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DARWIN'S DEBT TO PHILOSOPHY: AN EXAMINATION OF THE INFLUENCE OF THE PHILOSOPHICAL IDEAS OF JOHN F. W. HERSCHEL AND WILLIAM WHEWELL ON THE DEVELOPMENT OF CHARLES DARWIN'S THEORY OF EVOLUTION

Charles Darwin went up to Cambridge as an undergraduate in 1828. He set off on his voyage around the world on the Beagle in 1831, returning in 1836. About the time of his return he became an evolutionist, and he hit upon the evolutionary mechanism for which he is most famous, natural selection brought on by the struggle for existence, in the autumn of 1838. In 1842 he wrote a short sketch of his theory, and in 1844 he expanded this into a fairly substantial essay. At the urging of his friends, in 1856 he started to prepare for publication a massive evolutionary work incorporating his basic ideas. This (as yet unpublished) work was interrupted by the arrival of A. R. Wallace's essay on evolution, one in which he mirrored Darwin's ideas in an uncanny fashion, in 1858. Thereupon, Darwin dropped all else, wrote an 'abstract' of his evolutionary ideas, and this was published as the Origin of Species in 1859.

In this paper I argue that an important factor in Charles Darwin's development of his theory of evolution through natural selection was the philosophy of science in England in the 1830s. When this factor is recognized, then new light is thrown both upon Darwin's discovery of his

2 Both the sketch and the essay are reprinted in Evolution by Natural Selection, G. de Beer (ed.) (Cambridge: Cambridge University Press, 1958).
4 'On the Tendency of Varieties to Depart Indefinitely from the Original Type', J. Proc. Linn. Soc., Zoology, 3 (1859), 55, reprinted in Evolution, de Beer (ed.).

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evolutionary mechanism and upon the way in which he prepared his theory for public presentation.

I

Philosophy of science, 1830–40

England's most influential philosopher of science in the 1830s was the famed astronomer, John F. W. Herschel, whose philosophical reputation rested upon the deservedly popular *Preliminary Discourse on the Study of Natural Philosophy*.6 Not surprisingly, for Herschel the paradigmatic sciences were the physical sciences, particularly Newtonian astronomy (of the 1830s), and the claims Herschel made about the way science is, or ought to be, reflect this bias. Consequently, many of Herschel's major claims have a curiously familiar ring to today's reader, for in important respects he anticipated the modern philosophical school which also looks to physics for its ideals, so-called 'logical empiricism'. I shall now sketch those tenets of Herschel's philosophy which might have been of interest to a budding scientist; that is, I shall ignore Herschel's metaphysical speculations on the ultimate nature of science and concentrate exclusively on his methodology. I shall consider what, in Herschel's opinion, was the kind of theory a scientist ought aim for and the kind of evidence a scientist ought offer. I shall not at present consider any methodological directives that Herschel thought peculiarly applicable to the biologist, although I break no confidences in admitting that Herschel was not sympathetic towards evolutionary theories.

Essentially Herschel saw scientific theories as hypothetico-deductive systems. Thus he wrote that 'the whole of natural philosophy consists entirely of a series of inductive generalizations . . . carried up to universal laws, or axioms, which comprehend in their statements every subordinate degree of generality, and of a corresponding series of inverted reasoning from generals to particulars, by which these axioms are traced back into their remotest consequences, and all particular propositions deduced from them . . .'.7 Moreover, Herschel made clear that what distinguishes


7 *Op. cit.* note 6, 104. 'Deduction' was a word used very loosely in the nineteenth century. One
scientific axiom systems from other such systems is that the former, unlike the latter, contain laws; these being universal, empirical statements 'of what will happen in certain general contingencies'. What elevates a law above a mere catalogue of empirical facts is that in some sense it expresses the way things must be, that is, to use modern terminology, it allows for 'counterfactuals': if A were to occur (even if it does not), then B would follow. 'Every law is a provision for cases which may occur, and has relation to an infinite number of cases that never have occurred, and never will'.

Herschel distinguished upper level laws, 'fundamental laws', from lower level (derived) laws, 'empirical laws'. Newton's laws of motion and gravitation are the highest of all fundamental laws, Kepler's laws are prime examples of empirical laws. It goes almost without saying that although empirical laws have an indispensible role in science, the ultimate aim of the scientist is fundamental laws, and there are strong hints in Herschel of the distinction modern logical empiricists draw between observable and unobservable concepts (reference to the latter occurring in the axioms of a scientific system and reference to the former occurring in the lower-level derived laws of the system). Thus Herschel wrote that 'the agents employed by nature to act on material structures are invisible, and only to be traced by the effects they produce'. Herschel argued also that the best kind of fundamental or higher law is quantitative; for instance, the law of gravitation, 'the most universal truth at which human reason has yet arrived', gives exact ratios for gravitational attractions.

One point which Herschel emphasized at length is the need of the scientist to make reference in his fundamental laws to (and thus to explain through) causes. In particular, the scientist should aim at explaining through verae causae, where these are causes 'competent, under different modifications, to the production of a great multitude of effects, besides those which originally led to a knowledge of them'. In other words, the scientist must aim to get away from ad hoc putative causes, proposed just to explain one set of phenomena; he must try to relate phenomena of different kinds and to explain them through one embracing all-sufficient

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8 Ibid., 96. 9 Ibid., 36. 10 Ibid., 178, 200. 11 Ibid., 178. 12 Ibid., 193.
13 Ibid., 129 14 Ibid., 144
cause or mechanism. Only then can the scientist be reasonably certain that he has 'causes recognized as having a real existence in nature, and not being mere hypotheses or figments of the mind'. Needless to say, at the top of *verae causae* is force; indeed, Herschel speculated whether all causes might not reduce ultimately to some kind of force.

Finally, what should be mentioned is a point Herschel made so frequently about the confirmation of theories that it might well be regarded as the *leitmotif* of his book, namely that the mark of a truly confirmed theory, one which absolutely has to be taken as true and resting on a *vera causa*, is that the theory be found to explain phenomena in ways unanticipated when the theory was first devised or to explain phenomena which seemed hostile to the theory when first devised.

The surest and best characteristic of a well-founded and extensive induction, however, is when verifications of it spring up, as it were, spontaneously, into notice, from quarters where they might be least expected, or even among instances of that very kind which were at first considered hostile to them. Evidence of this kind is irresistible, and compels assent with a weight which scarcely any other possesses.

The other important philosopher of science in this period being considered was Herschel's close friend, William Whewell. Herschel and Whewell came to differ quite considerably over what I have called the 'metaphysical' aspects of science, Herschel inclining more to empiricism whereas Whewell was much influenced by Kant. However, they differed little, if at all, with respect to 'methodological' questions, the kind of theory a scientist should aim to produce and the way he should try to confirm it. This is perhaps not surprising because I think Herschel and Whewell worked out their philosophies far more in conjunction than independently, and (the Cambridge-educated) Whewell agreed fully

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16 Ibid., 144.
17 Ibid., 170. See also 29–34, and 97–8.
with (the Cambridge-educated) Herschel that the finest of all sciences is Newtonian mechanics, particularly Newtonian astronomy. Indeed, in an address to the British Association in 1833 Whewell spoke of Newtonian astronomy as being the ‘queen of the sciences’,\(^{19}\) and in his *History of the Inductive Sciences* he wrote that ‘Newton’s theory is the circle of generalization which includes all the others; the highest point of the inductive ascent; the catastrophe of the philosophic drama to which Plato had prologized; the point to which men’s minds had been journeying for two thousand years’.\(^{20}\)

Whewell’s major work on the philosophy of science, *The Philosophy of the Inductive Sciences*, did not appear until 1840; but in various writings in the 1830s he managed to show his support of many of the important tenets of Herschel’s philosophy. Thus, for instance, Whewell wrote an enthusiastic review of Herschel’s *Discourse* in the *Quarterly Review* for April 1831. He adopted and emphasized Herschel’s point about the best kind of laws being quantitative laws. Then, in 1833 in his book on natural theology, Whewell agreed not only that the aim of science is to find laws, ‘rules describing the mode in which things *do* act; [things] invariably obeyed’,\(^{21}\) but he advocated, explicitly, the hypothetico-deductive ideal for science.\(^{22}\) And then in his *History*, Whewell followed Herschel in distinguishing between two kinds of laws, speaking of ‘formal’ or ‘phenomenal’ laws and ‘physical’ or ‘causal’ laws, the models for this division being, once again, Kepler and Newton.\(^{23}\)

Finally, there is the question of confirmation. In his *History* Whewell was at great pains to show that the strength of great theories, particularly Newtonian mechanics, is the ability to explain in many different areas, including those unthought of before the theory was discovered.\(^{24}\) As is well known, in his *Philosophy* Whewell labelled this process the ‘consilience of inductions’, and, like Herschel, made much of the element of surprise: ‘the evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a kind different from those which were contemplated in the formation of our

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\(^{19}\) ‘Address to the British Association’, 5. By ‘Newtonian astronomy’, here as elsewhere, is meant the astronomy of the 1830s.


\(^{24}\) See particularly Book 7, chapter 4, ‘Verification and Completion of the Newtonian Theory’. Section 3, ‘Application of the Newtonian Theory to Secular Inequalities’ deals explicitly with surprising or unanticipated phenomena.
hypothesis'. Hence, both with respect to theory–nature and with respect
to theory–proof Herschel and Whewell spoke with almost one voice.

II

Darwin and the philosophers

That Darwin was aware of and responded positively to this philosophy
of science is undeniable. Take the influence of Herschel. Darwin first
read Herschel’s Discourse early in 1831, he reacted enthusiastically to it at
the time, urging his cousin to ‘read it directly’, and, late in life looking
back over his career, he spoke of Herschel’s work in the highest possible
terms.

During my last year at Cambridge I read with care and profound interest
Humboldt’s Personal Narrative. This work and Sir J. Herschel’s Introduction to
the Study of Natural Philosophy stirred up in me a burning zeal to add even the
most humble contribution to the noble structure of Natural Science. No one or
a dozen other books influenced me nearly so much as these two.

Darwin reread the Discourse late in 1838, by which time he knew Herschel
personally. Their social circles overlapped and, more interestingly,
they both appear to have been active members of the (London) Geological

25 Whewell, Philosophy, II, 230.
26 Laudan, ‘William Whewell’, suggests that there is a subtle difference between Herschel
and Whewell on theory-confirmation. ‘Whereas Whewell attaches greatest importance to the
explanation of surprising facts, Herschel seems to lay greatest stress on the successful explanation
of facts which had previously been regarded as counter-examples’, (374, fn. 13).
27 Unpublished letter dated 5 February 1831, from Darwin to W. Darwin Fox, property of
Christ’s College, Cambridge.
29 Darwin filled four notebooks on the species problem between mid-1837 and mid-1839; that
is, just at the time when he was discovering his theory of evolution through natural selection.
These he labelled ‘B’, ‘C’, ‘D’, and ‘E’. They have been transcribed and edited by G. de Beer
and others, Bulletin of the British Museum (Natural History) Historical Series, 2, nos. 2–6, and 3,
no. 5 (London, 1960–7). Darwin also wrote two (consecutive) notebooks which he labelled ‘M’
and ‘N’. M was begun in mid-1838, and on the cover Darwin later wrote: ‘This book full of
Metaphysics on Morals and Speculations on Expression’. N was started on 2 October 1838 and
was labelled ‘Metaphysics and Expression’. They have been transcribed and edited by P. Barrett,
Darwin on Man (New York: Dutton, 1974). I have worked out my dates from comments in these
notebooks and from a reading-list Darwin kept, ‘Books to be Read’. All of these books are in the
Darwin Collection, University Library, Cambridge, as are Darwin’s copies of Herschel’s
Discourse and Astronomy (London: Longmans, 1833) and Whewell’s History. Comments about
Herschel in the margins of the History suggest that Darwin read first Herschel’s Discourse and
then Whewell’s History. There is a reference to the History, showing Darwin had finished reading
it (‘The end of each volume of Whewell’s Induction History contains many most valuable refer-
ences’) on p. 69 of E, dated Dec. 16th (1838). On p. 49 of N there is a reference to Herschel’s
Discourse, written after Nov. 28th (the last preceding date, given on p. 45). Darwin read Her-
schel’s Astronomy at some point—it is dated 1837.
Society.\(^{30}\) Darwin wrote of Herschel that ‘He never talked much, but every word which he uttered was worth listening to’.\(^{31}\) I shall show later that Darwin always thought highly of Herschel and craved his praise.

Darwin’s relationship with Whewell is most interesting. Whewell was a violent anti-evolutionist, and I suspect that in later life neither he nor Darwin was over-keen to emphasize their earlier intimacy. But such intimacy there certainly was. Whilst an undergraduate Darwin knew Whewell well: for his full three years at Cambridge Darwin attended the lectures on botany by the Rev. J. S. Henslow, as also did Whewell.\(^{32}\) Whewell and Darwin met also at Henslow’s weekly scientific evenings, Darwin walking home with Whewell. About Whewell Darwin wrote that ‘Next to Sir J. Mackintosh he was the best converser on grave subjects to whom I ever listened’.\(^{33}\) It goes without saying that, given the context, these ‘grave subjects’ would have included much about science: no doubt in 1831 the enthusiastic Darwin and the equally enthusiastic Whewell talked about Herschel’s *Discourse*.

After his return from the *Beagle* voyage Darwin lived (early in 1837) in Cambridge for three months, but his most important contact with Whewell was through the Geological Society. Whewell was president in 1837 and 1838 whilst Darwin was on the council, and this led to fortnightly meetings.\(^{34}\) Whewell seems to have pushed Darwin’s scientific career strongly, he urged him to get on with the publishing of the results of the *Beagle* voyage, he pressed Darwin into accepting a secretaryship of the Society,\(^{35}\) and, in his second presidential address to the Society, heaped the highest possible praise on Darwin (and hinted, incidentally, that he felt some credit due to himself as one of Darwin’s teachers). In letters to Whewell, Darwin thanked him for having ‘shown so much interest and kindness in all my affairs’ and for ‘the manner of your whole intercourse with me, since my return to England’.\(^{36}\)

I think Whewell’s major influence on Darwin would have been through conversation, but Darwin did read several things by Whewell. These

\(^{30}\) Whewell wanted Herschel to take over presidency of the Geological Society.


\(^{32}\) Information from Henslow’s lecture list, Darwin Collection.


\(^{34}\) My evidence for Whewell and Darwin’s connections with the Geological Society comes from the contemporary minute books, still in the Society’s possession. I am obliged to the Society for having been allowed to look at these.

\(^{35}\) Information from unpublished letters from Darwin to Whewell, Whewell Collection, Trinity College, Cambridge.

\(^{36}\) Unpublished letters in the Whewell Collection.
include Whewell's address to the British Association (Whewell sent Darwin a copy\textsuperscript{37}), the \textit{Bridgewater Treatise} (Darwin read this twice, in early 1838 and in 1840\textsuperscript{38}), and the \textit{History}. Darwin owned a copy of this last-named work, he skimmed it at some point in 1838, probably in early October, and then, just after his rereading of Herschel, read it very carefully, annotating it fully.\textsuperscript{39} He liked the work, praising it to Whewell and to others,\textsuperscript{40} and moreover, Darwin who was so notoriously careless of his books, had the volumes leather-bound. I doubt if Darwin ever read Whewell's \textit{Philosophy}, but he did respond with great interest to a large detailed review of Whewell by Herschel. '—From Herschel's Review Quart. June 41 I see I MUST STUDY Whewell on Philosophy of Science'.\textsuperscript{41}

Darwin was therefore fully aware of the Herschel–Whewell philosophy of science, and all the direct evidence points to an enthusiastic reaction. Moreover, the genuineness of this reaction is supported, both by comments which Darwin made about scientific methodology and by the scientific works which he produced. We have seen that central to the philosophy was the taking of Newtonian astronomy as the paradigm for science. Many comments made by Darwin show that he accepted this claim entirely, and that, indeed, his aim was to be the Newton of biology. Thus, for example, he wrote as follows in a private notebook in 1837.

Astronomers might formerly have said that God ordered each planet to move in its particular destiny. In same manner God orders each animal created with certain form in certain country, but how much more simple and sublime power let attraction act according to certain law, such are inevitable consequences—let animal be created, then by the fixed laws of generation, such will be their successors. Let the powers of transportal be such, and so will be the forms of one country to another.—Let geological changes go at such a rate, so will be the number and distribution of the species!!\textsuperscript{42}

And when he was presenting his theory again and again Darwin defended himself against possible criticisms on the grounds that he was being more Newtonian than any would-be critics. Thus, in his first full-length ex-

\textsuperscript{38} Darwin, 'Books to be Read', 7; notebook C, 72.
\textsuperscript{39} 'A short time since I finished, having only skimmed parts before ... the \textit{History of Inductive Sciences}'. Unpublished letter from Darwin to Whewell, postmark April 17, 1839, property of Trinity. In notebook N, p. 14, dated Oct. 8th, there is a reference to the \textit{History}—V. Whewell, \textit{Induct. Science}, vol. 1, p. 334.
\textsuperscript{40} Darwin praised it to R. Brown, \textit{Autobiography}, 104, and to Whewell in the unpublished letter of April 1839.
\textsuperscript{41} 'Books to be Read', no page number given. Herschel's review was in the \textit{Quarterly Review}, 68 (1841), 177–238.
\textsuperscript{42} Darwin, notebook B, 101–2.
position of his theory (the Essay of 1844), Darwin asked ‘... shall we then say that a pair, or a gravid female, of each of these three species of rhinoceros, were separately created ...? For my own part I could no more admit [this] proposition than I could admit that the planets move in their courses, and that a stone falls to the ground, not through the intervention of the secondary and appointed law of gravity, but from the direct volition of the Creator'.

Were one to single out from the Herschel-Whewell philosophy the two features that one would expect most likely to be manifested in any scientific theory consciously influenced by the philosophy, they would probably be first the hypothetico-deductive model and secondly the use of one central mechanism or cause to explain phenomena in widely different areas. Both of these features are manifested, to a significant extent, in Darwin’s theory in the Origin, and they can be traced back to Darwin’s earlier versions of his theory, the Sketch of 1842 and the Essay of 1844. Furthermore, these were features Darwin intended this theory to have and he took pride in the fact that he thought his theory did have them.

Take first the hypothetico-deductive ideal. Darwin’s following of this is particularly apparent in what one might call the ‘core’ arguments of his theory. Darwin’s major mechanism of evolutionary change, natural selection, is something which embodies both the notion that in each generation there is a differential reproduction of organisms, more organisms being born than can survive and reproduce, and the notion that the survival of the successful organisms is in part a function of characteristics which they, unlike unsuccessful organisms, possess. Darwin did not just drop natural selection into his theory, unannounced. Rather, he argued first to a struggle for existence and then to natural selection, and these arguments to the struggle and then to natural selection approximate closely to the hypothetico-deductive ideal. Thus Darwin started his arguments with statements which seem very much like laws (understood in the Herschelian sense), for instance, that given any species of organisms they will be found to have a tendency to increase their numbers at a geometrically high rate. And this, he tried to show, is something

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42 de Beer, Evolution, 250-1.
43 In the late 1830s, apart from his evolutionary theory, Darwin produced a coral-reef theory and an explanation of the parallel roads of Glen Roy. M. T. Ghiselin, The Triumph of the Darwinian Method (California: University of California Press, 1969) argues that Darwin was hypothetico-deductive in his coral-reef theory, and M. J. S. Rudwick informs me that Darwin aimed at a conciliation of inductions in his Glen Roy discussion.
which must hold for any species you like to name, even the most slow breeding of species. Then, from lawlike statements like these, Darwin tried to show that his conclusions, first about a struggle and then about selection, must follow. And, of course, this is what deduction is all about.

Even more obvious than Darwin's attempt to satisfy the hypothetico-deductive ideal was his attempt to use his mechanism of evolutionary change, natural selection, to explain phenomena in many widely different areas. Thus Darwin showed how natural selection solves problems of geographical distribution, of instinct, of geology, of classification, of comparative anatomy, of embryology, and so on. All of these various areas come under the umbrella of selection just as so many areas of physical inquiry come under the umbrella of Newtonian gravitational force. And, as I have mentioned, Darwin intended and took credit for having shown both this fact and the former fact, namely that he had manifested the hypothetico-deductive ideal. He wrote constantly of showing how things, first like the struggle and then like the phenomena of geographical distribution, follow 'inevitably' from laws, and whenever challenged about the truth of this theory Darwin pointed always to the wide scope of his mechanism: 'I must freely confess, the difficulties and objections are terrific; but I cannot believe that a false theory would explain, as it seems to me it does explain, so many classes of facts'.

It cannot be denied, as critics were quick to point out, that Darwin was not entirely successful at achieving the Herschel–Whewell theory ideal. In particular, many of the inferences in Darwin's theory taken as a whole were far from being rigorously deductive. However, this is not to deny Darwin's intentions, and one's estimation of the success he actually achieved becomes much increased when one compares Darwin's theory against the works in the 1830s of other non-physical scientists. Thus, although Lyell's chief aim was to show that the past world can be explained by laws of the present world, he never achieved even the limited hypothetico-deductive success of Darwin, preferring rather to make his points with strings of related examples. And the same goes for the work of someone like Henslow, who relied on description and example rather than the axiomatic method.

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46 For example, Origin, 63, 80-1, 489-90.
48 For example, W. Hopkins, Fraser's Magazine, 61 (1860), 739-52; 62 (1861), 74-90.
49 Principles of Geology (London: Murray, 1830-3).
50 Descriptive and Physiological Botany (London: Longmans, 1835).
In concluding this section, let me make one caveat. I argue that Darwin was influenced by the Herschel–Whewell theory ideal and I have given reasons to suggest that this would have been a direct influence. I do not, however, want to suggest that this was an entirely exclusive influence. I think that pretty well everybody in the 1830s accepted this philosophy of science and that Darwin would have received it from others as well. For example, Lyell and Whewell had a continuing debate over whether one ought to be a uniformitarian or catastrophist in geology, and both the uniformitarian Lyell and the catastrophist Whewell defended their respective positions as being more Newtonian than the others! And I am sure that a major reason why Darwin did not change his theory in any significant way after its first formulation was because there was no significant change in the philosophy of science (qua theory-nature ideal) between the writing of the Sketch (1842) and the writing of the Origin (1858–9). Even J. S. Mill, in his influential System of Logic, managed to incorporate many of the salient features of the hypothetico-deductive approach though he differed from Whewell at least in his estimate of the sufficiency of that method to yield a doctrine of proof. But then, as I shall show later, at this point where Mill diverged from Herschel and Whewell, Darwin sided with the earlier philosophers rather than with Mill.

But, whilst admitting this caveat about other possible influences on Darwin one must be careful not to under-estimate Herschel and Whewell themselves, and certainly one must be careful not to fall into the trap of thinking that because Herschel and Whewell were anti-evolutionists they cannot have been significant influences on Darwin. Nigh-on everyone was an anti-evolutionist in the 1830s; Lyell, probably Darwin’s greatest intellectual influence, was one of the leaders of the attack against evolutionary theories, and indeed, Lyell’s position was practically indistinguishable from Herschel’s. Nor should one assume that Darwin’s theory was bound to be the way it was, because every scientific theory was that way. As I have just pointed out, Darwin hardly got the salient

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51 Whewell, History, III, 680; and K. Lyell (ed.), Life, Letters, and Journals of Sir Charles Lyell (London: Murray, 1881), II, 5–6. In August 1838 Darwin did read with avid interest a review by Sir David Brewster of Comte’s Cours de Philosophie Positive (Edinburgh Review, 67 (1838), 271–308). What he would have got from this is that the aim of science is the ‘positive’ stage (280), that ‘The fundamental character of Positive Philosophy is to regard all phenomena as subjected to invariable natural laws’ (281), and that the best of all laws is the Newtonian law of gravitational attraction (282).

52 London: Longmans, 1843.

53 They both wanted organic origins through law, but both thought that these laws cannot be evolutionary. Whewell plumbed for miracles.
aspects of the Herschel-Whewell philosophy from the work of men like Lyell and Henslow, because these aspects were absent from their work. The direct influence of Herschel and Whewell, although not exclusive, should not be discounted; in any case, many of Darwin’s other influences like Lyell and Henslow probably got their philosophy of science from Herschel and Whewell in the first place.

III

New light on Darwin

I shall argue now that recognizing the importance for Darwin of the Herschel-Whewell philosophy of science enables us to solve several puzzles in the Darwinian story. I take first the question of Darwin’s discovery of his theory, and in particular the role played in this discovery by the thought of Malthus. As mentioned earlier, we know that Darwin came upon, or recognized, his main mechanism of evolutionary change some time in the autumn of 1838. His discovery seems to have been a two-part process; he grasped the principle of natural selection by analogy from breeders’ use of artificial selection on domestic organisms, and then, after reading the *Principle of Population* by T. R. Malthus, he saw in some way how he could use the struggle for existence as a kind of driving force behind natural selection. Thus, to Wallace, Darwin wrote: ‘I came to the conclusion that selection was the principle of change from the study of domesticated productions; and then, reading Malthus, I saw at once how to apply this principle.’

Recent Darwin scholarship has shown that Darwin’s route to discovery was less direct than he himself implied. For a start, before the reading of Malthus (about 28 September 1838) most of the comments Darwin made show that he, like everyone else at the time, looked on the domestic world as pointing away from a mechanism of evolutionary change, rather than towards it. For instance, one comment Darwin made shortly before reading Malthus was: ‘It certainly appears in domesticated animals that the amount of variation is soon reached—as in pigeons no new races—.’ However, despite comments like these, it does now seem that Darwin was definitely led to the mechanism of natural selection from the analogy with artificial selection. In particular, Darwin got the concept of natural

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56 Darwin, notebook D, 104. Written 13 September 1838.
selection in mid-1838 from reading animal breeders’ pamphlets, which pamphlets talked not only about artificial selection but talked as well about natural selection (not by that name) and explicitly drew an analogy between the two kinds of selection. Nevertheless, a puzzle about Malthus still remains. Why was it necessary for Darwin to read Malthus before he recognized that in natural selection he had a mechanism of evolutionary change? Before reading Malthus Darwin gave no hint that he differed from the breeder’s assessment of natural selection, namely that it was something which would cause only limited change within a species. It cannot be just that Malthus drew Darwin’s attention to the struggle for existence, because Darwin knew all about the struggle long before reading Malthus. The struggle is described explicitly and in detail in Lyell’s Principles of Geology, two editions of which Darwin had read by mid-1837. Indeed, Lyell even talks of the struggle for existence by that name.

Understanding the importance for Darwin of the Herschel–Whewell philosophy, Malthus’ contribution to Darwin’s discovery becomes readily explicable. Malthus showed Darwin how he could locate the struggle, with the consequent selection, in a hypothetico-deductively organized network of laws; of laws which were, moreover, quantitative; in Herschel’s and Whewell’s eyes the best kind of laws. Malthus argued that a struggle for existence amongst humans would inevitably ensue, unless prevented by moral restraint (or something unmentionable like contraception), because humans have a tendency to increase in number at a geometrical rate whereas their food supplies can increase only at maximum at an arithmetical rate. Darwin seized upon this argument, generalizing to all animals, thus eliminating the alternatives to the struggle. He now had quantitative laws, leading deductively to the struggle, which he was then able to extend to selection. Thanks to Malthus, Darwin was able to put his mechanism for evolutionary change into a satisfactory context; a context, that is, which satisfied the Herschel–Whewell theory ideal.

But Malthus was important for Darwin for another, related reason. The Herschel–Whewell philosophy demanded that one explain through causes, the best kind of which, perhaps the only kind of which, were

58 Lyell, Principles 5th ed., II, 430. (Darwin owned and read this edition.)
forces. Through Malthus, Darwin saw the struggle as being a kind of force, which would in turn, as it were, propel the force of selection. As soon as he read Malthus the excited Darwin scribbled in his notebook that:

Population is increased at geometrical ratio in FAR SHORTER time than 25 years—yet until the one sentence of Malthus no one clearly perceived the great check amongst men.—there is spring, like food used for other purposes as wheat for making brandy.—Even a few years plenty, makes population in man increase and an ordinary crop causes a dearth. take Europe on an average every species must have same number killed year with year by hawks, by cold etc.—even one species of hawk decreasing in number must affect instaneously all the rest.—The final cause of all this wedging, must be to sort out proper structure, and adapt it to changes.—to do that for form, which Malthus shows is the final effect (by means however of volition) of this populousness on the energy of man. One may say there is a force like a hundred thousand wedges trying [to] force every kind of adapted structure into the gaps in the oeconomy of nature—or rather forming gaps by thrusting out weaker ones.60

As this passage shows, Malthus enabled Darwin to see the struggle and the consequent selection in terms of force. Hence, Darwin, working in the light of the Herschel-Whewell philosophy, felt able to regard selection as a possible evolutionary mechanism.

If, as I argue, the Herschel-Whewell philosophy was an important factor in Darwin’s response to Malthus, one might naturally ask if the philosophy played any role in Wallace’s discovery of natural selection, because he like Darwin acknowledged an important debt to Malthus.61 Although Wallace certainly read Whewell’s History,62 I suspect the real key to Wallace’s response lies in Robert Chambers’ Vestiges of the Natural History of Creation.63 In a recent book H. L. McKenney argues that ‘the influence of the Vestiges [on Wallace] . . . can scarcely be over-emphasized’.64 But a major aim of Vestiges is to show that as good New-tonians we much accept a biological evolutionary theory. Wallace, I think, whilst rejecting as inadequate Chambers’ own evolutionary theory, entirely accepted Chambers’ research programme, to find the biological analogue of Newtonian astronomy.65 Thus I would suggest that Wallace

60 Darwin, notebook D, 135.
64 McKinney, Wallace, 12.
65 In 1855 Wallace proposed the law that ‘Every species has come into existence coincident both in time and space with a pre-existing closely allied species’. He wrote that ‘Granted [this] law, and many of the most important facts in Nature could not have been otherwise, but are almost as necessary deductions from it, as are the elliptic orbits of the planets from the law of
like Darwin, may have reacted favourably to Malthus' ideas because he
could then start to see his way towards a biological equivalent of Newton-
ian astronomy. Hence, I think that Darwin and Wallace quite possibly
started from similar philosophical positions, although I have no reason
to believe that they drew on exactly the same immediate sources for the
philosophies. Indeed, I doubt that their sources were exactly the same,
for, as I shall show next, Darwin took an altogether different methodo-
logical step from Wallace because of what I think were Darwin's desires
to present a theory which would satisfy Herschel's criteria of theory-
excellence.

As 1838 drew to a close, Darwin had his major mechanism of evolution-
ary change. He had now to start to think about converting his mechanism
into a full-blown theory, one which he would present to the world. An
understanding of the Herschel-Whewell influence remains crucial to the
grasping of Darwin's reasonings, particularly the way in which he used
the analogy from artificial selection.

Darwin knew well that any theory of evolution was going to be highly
controversial, to say the least. That meant he had to make the best pos-
sible case, particularly the best possible case in the eyes of the ultimate
 arbiters of scientific acceptability, Herschel and Whewell. He felt he had
to satisfy their criteria of good science. Indeed, interestingly, Darwin
always felt this way. By 1859, the year of publication of the Origin, the
long-invalid Darwin moved in different circles from the philosophers,
Whewell particularly. Nevertheless, Darwin sent copies of the Origin to
both Herschel and Whewell, and he prefaced the Origin with a quotation
by Whewell to the effect that the world works exclusively according to
law (as if to point out that he, Darwin, was merely following Whewell's
prescriptions66), and, most significantly, waited with interest and trepida-
tion for Herschel's evaluation of his theory. When the great man was
reputed as having characterized the Origin as 'the law of higgledy-pig-
gledy', Darwin spoke of Herschel's evaluation as 'a great blow and
discouragement'.67 (Actually, as we shall see, Herschel's verdict was not
entirely negative.)

In December of 1838, Darwin turned seriously to the question of how
best he ought to develop and present his theory. To this end, he reread

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66 The passage comes from the Bridgewater Treatise, 965.
Herschel's *Discourse* and went very carefully through Whewell's *History*. Gauging his interest in the latter work from the extent to which he annotated and marked the various sections of his own copy, Whewell's volumes were of particular interest for two reasons. On the one hand, Darwin wanted to see what were the precise merits of a theory like Newton's, what made it so exceptional a theory. On the other hand, Darwin wanted to see what was the strongest possible case that could be made by an anti-evolutionist: Darwin wanted to leave no possible criticism unconsidered. Thus, when Whewell claimed that every evolutionist would be saddled with Lamarckian assumptions about necessary progressive evolutionary tendencies and constant creation of new sparks of life ('monads'), Darwin exclaimed in the margin that 'These are not assumptions, but consequences of my theory, and not all are necessary'.

Now, Whewell's major criticism of the evolutionist, one which was to be found in both Cuvier and Lyell, was that present evidence, particularly that of animal and plant breeders, points away from rather than towards the creation of new species. Hence, argued Whewell, new species cannot have been created naturally in the past. Darwin realized that if he were to make his case he had to counter this criticism, and as is well known, the way in which he tried to counter the criticism was by arguing that Whewell and others were wrong to cite modern breeding techniques and results as evidence *against* evolution. Darwin argued that in fact such techniques and results were evidence *for* evolution. But why did Darwin employ this strategy? We saw that earlier in 1838 Darwin himself seems to have agreed that the domestic world points away from rather than towards evolution. Why did Darwin not employ the kind of strategy employed by Wallace in his 1858 evolutionary essay, and argue that since the domestic and the wild worlds are so drastically different, one cannot possibly draw any analogies between the two, and that hence the failure to produce new permanent forms in the domestic world does not prove that no such forms can be produced in the wild world?

Part of the reason why Darwin adopted the particular strategy that he did stems, no doubt, from the fact that by the end of 1838 he was beginning to doubt the conventional wisdom on animal and plant breeding; he was starting to get evidence that artificially induced changes could be fairly permanent. But this was not the main reason why he suddenly became so keen to stress the analogy between domestic and

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68 In the margin of Whewell, *History*, III, 579.
natural selection; why he suddenly swung round completely from his earlier position and wrote in a notebook that 'It is a beautiful part of my theory, that domesticated races of organics are made by precisely same means as species—but latter far more perfectly and infinitely slower'.

The answer to Darwin's switch lies in the doctrine of *verae causae*, a doctrine, as we have seen that was absolutely central to a Herschellian philosophy of science.

Herschel argued that one must aim to base one's reasonings on *verae causae*, and Darwin was desperately keen to show that his evolutionary reasonings were based on a *vera causa*, natural selection. But how was Darwin to show beyond doubt, that natural selection was a *vera causa*? Here, Herschel's discussion becomes of vital interest: the most convincing evidence that something is a *vera causa*, Herschel argues, is when we can argue analogically from something else which we know already to be a *vera causa*. Herschel writes:

Here, then, we see the great importance of possessing a stock of analogous instances or phenomena which class themselves with that under consideration, the explanation of one among which may naturally be expected to lead to that of all the rest. If the analogy of two phenomena be very close and striking, while, at the same time, the cause of one is very obvious, it becomes scarcely possible to refuse to admit the action of an analogous cause in the other, though not so obvious in itself. For instance, when we see a stone whirled round in a sling, describing a circular orbit round the hand, keeping the string stretched, and flying away the moment it breaks, we never hesitate to regard it as retained in its orbit by the tension of the string, that is, by *a force* directed to the centre; for we feel that we do really *exert* such a force. We have here the *direct perception* of the cause. When, therefore, we see a great body like the moon circulating round the earth and not flying off, we cannot help believing it to be prevented from so doing, not indeed by a material tie, but by that which operates in the other case through the intermedium of the string,—*a force* directed constantly to the centre.

Take note, not only of the overall point Herschel was trying to make, but of his actual example. We have, in the case of the swinging stone, *a force, directly perceived and caused by us*; hence, we know there must be a force causing the moon to swing around the earth. Darwin, who thought already of natural selection as a force, realized that he had an absolutely identical situation in biology. We have in artificial selection, *a force*

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70 The Wilkinson pamphlet, read mid-1838, suggests that some artificial change may be permanent, and Darwin noted this point. But even then, he still wrote (later) that the possible change in pigeons is limited.

71 Darwin, notebook E, 71. This comment was made between Dec. 16th and Dec. 21st, 1838.

72 Herschel, *Discourse*, 149, his italics.
directly perceived and caused by us; hence, analogically, given the struggle and given wild variation, it cannot be denied that there is a natural force of selection making different organisms, just as man makes different organisms. In Herschel’s own terms he had definitive proof that natural selection is a *vera causa*. No wonder, therefore, that Darwin who had just finished reading Herschel wrote excitedly that it was a virtue of his theory that his mechanism of evolutionary change was virtually the same mechanism as that of domestic organic change.

This, then, was Darwin’s major motive for stressing the analogy between artificial and natural selection. What we find, from his notebooks and his reading lists, is that in 1839 Darwin set with a will to the study of the results of breeders, to see just how much and how permanent artificial changes would be. To this end, Darwin was in a peculiarly favoured position. First, his family had long and with success kept and bred pigeons for a hobby.73 So he had direct access to the world of the pigeon fancier. Secondly, his uncle (and after early 1839 father-in-law) Josiah Wedgwood (Jnr) had extensive experience in sheep breeding and selecting.74 Also, Wedgwood was much involved in the Society for the Diffusion of Useful Knowledge,75 which was trying to disseminate breeding knowledge, in the main through the sponsorship of the classic works on breeding by Youatt, many of which works Darwin read in 1839. Convinced of the importance of the analogy between artificial and natural selections, Darwin had little trouble in persuading himself that, far from detracting from an evolutionary hypothesis, the domestic world supported such an hypothesis. The arguments of Whewell, and others like Lyell, were countered, and so, when Whewell in his 1839 Presidential Address to the Geological Society said that ‘If we cannot reason from the analogies of the existing to the events of the past world, we have no foundation for our science’, Darwin replied triumphantly that ‘but experience has shown that we can and that analogy is sure guide and my theory explains why it is sure guide’.76

Until he felt ready to write out his theory, Darwin’s chief concern was that of seeing how his mechanism of natural selection could be used to solve problems in different areas like instinct, geographical distribution, and so on. Although Darwin thought always that the analogy from artifi-

74 See correspondence with Thomas Poole (in British Museum). Wedgwood, at one point, had 2000 sheep including 300 Merinos which he was trying to introduce to England.
75 He was president of the Etruria branch.
76 Darwin, notebook E, 128. Interestingly, Darwin sought information on breeding by means of a printed questionnaire—a technique explicitly advocated by Herschel, *Discourse*, 134.
cial selection was support for his theory, the chief proof for Darwin of the truth of his theory was that it had explanatory power in all of these many diverse areas. And in this belief, as I have shown, Darwin was treading in the footsteps of Herschel and Whewell. There was, however, more to Darwin's acceptance of the Herschel-Whewell position on theory-confirmation than so far explained, and when this is revealed, an answer is found for one of the knottier puzzles in the Darwinian story, namely Darwin's attitude towards embryology.

In the *Origin* and in the *Sketch* and the *Essay*, Darwin used natural selection to explain one of the more curious aspects of embryology, namely that organisms of different species which have widely different adult forms, sometimes have almost identical embryonic forms. Darwin pointed out that the embryos of different species are often under almost identical environmental conditions (and hence subject to the same selective forces), whereas the adults might be under very different conditions (and hence subject to very different selective forces). He suggested that some new variations may occur just in the adult forms, and that the different selective pressures on the adults could lead to different variations being preserved, and hence to the difference in adults of different species which we see today. The embryos of different species, on the other hand, would be subject to the same selective pressures, and hence there would be no parting of the ways. Obviously, absolutely crucial to this explanation is the belief that selection can preserve a characteristic for just part of an organism's life, in particular, a characteristic can be preserved in the adult with no selective pressure forcing it back to the embryonic form.

Darwin put inordinately great value on this embryological explanation. Although the discussion of embryology is one of the briefest in the *Origin*, time and again he argued that one must accept his theory, not merely because it explains so widely, but because it explains the facts of embryology. Thus, to Hooker he wrote that 'Embryology is my pet bit in my book, and, confound my friends, not one has noticed this to me . . .' And to Lyell he wrote that, if Lyell were not prepared to accept his theory, 'you give up the embryological argument (the weightiest of all to me), and the morphological or homological argument'. And although many people thought embryology important to evolutionary studies because, like

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77 Letter to G. Bentham, May 1863, Darwin, *Life*, III, 25. In this letter, Darwin wrote of natural selection as a *vera causa*, although by then the term was used almost as loosely as 'deduction'.

78 Darwin, *Life*, II, 244.

Ernst Haeckel, they believed ontogeny in some sense ‘recapitulates’ phylogeny, for Darwin it was his own embryological explanation which was important, ‘Hardly any point gave me so much satisfaction when I was at work on the Origin as the explanation of the wide difference in many classes between the embryo and the adult animal, and of the close resemblance of the embryos within the same class’.

The reason why embryology was so important for Darwin stems directly from his acceptance of the Herschel–Whewell philosophy of theory–confirmation. We have seen that for Herschel (and for Whewell) the highest possible mark of theory–truth is when a theory explains phenomena, not built into the original explanation but perhaps even surprising or thought hostile by the scientist when he first conceives of his theory. ‘Evidence of this kind is irresistible, and compels assent with a weight which scarcely any other possess’. But in embryology Darwin had just such evidence. After he had thought of natural selection as an evolutionary mechanism, he was at first convinced that the struggle would be such that no characteristic could be preserved, unless it was of value for the whole life of an organism, ‘No structure will last without it is adaptation to whole life of animal, and not if it be solely to womb as in monster, or solely to childhood, or solely to manhood—it will decrease and be driven outwards in the grand crush of population—’. Then, some time after writing this (but before writing the Sketch of 1842), Darwin realized that his theory did not commit him to this, and that if he took the opposite position, he could explain the facts of embryology: facts which were certainly hostile to the theory if it did indeed imply that characters must be of value to the organism’s whole life.

I suspect that Darwin’s realization that he had in his grasp an explanation of the facts of embryology may have come through his study of domestic organisms. Breeders usually select just adults, with the consequence that although the juveniles of different varieties may be similar the adults are different. Darwin certainly used facts like these as strong evidence for his embryological explanation. But, be this as it may, embryology yielded just the kind of evidence Herschel and Whewell argued counted most decisively in favour of a theory’s truth, and their follower Darwin agreed. The facts of embryology, which had once seemed hostile to his theory were now the theory’s most positive evidence.

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80 Darwin, Autobiography, 125.  
81 Herschel, Discourse, 170.  
82 Darwin, notebook E, 9, his italics. This passage was written in October 1898.  
83 Even if Laudan is right in finding a difference between Herschel and Whewell on theory-confirmation, Darwin’s embryological explanation seems to fit either’s criteria.
Curiously, Darwin’s feeling that he had definitive evidence in favour of his theory was not a feeling universally shared by the Darwinians. T. H. Huxley, Darwin’s ‘bulldog’, always had reservations about the ultimate efficacy of natural selection, arguing that it would not be proven definitively as an evolutionary mechanism until someone had given direct evidence that selection, artificial or natural, had caused physiological speciation. In this he differed from Darwin who was absolutely convinced that because of his theory’s wide scope (extending to embryology) it was conclusively proven. A plausible hypothesis of this difference of opinion lies in the fact that Huxley was much influenced by the philosophy of Mill, and that the question of theory-confirmation is one point where we do find a difference between Herschel and Whewell on the one hand and Mill on the other. In particular, Mill denied that explanations of surprising or apparently hostile phenomena are incontrovertible marks of theory-truth. Although they may add to a theory’s likelihood, argued Mill, they still leave room for doubt. Possibly, the difference between Darwin and Huxley reflects the difference between their philosophical mentors. Mill himself, although sympathetic to Darwin’s theory, was certainly not convinced of its ultimate truth.

I come to the final aspect of Darwin’s thought for which I want to suggest that the Herschel-Whewell influence was crucial. Darwin thought always that his theory of evolution was essentially incomplete. In particular, he thought he ought to provide a theory of heredity to explain the facts of new variation and transmission from one generation to the next. As is well known, eventually Darwin did produce some such theory, ‘pangenesis’, although he never incorporated this theory into the argument of the Origin.

Why did Darwin feel the need to supply such a theory as pangenesis? Why did he not take the facts of variation as given? The reason lies in the Herschel-Whewell dichotomy between formal or phenomenal laws and causal or fundamental laws, and Darwin’s acceptance of this dichotomy. Herschel and Whewell argued that one ought, as it were, get behind the phenomena to the underlying causes, where these might well involve invisible entities. Darwin agreed that one ought to get behind the visible facts of variation and delve into, perhaps unseen, causes, and we find that

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84 Darwin, Life, II, 158.
88 See Darwin’s, Variation of Animals and Plants under Domestication (London: Murray, 1868).
in early 1839, as he was putting his theory together, he speculated on this problem, *in Whewell's very language.*

Those discovering the *formal* laws of the correlation of parts in individuals, will care little, whether the individual be species or variety, but to discover *physical* laws of such correlation, and changes of individual organs, must know whether the individuals forms are permanent, all steps in the series, their relation to the external world, and every possible contingent circumstance . . . .

Treating of the formal laws of correlation of parts and organs it may serve perfectly to specify types and limits of variation, and hence indicate gaps.—by this means the laws probably would be generalized, and afterwards by the examination of the special cases, under which the individual stages in the series have been fixed, to study the physical causes. 89

Darwin felt that one had to get to the physical laws of variation, and he did not rest until he produced his theory of pangenesis, a theory which traces the facts of heredity to minute *unseen* gemmules being cast off by body cells. Not surprisingly, although few of his friends felt much enthusiasm for this theory, Darwin defended it on the grounds that it explained a wide variety of phenomena. 90 He remained faithful to the Herschel–Whewell philosophy.

### IV

#### Epilogue

Both Herschel and Whewell reacted unfavourably to the theory of the *Origin*. Herschel, as already noted, spoke of the law of higgledy-piggledy, and Whewell supposedly refused to let the offensive volume into the library of Trinity. 91 They both felt that Darwin had failed to do what any good biological theorist *must* do, pay adequate recognition to the role of God’s Design in the formation of organisms. To them it was inconceivable that organic adaptation, something like the hand, had not been Designed, and to them it was inexcusable for a biologist not to have given this Design a central explicit place in his theorizing. Actually, the reactions of Herschel and Whewell did differ somewhat, Whewell rejected Darwin’s theory entirely, 92 whereas Herschel thought Darwin’s theory might be salvaged and shown of value, if only he would make place for Design. We must

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89 Darwin, notebook E, 53–5, his italics. Written early November 1838.

90 *Darwin, Life*, III, 78.

91 This is a story of Huxley’s, and should perhaps be taken with a pinch of salt—although, knowing Whewell, not too big a pinch.

admit 'an intelligence, guided by a purpose. . . . On the other hand, we do not mean to deny that such intelligence may act according to law. . . .' " And Herschel went on to concede that, granting some reservations about the origin of man, 'we are far from disposed to repudiate the view taken of this mysterious subject in Mr. Darwin's book.'

Darwin thought that his mechanism of natural selection made an appeal to explicit Design unnecessary. Organic adaptation could be seen to be the result of normal, undirected laws. Against Herschel, Darwin wrote that '. . . astronomers do not state that God directs the course of each comet and planet. The view that each variation has been providentially arranged seems to me to make Natural Selection entirely superfluous, and indeed takes the whole case of the appearance of new species out of the range of science.'

Herschel and Whewell had presented to the world a philosophy of science inspired chiefly by Newtonian physics, particularly Newtonian astronomy. Their greatest pupil had learnt his lesson well; so well, in fact, that when the time came, as pupils are wont to do, Darwin turned their teaching back against them. Darwin's was a theory modelled, through the medium of Herschel and Whewell, on Newtonian astronomy. Why then should his theory be expected to do that which astronomy does not do?

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92 Physical Geography (Edinburgh: Black, 1861), 12.
93 Herschel, Geography, 12.
94 Darwin, Life, II, 191