light. New York: Dover. (First published 1730)

Newton, I. (1999). *The Principia: mathematical principles of natural philosophy* (I. B. Cohen, & A. Whitman, Trans.). Berkeley: University of California Press. (First published 1726)

Palmieri, P. (1998). Re-examining Galileo's theory of tides. *Archive for History of Exact Sciences, 53*, 223–375.

Reidy, M. S. (2000). *The flux and reflux of science: The study of the tides and the organization of early Victorian science*. Ph.D. thesis, University of Minnesota.

Reidy, M. S. (2008). *Tides of history, ocean science and Her Majesty's Navy*. Chicago: University of Chicago Press.

Richards, J. (1996). Observing science in early Victorian England: Recent scholarship on Whewell. *Perspectives on Science, 4*, 231–247.

Robson, R., & Cannon, W. F. (1984). William Whewell, FRS (1794–1866). *Notes and Records of the Royal Society of London, 19*, 168–191.

Ruse, M. (1976). The scientific methodology of W. Whewell. *Centaurus, 20*, 227–257.

Ruse, M. (1991). William Whewell: Omniscientist. In M. Fisch, & S. Schaffer (Eds.), *William Whewell: A composite portrait* (pp. 87–116). Oxford: Oxford University Press.

Russell, J. S. (1838). On the terrestrial mechanism of the tides. *British Association for the Advancement of Science, Annual Report*, 179–181.

Schickore, J. (2006). A forerunner?—Perhaps but not in the context distinction: William Whewell's Germanico-Cantabrigian history of the fundamental ideas. In J. Schikore, & F. Steinle (Eds.), *Revisiting discovery and justification: Historical and philosophical perspectives on the context distinction* (pp. 57–77). Dordrecht: Springer.

Schliesser, E. (2005). Galilean reflections on Milton Friedman's 'Methodology of positive economics', with thoughts on Vernon Smith's 'Economics in the laboratory'. *Philosophy of the Social Sciences, 35*, 50–74.

Snyder, L. J. (1994). It's all necessarily so: William Whewell on scientific truth. *Studies in History and Philosophy of Science, 25*, 785–807.

Snyder, L. J. (1997a). Discoverers' induction. *Philosophy of Science, 64*, 580–604.

Snyder, L. J. (1997b). The Mill–Whewell debate: Much ado about induction. *Perspectives on Science, 5*, 159–298.

Snyder, L. J. (1999). Renovating the *Novum organum*: Bacon, Whewell and induction. *Studies in History and Philosophy of Science, 30*, 531–557.

Snyder, L. J. (2002). Whewell and the scientists: Science and philosophy of science in 19th century Britain. In M. Heidelberger, & F. Stadler (Eds.), *History and philosophy of science* (pp. 81–94). Dordrecht: Kluwer.

Snyder, L. J. (2006). *Reforming philosophy: A Victorian debate on science and society*. Chicago: University of Chicago Press.

Weber, E. H., & Weber, W. (1825). *Wellenlehre auf Experimente gegründet; oder, Über die Wellen tropfbarer Flüssigkeiten mit Anwendung auf die Schall- und Lichtwellen*. Leipzig: G. Fleischer.

Wettersten, J. (1993). Rethinking Whewell. *Philosophy of the Social Sciences, 23*, 481–515.

Wettersten, J. (2005). *Whewell's critics: Have they prevented him from doing good?* (J. A. Bell, Ed.). Poznań Studies in the Philosophy of Sciences and the Humanities, 85. New York/Amsterdam: Rodopi.

Whewell, W. (1831a). Mathematical exposition of some of the leading doctrines in Mr Ricardo's 'Principles of political economy and taxation'. *Transactions of the Cambridge Philosophical Society, 4*, 155–198.

Whewell, W. (1831b). Modern science: Inductive philosophy. Review of *Preliminary discourse on the study of natural philosophy*. *The Quarterly Review, 45*, 374–407.

Whewell, W. (1833). Essay towards a first approximation to a map of cotidal lines. *Philosophical Transactions of the Royal Society of London, 123*, 147–236.

Whewell, W. (1834a). Memoranda and directions for tide observations. *Nautical Magazine, 3*, 41–43, 98–102, 170–171.

Whewell, W. (1834b). On the empirical laws of the tides in the port of London, with some reflexions on the theory. *Philosophical Transactions of the Royal Society of London, 124*, 15–45.

Whewell, W. (1835). On the results of tide observations made in June 1834 at the coast guard stations in Great Britain and Ireland. *Philosophical Transactions of the Royal Society of London, 125*, 83–90.

Whewell, W. (1836a). Researches on the tides.—Fourth series: On the empirical laws of the tides in the port of Liverpool. *Philosophical Transactions of the Royal Society of London, 126*, 1–15.

Whewell, W. (1836b). Researches on the tides.—Fifth series: On the solar inequality and on the diurnal inequality of the tides at Liverpool. *Philosophical Transactions of the Royal Society of London, 126*, 131–147.

Whewell, W. (1836c). Researches on the tides.—Sixth series: On the results of an extensive system of tide observations made on the coasts of Europe and America in June 1835. *Philosophical Transactions of the Royal Society of London, 126*, 238–336.

Whewell, W. (1837a). Researches on the tides.—Seventh series: On the diurnal inequality of the height of the tide, especially at Plymouth and at Singapore; and on the mean level of the sea. *Philosophical Transactions of the Royal Society of London, 127*, 75–85.

Whewell, W. (1837b). Researches on the tides.—Eighth series: On the progress of the diurnal equality wave along the coasts of Europe. *Philosophical Transactions of the Royal Society of London, 127*, 227–266.

Whewell, W. (1838a). Description of a new tide-gauge, constructed by Mr. T. G. Bunt, and erected on the eastern bank of the River Avon, in front of the Hotwell House, Bristol. *Philosophical Transactions of the Royal Society of London, 128*, 249–251.

Whewell, W. (1838b). Researches on the Tides.—Ninth series: On the determination of the laws of the tides from short series of observations. *Philosophical Transactions of the Royal Society of London, 128*, 231–247.

Whewell, W. (1839a). Researches on the tides.—Tenth series: On the laws of low water at the port of Plymouth, and on the permanency of mean water. *Philosophical Transactions of the Royal Society of London, 129*, 151–161.

Whewell, W. (1839b). Research on the tides.—Eleventh series: On certain tide observations made in the Indian seas. *Philosophical Transactions of the Royal Society of London, 129*, 163–166.

Whewell, W. (1840a). Additional note to the eleventh series of researches on the tides. *Philosophical Transactions of the Royal Society of London, 130*, 161–174.

Whewell, W. (1840b). *Philosophy of the inductive sciences*. London: Longmans.

Whewell, W. (1840c). Researches on the tides.—Twelfth series: On the laws of the rise and fall of the sea's surface during each tide. *Philosophical Transactions of the Royal Society of London, 130*, 255–272.

Whewell, W. (1848). The Bakerian lecture. Researches on the tides.—Thirteenth series: On the tides of the Pacific, and on the diurnal inequality. *Philosophical Transactions of the Royal Society of London, 138*, 1–29.

Whewell, W. (1850). Researches on the tides.—Fourteenth series: On the results of continued tide observations at several places on the British coasts. *Philosophical Transactions of the Royal Society of London, 140*, 227–233.

Whewell, W. (1851). On our ignorance of the tides. *British Association Report 1850*, 27–28.

Whewell, W. (1856). Of the transformation of hypotheses in the history of science. *Transactions of the Cambridge Philosophical Society, 9*, 139–146.

Yeo, R. (1979). William Whewell, natural theology and the philosophy of science in mid nineteenth century Britain. *Annals of Science, 36*, 493–516.

Yeo, R. (1989). Reviewing Herschel's discourse. *Studies in History and Philosophy of Science, 20*, 541–552.

Yeo, R. (1993). *Defining science: William Whewell, natural knowledge and public debate in early Victorian Britain*. Cambridge: Cambridge University Press.

Yeo, R. (2009), Whewell, William (1794–1866). In *Oxford dictionary of national biography*. Oxford: Oxford University Press. http://www.oxforddnb.com/view/article/29200. (Accessed 12 August 2009)