SIR JOHN F. W. HERSCHEL AND CHARLES DARWIN: NINETEENTH-CENTURY SCIENCE AND ITS METHODOLOGY

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There are a bewildering variety of claims connecting Darwin to nineteenth-century philosophy of science—including to Herschel, Whewell, Lyell, German Romanticism, Comte, and others. I argue here that Herschel's influence on Darwin is undeniable. The form of this influence, however, is often misunderstood. Darwin was not merely taking the concept of "analogy" from Herschel, nor was he combining such an analogy with a consilience as argued for by Whewell. On the contrary, Darwin's *Origin* is written in precisely the manner that one would expect were Darwin attempting to model his work on the precepts found in Herschel's *Preliminary Discourse on Natural Science*. While Hodge has worked out a careful interpretation of both Darwin and Herschel, drawing similar conclusions, his interpretation misreads Herschel's use of the *vera causa* principle and the verification of hypotheses. The new reading that I present here resolves this trouble, combining Hodge's careful treatment of the structure of the *Origin* with a more cautious understanding of Herschel's philosophy of science. This interpretation lets us understand why Darwin laid out the *Origin* in the way that he did and also why Herschel so strongly disagreed, including in Herschel's heretofore unanalyzed marginalia in his copy of Darwin's book.

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It has been claimed time and again that a large part of the impact of Charles Darwin's *On the Origin of Species* (1859) was methodological. Lennox (2005, 85), for example, considering the question of whether or not Darwin was an innovator in his field, has argued that "if Charles Darwin meets this condition, it is as a philosopher and methodologist." Philosophers of science have therefore naturally wondered what relationship Darwin's methodology might have to the fertile climate of nineteenth-century philosophy of science.

Unfortunately, on this point, Darwin studies have bequeathed us precious little clarity. We know that Darwin's intellectual development and reading were incredibly multifaceted (Manier 1978; Sloan 2009), but even this cannot explain the variety of claims we find regarding Darwin's relationship to various philosophers (and philosophies) of science. We learn that Darwin was influenced by Herschel (Ruse 1975; Hodge 1977), that Darwin was not influenced by Herschel (Cannon 1976a, 1976b; Thagard 1977; Desmond and Moore 1992), that Darwin was influenced by Whewell (Ruse 1978, 2000; Curtis 1987), that Darwin was not influenced by Whewell (Hodge 1989, 1991, 2000), and that Darwin was influenced by Lyell (Hodge 1983a, 1983b, 1990, 2009), Comte (Schweber 1977), and German Romanticism (Sloan 2001; Richards 2002, 2009). Of course, these various claims (or at least the noncontradictory ones) are not necessarily mutually exclusive. It suffices to say, however, that the waters in this realm are rather muddied.

In this article, I will endeavor to clarify at least one of these relationships—that between Darwin and Sir John Herschel. The central question, for my purposes here, is this: to what extent did Darwin absorb and proceed to follow, in the construction of the *Origin*, the methodological dictates laid down by Herschel? I will proceed by focusing on four issues. First, what was Herschel's philosophy of science, at least with regard to the formation and verification of hypotheses and theories like Darwin's? My reconstruction will center on the two of Herschel's arguments most relevant to Darwin—his discussion of hypotheses and his elaboration of the *vera causa* principle—in particular, his distinction in both these settings between the proposal and the verification of a hypothesis.[1] With this established, then, what can we say about Darwin's exposure to Herschel? As it turns out, Darwin (like most other British men of science in the mid-nineteenth century) read and appreciated Herschel's work, and we have evidence that he returned to it in a particularly crucial period during the composition of the theory of natural selection. Next, can we see any evidence of the

1. As an anonymous reviewer noted, many more concepts from Herschel's work could be useful or relevant to understanding Darwin. I will restrict my focus in what follows to these two, for reasons of space and because they seem to be the most important to giving us a window on Darwin's work.

use of Herschel's philosophy in the *Origin* or Darwin's notebooks? I argue that we indeed can, based on a three-part reading of the *Origin* related to, but different in important ways from, the interpretation offered by Jonathan Hodge. Fourth and finally, we can confirm this new reading of Herschel's influence on Darwin via an analysis of Herschel's own criticism of Darwin. Herschel published little public critique of Darwin's theory (amounting to only a single footnote), but consultation of the marginalia that Herschel wrote in the copy of the *Origin* that Darwin sent to him is exceptionally instructive.

1. John Herschel's Philosophy of Science

John Herschel was, without a doubt, one of the most highly regarded figures of nineteenth-century science in Britain. His tomb at Westminster lies near Newton's (and next to Darwin's), and it is a safe bet that whenever a mid-nineteenth-century author refers to "one of our greatest natural philosophers," as Darwin did on the first page of the *Origin*, he is referring to Herschel.[2] His *Preliminary Discourse on the Study of Natural Philosophy* (Herschel 1830) is one of the central works of nineteenth-century British philosophy of science.

Unfortunately, philosophical scholarship on Herschel is profoundly lacking and has occurred mainly obliquely, in relationship to Darwin studies (Ruse 1975, 1976, 1978, 2000; Hodge 1977, 1992; Schweber 1985; Recker 1987; Lennox 2005; Hull 2009) and in the context of several PhD theses (Kavaloski 1974; Bolt 1998).[3] In addition, a fair bit of work on Herschel has been flatly confused, attempting to read Herschel as a naive Baconian inductivist—even more naive, it is said, than Bacon himself (e.g., Agassi 1981).

Bolt's characterization of Herschel's overall project provides us a place to begin. He notes that "far from being a thorough-going inductivist, John Herschel emphasizes not the process by which scientific theories arise but rather the manner in which one tests, draws conclusions from, and evaluates such theories" (1998, 41). Once we obtain a generalization, by whatever means available, Herschel then focuses on a carefully articulated account of how we *verify* such a hypothesis. As Bolt (1998, 287) notes, such an acceptance of hypotheses, by (apparent) contrast with Newton, formed a constraint on Herschel's writing of the *Preliminary Discourse*: he wished to support the wave theory of light, which seemed to many of his British readers precisely the sort of hypoth-

2. For biographical information on Herschel, see Cannon (1961) as well as introductions to reprints of the *Preliminary Discourse* (Partridge 1966; Fine 1987).

3. Two notable exceptions are Ducasse (1960) and Wilson (1974).

esis that Newton barred us from framing. Let us see, then, what Herschel considered to be the steps in proposing and evaluating a causal explanation.[4]

1.1. Hypothesis and Induction

How do we come up with a possible hypothesis in the first place? About this process, Herschel is not particularly concerned. We might use Baconian induction, gathering "an enumeration, if not complete, . . . at least of considerable extent, of [nature's] materials and combinations" (Herschel 1830, sec. 129), regulated and made more sophisticated by an extensive list of inductive methods that Herschel provides us (secs. 145–62). But if such a method is not available, simply arbitrarily proposing a hypothesis is acceptable. "We must not, therefore, be scrupulous as to *how* we reach to a knowledge of such general facts," Herschel argues; "provided only we verify them carefully when once detected, we must be content to seize them wherever they are to be found" (sec. 170).[5]

1.2. The *Vera Causa* Principle

We must therefore turn to the verification of a proposed hypothesis or induction. As a first constraint on the plausibility of a proposed cause, Herschel, in the tradition of Thomas Reid, turns to Newton's *vera causa* principle. Newton (1999, 794), as the first of his rules for philosophizing, wrote that "no more causes of natural things should be admitted than are both true and sufficient to explain their phenomena." Seizing upon the "true" in this phrase, as many before him had, Herschel sought to establish a minimal criterion for the plausibility of a putative cause. He introduces the term thus: "Experience having shown us the manner in which one phenomenon depends on another in a great variety of cases, we find ourselves provided, as science extends, with a continually increasing stock of such antecedent phenomena, or causes (meaning at present merely proximate causes), competent, under different modifica-

4. All explanation for Herschel is by definition causal: he claims that when we see a new phenomenon, we immediately look for "its *explanation*, or reference to an immediate producing cause" (Herschel 1830, sec. 83). I will cite the *Preliminary Discourse* from the text of the first edition, which Darwin owned (Darwin 1990), by section number, which is constant across the various reprints of the *Discourse*.

5. Note that this provides a substantial difference between the *vera causa* principle of Herschel and that of Thomas Reid. For one of the "foundation stones" of Reid's system "was his suspicion of, bordering on contempt for, any theories, hypotheses, or conjectures which are not *induced* from experiments and observations" (Laudan 1981, 89). This distinction is often missed (e.g., Hodge 1989, 171).

tions, to the production of a great multitude of effects, besides those which originally led to a knowledge of them. To such causes Newton has applied the term *verae causae*" (Herschel 1830, sec. 138).[6] A *vera causa* is thus one that has, on this definition, been shown (1) to exist and (2) to have produced other phenomena than those which originally led to its proposal.[7] We see the same definition of a *vera causa* when Herschel returns again to the subject later in the *Preliminary Discourse*: "[The causal agents in any theory] must be *verae causae*, in short, which we can not only show to exist and to act, but the laws of whose action we can derive independently, by direct induction, from experiments purposely instituted; or at least make such suppositions respecting them as shall not be contrary to our experience, and which will remain to be verified by the coincidence of the conclusions we shall deduce from them, with facts" (sec. 209). Note that this definition, in contrast with the first, makes Herschel's insistence on the production of phenomena other than those the cause was proposed to explain optional—if we cannot reach such breadth at the outset, we may content ourselves with merely proposing some cause that is analogous to a known cause and not "contrary to our experience." We will return to this point later.[8]

This, however, is *all* Herschel means by a *vera causa*. In attempting to link Herschel to Darwin, some writers have overemphasized the notion of a *vera causa*, claiming that it is somehow the end result of a successful process of scientific theorizing for Herschel (e.g., Ruse 1976, 122; Recker 1987, 161–62; Hodge 1992, 462). This is simply incorrect, as we can see by turning to Herschel's

6. For a careful explication of the various senses in which Herschel uses 'cause' (and hence an explanation of his reference in this quote to "proximate causes"), see Cannon (1961).

7. I thank an anonymous reviewer for noting that Newton's first rule, as stated, does not obviously include any reference to the production of phenomena other than those used to develop the theory. Herschel thus seems to be adding to Newton's original proposal when he elaborates the concept of *vera causa* in the *Preliminary Discourse*.

8. There is an interesting comparison to be drawn with Newton here. As William Harper and George E. Smith have each argued, Newton aimed to structure his mathematical derivations not just as equivalences between equations but as "theory-mediated measurement of parameters by the phenomena which they purport to explain," this stronger condition holding when the systematic dependencies between the equations are such that "tolerances to which the phenomenal magnitude is known to hold carry information about corresponding tolerances to which the inferred value of the corresponding theoretical parameter is known to hold" (Harper 2011, 128, 140). It is unclear whether Herschel intends something like this when he refers in the *Preliminary Discourse* to "verify[ing] the coincidence of the conclusions which we shall deduce from them, with facts" (sec. 209). Newton's stronger sense of this "coincidence" would make Herschel's bar here significantly higher. Casting doubt on such a parallel, however, is the fact that unlike Newton, Herschel seems much less concerned with inference from approximation (see Herschel 1830, sec. 227, where he argues that a sufficient number of observations can approximate the truth, or at least the truth as modified by any systematic bias).

examples.[9] In section 140, for instance, Herschel considers the possible effects on the earth's climate of a gradual decrease in eccentricity of the orbit of the earth. He calls the decreasing eccentricity an "astronomical *fact*" and claims that such a decrease would cause a drop in the amount of solar radiation absorbed by the earth. He concludes: "We have here, therefore, an evident real cause, of sufficient universality, and acting *in the right direction*, to account for the phenomenon. Its adequacy is another consideration" (Herschel 1830, sec. 130). This drop in eccentricity *is* a true cause, but it is far from a complete or verified explanation—we have not even yet determined, for example, whether it could possibly cause the phenomenon at issue.

Deeming something a *vera causa*, then, functions for Herschel as a minimal criterion for continued scientific exploration, and nothing more. Indeed, Herschel at one point describes our probability of success in the ascription of true causes almost as though we were merely checking through a list of our known *verae causae* against the facts at issue: this success will depend on "the number and variety of causes experience has placed at our disposal," "our habit of applying them to the explanation of natural phenomena," and "the number of analogous phenomena we can collect, which have either been explained, or which admit of explanation by some one or other of those causes" (Herschel 1830, sec. 141). It is thus clear that the establishment of a *vera causa* is only a first step toward a complete scientific explanation.

What about Herschel's famed relationship to the concept of analogy? Analogies are clearly important in the development of a hypothesis for Herschel. Shortly after the first quotation above, in which Herschel discusses our "continually increasing stock" of causes, he notes that "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" (Herschel 1830, sec. 142). We thus see that the *vera causa* principle invokes analogies in two different ways. First, it requires that we determine whether or not the causes in any given instance are analogous to some other, already known *vera causa* (as described above in Herschel 1830, sec. 138). Further, our success in finding the applicable *vera causa* will be aided by consulting our stock of "analogous instances," which allow us to expand the scope of application of a *vera causa*

9. I lack the space here to pursue an interesting and provocative claim by Bolt (1998, 527–28) and Kavaloski (1974) that, because most of these examples in secs. 138ff. are quite similar to those in Lyell's *Principles of Geology* (Lyell 1830), we might have reason to think that a large role was played by Lyell's *Principles* in "magnif[ying] the *vera causa* method initially motivated by [Herschel's] optical work" (Bolt 1998, 527).

from an initial known instance to the full collection of instances with which it is analogous.

When elaborating the connection between Darwin and Herschel, the latter is often sloganized as focusing only on analogies in scientific explanation. Ruse (1978, 324), for example, stops the argument at this point, claiming that "the key to a *vera causa* was an *analogy*" (see also Ruse 1975, 2000) and that analogy, in turn, is the key to understanding the relationship between Darwin and Herschel. But it should be clear from the preceding discussion that this is to substantially shortchange Herschel's philosophy of science. For the *vera causa* principle functions only as a sort of check on wild speculation, and analogy only lets us expand the scope of an already known *vera causa*.[10] Analogy therefore features in the earliest steps of Herschel's process, where we propose and evaluate the basic suitability of a hypothesis. It is not, by any stretch of the imagination, the linchpin of Herschel's philosophy of science.

1.3. Verification: Adequacy

How, then, are we to test a hypothesis, having deemed it to satisfy the *vera causa* criterion? Our first step in verification is quite simple. Herschel (1830, sec. 172) writes, "Whenever, therefore, we think we have been led by induction to the knowledge of the proximate cause of a phenomenon . . . our next business is to examine deliberately and *seriatim* all the cases we have collected of [the phenomenon's] occurrence, in order to satisfy ourselves that they are explicable by our cause." This is the notion of "adequacy" referred to by Herschel in the eccentricity example. Having a legitimate possible explanation—a *vera causa*—we must now turn to see whether or not the cause at issue could possibly produce the phenomena that we have proposed it to explain. We do this by consulting all known instances of the phenomenon, determining whether they give us reason to believe that the cause is in fact adequate to their production. This is the first step in the verification of an induction or a hypothesis. If it fails this test, it clearly cannot be the proper explanation.

We see here an instance of Bolt's apt depiction of Herschel as constantly alternating between the "inductive" and "deductive" modes of theory construction. Hypotheses are to be—or at least could be—initially formulated by inductive means, despite Bacon's having tarred overreliance on induction as one of the "idols." Such use of induction has, for Herschel, been justified by

10. Bolt (1998, 405) makes a similar point, arguing that "analogous instances played an important role for the production of hypotheses, but the fertility of an hypothesis" is where its full justification lies. See also Wilson (1974, 94).

a combination of the *vera causa* principle and his rules for inductive method. We then turn to *deduction*, however, when we ensure that all of the instances of the phenomena we wish to explain may be deduced from our hypothesized cause.

1.4. Verification: Consilience

Further, our deduction of consequences from our proposed cause must not stop at those phenomena we initially sought to explain, for "a law of nature has not that degree of generality which fits it for a stepping-stone to greater inductions, unless it be *universal* in its application. . . . Our next step in the verification of an induction must therefore consist in *extending* its application to cases not originally contemplated: in studiously varying the circumstances under which our causes act, with a view to ascertain whether their effect is general; and in pushing the application of our laws to extreme cases" (Herschel 1830, sec. 176). Universality, as Herschel calls it, is evidently an essential requirement for novel causes in his philosophy.[11] But more than simple universality, however, this sounds much like the concept of consilience, which deserves more explication here. Consilience is a notion almost always attributed to William Whewell, who coined the term in the second volume of his *Philosophy of the Inductive Sciences* (first published in 1840). He writes there that "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 hypothesis. . . . I will take the liberty of describing [this] by a particular phrase; and will term it the *Consilience of Inductions*" (Whewell 1847, 65). Although Herschel does not apply Whewell's term (as he could not, Whewell's work not being published until a decade after his),[12] he clearly has an idea much like consilience in mind—both in his first definition of a *vera causa*, when he mandates that we know a cause in ways "besides those which originally led to a knowledge of them" (Herschel 1830, sec. 138), and later, in this second manner of verifying a hy-

11. It is worthy of note that Darwin scored the margin next to this passage in his copy of the *Preliminary Discourse*, one of only a handful of passages that he annotated (Darwin 1990).

12. The history of the concept of consilience is carefully examined by Laudan (1971), who notes that as early as the late 1820s, a notion that appears very much like consilience was present as part of the *vera causa* criteria in Whewell's unpublished "Rules for Philosophizing." I thank an anonymous reviewer for noting that Herschel and Whewell may well have discussed the notion in the days before the publication of the *Preliminary Discourse*, making it exceptionally difficult to judge priority. Bolt cites a similar peculiarity in priority of discovery, noting that all of "Mill's Methods" are present in Herschel's *Preliminary Discourse*, again published some 13 years before Mill's *System of Logic*. Bolt (1998, 398) proposes that we rechristen them "Herschel's Habits."

pothesis. While I have no wish to pursue the priority claim here, noting the existence of consilience in Herschel will prove important when we come to evaluating his relationship to Darwin.

We can thus recap our observations regarding Herschel's view of the proposal and verification of a hypothesis. We begin with (1) hypotheses derived either by enumerative inductions or arbitrary proposition. The basic criterion of acceptability for hypotheses is that they (2) be grounded in *verae causae*, which, for Herschel, means that the causes in a given explanation either are or are analogous to causes that are known to exist. Now, how do we verify our putative hypothesis? We initially (3) ensure that the cause at issue is adequate to the production of the phenomena at issue—that is, that the cause could be responsible for the phenomena it was proposed to explain. Finally, we expand our search, via (4) consilience of this hypothesis with other and surprising data. If we have survived all these tests, we have produced a genuinely acceptable scientific explanation, by Herschel's lights.

An interesting allied question arises here: is Herschel's depiction of this process intended to be normative or merely descriptive of how good science is in fact performed?[13] For if Herschel's project is merely descriptive, then alignment between Herschel's edicts and Darwin's reasoning would be unsurprising—Herschel is describing how good science is done, and Darwin is doing good science. It is clear, however, that Herschel does indeed intend his depictions of the scientific process to be prescriptive. To begin, the very method of science itself is described by Herschel as a normative check on the mind's "tendency to rush at once upon its object, to undervalue the means in over-estimation of the end, and while gazing too intently at the goal which alone it has been accustomed to desire, to lose sight of the richness and variety of the prospects that offer themselves on either hand on the road" (Herschel 1830, sec. 9). Further, while Herschel certainly believes that he is drawing on the inductive evidence of history to produce his rules for scientific reasoning, these rules nonetheless have a normative character. As he puts the matter when summarizing the next section of the work to come,

> We shall state the helps which may be afforded us, in a work of so much thought and labour, by a methodical course of proceeding, and by a careful notice of those means which have at any time been found successful, with a view to their better understanding and adaptation to other cases: a species of mental induction of no mean utility and extent in itself; inasmuch as *by pursuing it alone* can we attain a more intimate knowledge

13. Many thanks to José Díez and Hasok Chang for raising this worry.

> than we actually possess of the laws which regulate our study of truth, and of the rules, so far as they extend, to which invention is reducible. (sec. 108; emphasis added)

The "helps" to reasoning to which we may have recourse as we study the sciences, that is, have not only been historically successful (as Herschel will illustrate with manifold examples throughout his work), but careful study of them is the only way in which we will be able to improve our knowledge of causes in the natural world.

If these are Herschel's methodological maxims, what are we to say about their application to Darwin's work? We must begin by gauging the extent of Darwin's exposure to Herschel's philosophy.

2. Darwin's Exposure to Herschel

In 1831, as Darwin was finishing the residency requirements for his Cambridge degree, he picked up a copy of Herschel's *Preliminary Discourse*, probably on the advice of his teacher, mentor, and friend, the botanist John Stevens Henslow (Ruse 1975, 164; Sloan 2009, 27). On February 15 of that year, he enthusiastically wrote to his cousin William Darwin Fox that "if you have not read Herschel in Lardners Cyclo—read it directly."[14] Although we must be cautious to take Darwin's later reminiscences from his *Autobiography* with a healthy serving of salt, he fondly remembered his first exposure to Herschel's work: "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* [the *Preliminary Discourse*] 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 1958, 67–68). Of course, this recollection is consistent with the view that, as Cannon (1976a, 118) put it, Darwin learned nothing from Herschel "more complicated than that it would be wonderful to be a scientist." Further evidence, however, indicates that Herschel was far more significant than this for Darwin.

The fall and winter of 1838 was a pivotal time for Darwin and the development of his theory.[15] He famously read Malthus on September 28 (Darwin

14. The text of this letter, as well as the texts of over 8,500 more, can be found on the website of the Darwin Correspondence Project (http://www.darwinproject.ac.uk). Herschel's *Preliminary Discourse* was published as the first volume in the *Cabinet Cyclopedia*, edited by Dionysius Lardner.

15. For a more detailed version of this chronology, see Hodge (1983a, 2009) and especially Ospovat (1981).

1838b, D 134–35),[16] drawing from him the important principle of superfecundity—the geometric multiplication of populations, faster than any possible arithmetic growth in available food and space. This principle would give rise to Darwin's "struggle for existence," one of the necessary ingredients, he claims, for natural selection. A couple of months later, on November 27, Darwin first writes about the mechanisms of differential reproduction and the inheritance of acquired characters, two central elements of his description of natural selection.[17] Differential reproduction grounds selective change, as fitter organisms will eventually outreproduce the less fit. The inheritance of acquired characters (sometimes, inaccurately, dubbed Lamarckism) allows for faster adaptation to local conditions by parents' transmitting characters obtained during their lives (like the strong arms of blacksmiths) to their children. Both were crucial to Darwin's understanding of how natural selection worked.

Only seven pages later, in the middle of this important theoretical period, we see the return of a reference to Herschel. Darwin writes of the mind of man (referring to Herschel's discussion of Bacon) that "it is (I presume—see p. 188 of Herschel's Treatise) a 'travelling instance' a—'frontier instance'" (Darwin 1838d, N 49).[18] We also find Herschel's *Preliminary Discourse* (again referred to by Darwin as "Herschel's Introd to Nat. Philosophy") on Darwin's "Books to Read" list—the last written date on the page preceding it is October 12, 1838, and across from the entry he notes "2d time of reading" (1838a, fol. 4v).

Only a few weeks afterward, on or just before December 2, his theory approaches its final structure, as he formulates the "three principles" grounding natural selection, which will remain the focus of his argument throughout his writing on evolution: "Three principles will account for all: (1) Grandchildren like grandfathers. (2) Tendency to small change especially with physical change. (3) Great fertility in proportion to support of parents" (Darwin 1838c, E 58). And, finally, somewhere between December 5 and 16, he compares predatory dogs with sporting dogs, the initial use of the analogy between artificial and natural selection, an analogy that will later form the first introductory chapters of the *Origin*: "If nature had the picking she would make them such a variety far more easily than man,—though *man's practised* judgment

16. References to Darwin's notebooks use the now standard lettering and pagination. Quotations from those notebooks, when they appear, use original spelling and punctuation, and they approximate original formatting.

17. "An habitual action must someway affect the brain in a manner which can be transmitted.—this is analogous to a blacksmith having children with strong arms.—The other principle of those children, which *chance?* produced with strong arms, outliving the weaker ones, may be applicable to the formation of instincts, independently of habits" (Darwin 1838d, N 42).

18. No date appears on this page; it must be from shortly after November 27.

even without time can do much" (1838c, E 63). We thus have all the central elements of Darwin's argument in the *Origin of Species* falling into place over the winter of 1838—and Darwin rereads Herschel's *Preliminary Discourse* in the middle of this crucial period.

Finally, the most direct evidence of Herschel's impact on Darwin comes from Darwin's letters. In a postscript to a letter to George Bentham, written on May 22, 1863, Darwin writes, "In fact the belief in natural selection must at present be grounded entirely on general considerations. (1) on its being a *vera causa*, from the struggle for existence; & the certain geological fact that species do somehow change (2) from the analogy of change under domestication by man's selection. (3) & chiefly from this view connecting under an intelligible point of view a host of facts."[19] At least in this letter, Darwin explicitly states that belief in natural selection is to be bolstered by its status as a (presumably Herschellian) *vera causa*.[20]

As is evident by the fact that my story about the *Origin*'s structure begins in 1838 and ends in 1863, I think that the argumentative structure of the *Origin* was broadly constant from the time it was laid down in the E and N notebooks, through the composition of Darwin's intermediate draft works known as the *Essay* and *Sketch*, and into the final version published as the *Origin of Species*.[21] Offering a full defense of this claim would take me too far afield here, but I may briefly appeal to a few citations in support of this claim. Ruse (2009, 7), for example, has argued that a structure heavily indebted to Herschel (and Whewell, about which more later) "is in the *Sketch*, the *Essay*, and the *Origin*—identical in form and presentation—and much of the evidence is just the same. Even the sub-bits, like the introduction of sexual selection along with natural selection, are the same."[22] I believe the balance of the evidence I offer here offers yet another argument in favor of this claim.

19. Darwin Correspondence Project (http://www.darwinproject.ac.uk).

20. As is common in Darwin studies, there is complicating evidence here. In a letter to Hooker dated February 14, [1860], Darwin laments that in a recent public lecture, Huxley "rates higher than I do the necessity of Natural Selection being shown to be a vera causa always in action." He claims, on the contrary, that "I have always looked at this doctrine of Nat. Selection as an hypothesis, which if it explained several large classes of facts would deserve to be ranked as a theory deserving acceptance; & this of course is my own opinion" (Darwin Correspondence Project [http://www.darwinproject.ac.uk]). Thagard (1977, 356) has argued that this means that Darwin was not in fact influenced by Herschel. I believe that this letter should, as discussed above, be read as emphasizing that the *vera causa* principle is only a very minimal criterion that any putative cause must meet.

21. I thank an anonymous reviewer for encouraging me to make this case more clearly.

22. Ruse (1979, 166–80) has argued for the same point elsewhere, as has Richards (1987, 99–105). The main voice of opposition here is Ospovat (1981, 191), who argues that in 1844, "the structure of Darwin's theory was largely determined by a network of assumptions" about perfect adaptation and natural harmony, while this structure was discarded by the writing of the *Origin*. I think this may well be

Thus Darwin, in the middle of what was for him a very fertile period of work on the fledgling theory of natural selection, returned to Herschel's *Preliminary Discourse*, and he later cited Herschel's notion of a *vera causa* as one of the foremost methodological credentials of natural selection. How, then, were these Herschellian themes expressed in his most significant work, the *Origin*?

3. Darwin's Argument in the *Origin*

In order to make the argument that the *Origin* is, in fact, structured along Herschellian lines, I must begin by considering alternative proposals concerning Darwin's philosophical influences. Darwin (1859, 459) famously claimed that the *Origin* constituted "one long argument," and there have been many attempts to clarify the nature of this long argument. Let us review a few of these in turn, proceeding in order of increasing methodological sophistication.

3.1. The Hypothetico-Deductive Model

We may begin with the claim that the *Origin* is a hypothetico-deductive theory, in the Hempelian sense (Hempel and Oppenheim 1948)—that Darwin intended to postulate "laws of nature" grounded on an inductive basis of phenomena, finally deriving predictions from these by logical deduction (Ruse 1971, 1975; Schweber 1985; Sober 1985). On this reading, Darwin begins by taking the "inductive data" he collected on artificial breeding and during his voyage on the *Beagle*, then infers the "laws" of differential fitness, Malthusian superfecundity, and so forth (in chaps. 1–3 of the *Origin*). He next attempts to derive from these laws both the claim that natural selection must occur (chap. 4) and further conclusions for other branches of the sciences (chaps. 10–13).

Assertions of this structure for the *Origin* are often linked to Newton, whose mechanics is supposedly the foremost instance of a hypothetico-deductive theory. Ruse (1975, 166), for example, argues that Darwin fully accepted "Newtonian astronomy as the paradigm for science" and that "his aim was to be the Newton of biology." He therefore consciously structured his theory in accordance with the hypothetico-deductive ideal.[23]

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right with respect to Darwin's thoughts about divergence and relative adaptation, but I agree with Ruse with respect to the overall argumentative structure.

23. For our purposes here, I leave aside recent work in Newton scholarship that conclusively demonstrates that Newton's own inferential method extends far beyond hypothetico-deductivism (Harper 2011).

Two considerations argue very strongly against this picture of Darwin's theorizing. First, the philosophical suitability of this model for looking at the *Origin* is questionable from the start. As Recker (1987, 151) argues, any application of the hypothetico-deductive model to Darwin's case will be necessarily sketchy, as Darwin lacked any understanding of the mechanism of heredity, something that ought to be among the most fundamental axioms of a hypothetico-deductive presentation of natural selection.[24] Further, Darwin himself argues that natural selection has not been proven in chapter 4 (the location of its supposed hypothetico-deductive derivation) but rather that "whether natural selection has really thus acted in nature . . . must be judged of by the general tenour and balance of evidence given in the following chapters" (1859, 127).[25]

Second, there exists a compelling historical reason to reject the hypothetico-deductive reading of the *Origin*. Darwin, at one point in his notebooks, does indeed consider a project that would have had such a structure and then abandons it. Hodge (2009, 54) writes about it at length:

> The structure of this prospective project was taken directly from the precedent set by the customary interpretation of the most prestigious physical science of the day: Newtonian celestial mechanics. This science was seen to have a threefold pyramidal structure. At the base were particular astronomical observations. . . . In the next level up were lawful generalisations about these motions. . . . These were descriptive not causal laws. Finally, at the top level there are causes: the lawful causes, the lawful forces of gravitation and inertia. . . . Darwin's promissory project was to have such a threefold structuring.

As mentioned, however, Darwin never completed this project. Natural selection, Hodge writes, "although arising from the lawful tendencies of heredity, variation, and superfecundity . . . was never seen by Darwin to have a law of its own" (68).[26] We can thus demonstrate that Darwin dabbled in the

24. Darwin's theory of pangenesis was probably worked out relatively early (Hodge 1985; Sloan 1985, 1986), but he clearly saw no need to argue for this theory in the *Origin*—it was not published until 9 years later in *The Variation of Animals and Plants under Domestication*.

25. See the similar and related arguments in Sloan (1986) and Hodge (1991). For more general considerations of the structure of the *Origin*, see Hodge and Kohn (1985).

26. Notably, in one of the first negative reviews of the *Origin*, Adam Sedgwick indicts Darwin for having failed to follow the hypothetico-deductive method. "I must in the first place," he writes, "observe that Darwin's theory is not *inductive*,—not based on a series of acknowledged facts pointing to a *general conclusion*,—not a proposition evolved out of the facts, logically, and of course including them" (1860, 285). At least some of Darwin's contemporaries, therefore, did not believe that his theory was struc-

hypothetico-deductive method and promptly proceeded to abandon it. It seems that we should as well.

3.2. An Analogy and a Consilience

Another reading has viewed Darwin as straightforwardly synthesizing two of his philosophical contemporaries. Herschel, as noted above, has occasionally been glossed as concerned above all with analogies. The traditional gloss on Whewell, in contrast, is that he stands concerned first and foremost with the "consilience of inductions"—the demonstration that many disparate lines of evidence may be unified under a single putative cause (Ruse 1978; Recker 1987). We can find both an analogy (with artificial selection, in chaps. 1–4) and a consilience (across many areas of science, in chaps. 10–13) in the *Origin*, and here we have an explanation for the structure of its argument.[27] Ruse (1978, 328) even goes so far as to claim that "the important thing is that Darwin, covering his options, wrote into his evolutionary theory both a Herschellian analogical *vera causa* and a [Whewellian] consilience."

This interpretation seems unsatisfactory as well. First of all, there is the implication that these are somehow two "separate" arguments for natural selection—as if we establish natural selection's bona fides twice, once by Herschel's criteria and once by Whewell's. We have already noted, however, that Darwin does not consider the argument for natural selection concluded at the end of chapter 4—we must rather consider the rest of the evidence for and against Darwin's proposal that comes in the remainder of the volume.

Second, it seems that Herschel's and Whewell's criteria for *verae causae* are at least occasionally contradictory, and thus it would be challenging (to say the least) to hold one's theory to both. Whewell (1847, 283) writes at length *against* Herschel's use of analogy, claiming that if all we want from a *vera causa* is "close similarity with some known kind of cause," then "no forces, or virtues, or sympathies, or fluids, or ethers, would be excluded by this interpretation of *verae causae*. Least of all, would such an interpretation reject the Cartesian hypothesis of vortices; which undoubtedly, as I conceive, Newton intended to condemn" by his first rule of philosophizing. Thus, Whewell's method would (at least on his own reading) disqualify Darwin's Herschellian analogy.

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tured as a generalization from an inductive base of observed facts—and believed that the theory was so much the worse off for this failing.

27. In this connection, Ruse (1975, 162) has oddly claimed that Herschel and Whewell "differed little, if at all, with respect to 'methodological' questions." Nothing seems to me to be further from the truth. For more information on the analogy in the first chapters of the *Origin*, see Sterrett (2002) and Largent (2009).

Next, this analysis seems to shortchange both Herschel and Whewell. To reduce Herschel to the single concept of "analogy," as was argued above, is to choose only one (and one relatively unimportant, at that) of Herschel's criteria for the proposal and verification of a hypothesis. Although I lack the space to pursue the claim fully, the same is true with the reduction of Whewell to "consilience." Whewell's neo-Kantian inclination, for example, is by now well known, and this side of Whewell's version of Newtonianism is entirely misrepresented by such an interpretation.[28]

Finally, and most importantly, we have a problem of anachronism here. As mentioned above, the argumentative structure of the *Origin* is laid down over the course of 1838 and remains relatively constant until the *Origin*'s publication. Whewell coined his notion of consilience in his *Philosophy*, which was not published until 1840.[29]

3.3. Common Descent without Natural Selection

One more important feature of Darwin's relationship to Herschel should be mentioned here. One might well be worried that emphasizing Darwin's relationship to Herschel, and hence the *vera causa* argument for natural selection, runs afoul of one pervasive feature of the response to the *Origin*. As we well know from Bowler's (1992) reconstruction of the period that has come to be known as the "eclipse of Darwinism," many of Darwin's critics rejected natural selection as an agent of change in the organic world while still accepting the truth of the common descent of all living organisms from a small number of ancestors. If Darwin's argument was structured around a *vera causa* claim for natural selection, how is such a reading possible? Does not the failure of the argument for natural selection mean that the entire edifice crumbles?

Sober (2009, 10054) has pushed the point further, claiming that for Darwin, while "selection has causal priority" in the sense that natural selection is the main causal driver of evolutionary change, "common ancestry has evidential priority." This is because the evidence for common ancestry (which appears in the third part of the *Origin*, covering examples drawn from the fossil

28. Wilson (1974, 80–83) presents the case quickly and straightforwardly. See also Ducasse (1960, 180). One of the best recent treatments of Whewell and Darwin that does some justice to the neo-Kantian angle is Richards's discussion in Richards and Ruse (2016).

29. As mentioned above, the only relevant published Whewell materials that predate the bulk of the construction of Darwin's argument are his reviews of Herschel's *Preliminary Discourse* and the first two volumes of Lyell's *Principles of Geology* (Whewell 1831a, 1831b, 1832), neither of which mention consilience, in name or in concept. The concept had appeared in Whewell's unpublished work, but I know of no evidence that Darwin would have had access to this material.

record, geographical variation, systematics, embryology, and more) provides the evidence that populations have, in fact, evolved across species boundaries, which then leads us to posit natural selection as a significant causal driver of that evolutionary change. Darwin thus, Sober (2009) provocatively claims, wrote the *Origin* "backwards."

What are we to make of these instances in which, apparently, common descent "outdoes" natural selection? I think the right response here is offered by Waters. Since the *Origin* gives us what Waters (2009, 141) calls "a flexible body of argumentation," we must be careful to keep a number of projects separate. Most importantly, the structure that Darwin intended the work to have does not, by any means, have to be the structure that every reader would have extracted from it. Those readers who rejected natural selection while still adopting common descent, Waters claims, found a different argument than the one Darwin had hoped they would: "By taking transmutation and common descent to be the cause of the various groups of phenomena that Darwin dealt with in the third part of the *Origin* (and in various sections of the second part), they could view the structure of the *Origin* as a Whewellian consilience of inductions" (141). The book's concluding chapters, on this reading, form a collection of independent evidence that supports common descent independently, regardless of its relationship with natural selection. And this is so whether or not Darwin intended it.

Sober's reading, similarly, may well be right with respect to "evidential priority"—that is, the evidence for common ancestry comes in the last third of the book, which grounds a claim about species (in the first few chapters), which in turn grounds a claim about natural selection as mechanism (in the middle section; see Pence et al. 2011). But again, this is a feature not of the book that Darwin wrote but rather the logical structure of the arguments contained therein—and the ability of that structure to be interpreted in a wide variety of ways, not necessarily conceived of by Darwin or related to his own project. Let us turn to an analysis that does uncover that project and is directly tied to a view of Darwin's relationship to Herschel.

4. The Relationship between Darwin and Herschel

In a remarkable series of papers, Hodge (1977, 1983b, 1987, 1989, 1992, 2000, 2009) has argued in great detail for a tripartite reading of the *Origin* and has garnered the agreement of a sizable portion of the Darwin studies community (e.g., Lennox 2005; Hull 2009; Lewens 2009; Waters 2009). Importantly for our purposes, his view of the *Origin* is premised on Herschel's *vera causa* principle. Let us begin with his reading of Darwin.

4.1. Hodge on Darwin

Hodge (1977, 239) describes the overarching argumentative structure of the *Origin* as follows, explicitly connecting it to the philosophy of Herschel and related concepts in Lyell: "We can see in Herschel's and Lyell's upholding of the [*vera causa* principle] the source for Darwin's taking up, in the *Sketch*, the following in turn: (i) the case for the *existence* of natural selection; (ii) the case for its *competence* to produce new species; and (iii) the case for its having been *responsible* for the production of extant and extinct species."[30] How do these three phases work in Darwin's thought, and to which parts of the argument in the *Origin* do they correspond?

First, the existence phase. This consists of the analogy in the *Origin*'s chapters 1–3. We know that the selective modification of species by artificial breeding is incredibly effective—this is a cause that we know to exist. Further, we can locate three features in the natural world—hereditary variation, differential reproduction, and the Malthusian pressure on population—that are sufficient to instantiate a similar selective breeding process in the wild. Thus, natural selection exists.

Second, the primary portion of the competence (or, elsewhere, adequacy) case comes in chapter 4 of the *Origin*, where Darwin argues that this selective breeding process, which is insufficient as applied in artificial selection to create new species, will be sufficient, given the much more extensive time with which natural selection has to work and the precision with which it can act, to produce new species that breed true. Then, chapters 5–8 attempt to undercut obvious objections to this adequacy thesis.

Third and finally, chapters 9–13 of the *Origin* constitute what Hodge (1992, 463) calls the responsibility phase, arguing that "natural selection . . . is more probable, and so is to be preferred over any rival theory because it is better than any other at explaining several kinds or classes of facts about those species: biogeographical facts, embryological facts and so on." Darwin's aim in these chapters, according to Hodge, is to make the case for natural selection having been the agent actually operative in the historical production of species on the earth.

Hodge cites much evidence in support of this reading of the *Origin*, far more than I could do justice to here. This explanation makes sense of Darwin's early work in his notebooks prior to the development of natural selection (Hodge 1983a), it explains Darwin's rereading of Herschel in late 1838 (Hodge 1983b), it can help us understand Darwin's growing focus on his young theory as "publishable, public science" (Hodge 2009, 59), and it can help explain

30. Precisely the same division is clearly expressed in Hodge (1992).

Darwin's methodological or philosophical enthusiasm for Lyell (Hull 1983; Hodge 1987). All these positive cases, I agree, are quite valid.

However, Hodge mistakes Darwin's motivations, especially in the responsibility phase of the argument—and this misunderstanding is due to a small but significant misreading of Herschel. Let us look, then, at Hodge's view of the connection between these three phases of argument and Herschel's work.

4.2. Hodge on Herschel

Hodge, throughout his reading of Herschel, remains focused on the *vera causa* principle. Darwin's three phases of argumentation, insofar as they are traceable to Herschel, are supposed to derive entirely from a straightforward explication of Herschel's use of *verae causae*. Hodge (1977, 239) writes that "we may take the whole [*vera causa*] rule or principle to specify the following: in explaining any phenomenon, one should invoke only causes whose *existence* and *competence* [or *adequacy*] to produce such an effect can be *known* independently of their putative *responsibility* for that phenomenon."[31]

These three phases are thus, on Hodge's reading of Herschel, elements of what it is to claim that something is a *vera causa*. Put differently, Darwin's goal—what he has taken from his reading of Herschel—is to establish that natural selection is a *vera causa*. Thus, he must establish its existence, adequacy, and responsibility. Based on the presentation of Herschel in section 1, I am quite doubtful that this is an accurate reading of Herschel's methodological prescriptions. First, as has already been discussed at length in section 1.2, the *vera causa* principle does not constitute a goal or end of scientific theorizing for Herschel. On the contrary, it is a very early and very low bar that any putative explanation must clear.

Second, even if this were the role of the *vera causa* principle, this three-part structure is an inadequate reading of what Herschel means by a *vera causa*. To begin, we know from Herschel's example of the declining eccentricity of the earth's orbit that adequacy is an issue entirely orthogonal to whether or not a cause is a *vera causa*. Herschel (1830, sec. 140), recall, writes of this drop in eccentricity that "we have, therefore, an evident real cause, of sufficient universality, and acting *in the right direction*, to account for the phenomenon. Its adequacy is another question."

31. Hodge (1977, 1989) also gives, along with this three-part reading of the *Origin*, a two-part reading: first, "natural selection established as [*vera causa* principle] cause for species" in chaps. 1–3, and then "natural selection as, on balance, probably responsible for species" in the rest of the book (1977, 243). This collapses the existence and adequacy phases into one "*vera causa*" phase. My arguments will apply equally well, in general, to either of these readings.

Third and finally, responsibility in Hodge's sense is not a necessary requirement for the postulation of a hypothetical cause in Herschel's philosophy. This responsibility requirement might derive from the first definition of *vera causa* in section 138 of Herschel's *Preliminary Discourse*, where Herschel writes that such a cause is "competent, under different modifications, to the production of a great multitude of effects, *besides those which originally led to a knowledge of them*" (emphasis added). But Herschel's account of the process by which we establish hypotheses, as we have seen it worked out in his examples, is less strict than the emphasized portion of this quotation might lead us to believe. We must have, eventually, this sort of "consilience" in order to possess a genuine causal explanation—it is one of the later steps in the verification of causal explanations in Herschel's system (and Darwin did believe he could demonstrate it, about which more later). But we need not have this sort of consilience when we propose a given causal agent and satisfy the *vera causa* principle. As noted above, Herschel says in his later, second definition of a true cause that a new proposed cause must be one either "the laws of whose action we can derive independently" or "at least make such suppositions respecting them as shall not be contrary to our experience, and which will remain to be verified by the coincidence of the conclusions we shall deduce from them, with facts" (1830, sec. 209). The first of these two possibilities corresponds to Hodge's responsibility and Herschel's statement from section 138. The second, I argue, is not only different but is the sense in which Darwin intended to propose natural selection when he initially engages with the *vera causa* principle. Without an account of heredity, it is hard to believe that Darwin would have thought we could independently derive the laws of action of natural selection.

Hodge therefore seems to slightly, yet significantly, misread Herschel. As I argued above, however, his tripartite reading of the *Origin* seems correct. Let us now try to combine Hodge's reading of Darwin with the elucidation of Herschel above, constructing a better view of the mark Herschel's philosophy left upon Darwin.

4.3. A New Reading of Herschel and Darwin

We can, I believe, profitably build a "Herschellian" reading of the *Origin* based on Hodge's three-part analysis of Darwin's argument and a cautious picture of Herschel's philosophy of science.[32]

32. As an anonymous reviewer notes, there may be ways to weaken this tripartite reading of the *Origin* without substantively altering my argument here. For example, the role of chap. 2 in that reading is somewhat disputed—it might be read simply as providing data for Darwin's claims in surrounding chapters. I lack the space to explore any of those possibilities here.

First, consider the analogy between artificial and natural selection in the *Origin*'s first three chapters, or what Hodge calls the existence phase. This, as I have shown, corresponds quite precisely to Herschel's narrowly drawn *vera causa* principle. Darwin is attempting to show here that natural selection is worthy of further study—that it is a *vera causa*. As I have argued extensively, this is, for Herschel, a necessary condition that an explanatory hypothesis must pass "to qualify it for a *vera causa* available in sound philosophy" (Herschel 1830, sec. 138)—and nothing more. Once we have such a cause, we can add it to the list of those that "experience has placed at our disposal" (sec. 141).

Darwin's argument proceeds as we would expect, according to Herschel's instruction in the *Preliminary Discourse*. He collects "analogous instances" from pigeon, dog, and horse breeding, as well as the "unconscious selection" of "the lowest savages" (Darwin 1859, 34). He attempts to bolster the analogy by as many means as he has available to him: as Sterrett (2002) argues, he draws an analogy between "methodical selection" and the principle of divergence, as well as a separate analogy between "unconscious selection" and the principle of extinction. Knowing full well that natural selection could not, in his day, be directly observed, he was forced to avail himself of Herschel's secondary reliance on hypothesis and analogy. Indeed, Herschel (1830, sec. 142) argued at one point that "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." Darwin seemed to earnestly desire to present this forceful of an analogy as the *Origin*'s opening argumentative move. Whatever one might go on to say about the adequacy of natural selection or its ability to explain a broad base of biological facts, Darwin did not want it to seem as fanciful as an ether or the notion of "progress" present in Chambers's *Vestiges of the Natural History of Creation* (Schwartz 1990).

Now, as Herschel (1830, sec. 140) tells us, "its adequacy is another question." Making the adequacy case is the goal of the fourth through ninth chapters.[33] Contra Hodge, however, this is not part of establishing something as a *vera causa* (Darwin has already completed that task) but rather the first step in the verification of Darwin's hypothesis. We thus have a separation between chapters 3 and 4. By the end of chapter 3, Darwin has provided enough evidence to demonstrate that natural selection is a legitimate hypothesis with which we are permitted to work. Next, Darwin turns to the verification of the hypothesis as proposed. Kavaloski, although he only elaborates on this claim very minimally,

33. Here lies a near agreement with Hodge, who claims the same thing about chaps. 4–8. I think that the ninth chapter on geological difficulties belongs better to this adequacy case than with those that come after it. Bowler (1996, 122) makes the same case.

agrees, arguing that the first chapters are "primarily interested in establishing the elements of his theory . . . as *verae causae*," and the rest is "the empirical *testing* of the theory" (Kavaloski 1974, 122–23).

Thus, Darwin must next argue that natural selection operates both in the right direction and with suitable intensity to have been able to produce the array of species that we now find. The goal, that is, is to establish that natural selection could in fact have produced the phenomena that it was proposed to explain. This is exactly how Darwin proceeds in the fourth through ninth chapters, discussing first the conditions under which natural selection might have produced species, genera, families, and so forth, via the working of the principle of the divergence of character, the ways in which variation may be expected to act, and then subverting possible objections to its adequacy from the evolution of highly specialized organs, instinct, hybrids, and the imperfection of the geological record, among others. Again, we see in this phase of Darwin's argument exactly what we would expect by Herschel's lights: having justified natural selection as a *vera causa*, he proceeds to Herschel's first step in the verification of a hypothesis and establishes its adequacy.

Finally, the tenth through thirteenth chapters clearly constitute a consilience—but not, as several have argued, a Whewellian consilience (on pain of anachronism), nor, as in Hodge, the sort of consilience (or "responsibility") that sometimes might justify a *vera causa* (on pain of contradicting Herschel's presentation of consilience). Rather, this is, as Herschel argues, the natural next step in the verification of a hypothesis once it has been successfully proposed and proven adequate to the phenomena at issue. "Cases not originally contemplated," as Herschel (1830, sec. 176) describes them, are precisely the subject matter of this last portion of the *Origin*, in marked and notable contrast to the earlier argument for natural selection's adequacy. Darwin moves here to areas as diverse as geology, biogeography, classification, morphology, and embryology. By the eleventh chapter, Darwin (1859, 352) already feels confident to say that "he who rejects [the claim that each species was created at only a single point on the globe], rejects the *vera causa* of ordinary generation with subsequent migration, and calls in the agency of a miracle."[34]

We can see as well an echo of a piece of advice from Herschel that we have not yet had occasion to discuss. Herschel (1830, sec. 180) argues (again, presaging Whewell) that we should search for confirmations of a theory "among instances of that very kind which were at first considered hostile to [it]." Ruse (1975, 2000) and Hull (2009) have noted that this explains several of Darwin's

34. To anticipate our later discussion of Herschel's response to Darwin, in his own copy of the *Origin*, Herschel scores this passage in the margin and unhappily marks it with a large *X*.

comments to the effect that his explanation of embryology was the most significant of all his evidence—"my pet bit in my book," as he once referred to it.[35] It should indeed be seen as weighty, on Herschel's criterion: embryology, as it was often understood in Darwin's day (with focus on the progression inherent in development), would more naturally be read as favoring a Lamarckian view of species transformation as opposed to a Darwinian one.[36]

5. Herschel's Response to Darwin

We now must turn to Herschel's own response to Darwin's work. Privately, as is often quoted, Darwin wrote to Charles Lyell on December 10, 1859 (some 2 weeks after the publication of the *Origin*), that "I have heard by round about channel that Herschel says my Book 'is the law of higgledy-pigglety.'—What this exactly means I do not know, but it is evidently very contemptuous.—If true this is great blow & discouragement."[37] Darwin speaks of very few other criticisms of his work in these sorts of terms—he was clearly deeply stung by Herschel's rejection. We lack sufficient evidence here, however, to see why Herschel would have objected to Darwin's argument.

Publicly, Herschel's criticism was limited to a single footnote (added in January 1861) in section 11 of the book-length version of his *Encyclopedia Britannica* article on physical geography (Herschel 1861). There are two interwoven arguments here (Bolt 1998). First is Herschel's claim that

> we can no more accept the principle of arbitrary and casual variation and natural selection as a sufficient account, *per se*, of the past and present organic world, than we can receive the Laputan method of composing books (pushed *a l'outrance*) as a sufficient one of Shakspeare and the Principia. Equally in either case, an intelligence, guided by a purpose, must be continually *in action* to bias the directions of the steps of change—to regulate their amount—to limit their divergence—and to continue them in a definite course. We do not believe that Mr. Darwin means to deny the necessity of such intelligent direction.

This argument constitutes merely a minor correction. That is, Herschel does not here object to the form of natural selection qua law or secondary cause

35. Letter to J. D. Hooker, December 14, [1859]. Darwin Correspondence Project (http://www.darwinproject.ac.uk).

36. See, e.g., the presentation of the developmental context to early Darwinian theory in Sloan (1986).

37. Darwin Correspondence Project (http://www.darwinproject.ac.uk).

but rather to the fact that Darwin has failed to emphasize that the acting out of this law still requires the active intervention of a higher power. Darwin believed this to be an exceptionally unfair criticism, leveled at him as it was by a variety of commentators, including perhaps most forcefully Sedgwick. In a letter to Henslow, he wonders "whether it was not allowable (& a great step) to invent the undulatory theory of Light—ie hypothetical undulations in a hypothetical substance the ether. And if this be so, why may I not invent hypothesis of natural selection . . . & try whether this hypothesis of natural selection does not explain (as I think it does) a large number of facts."[38] If the wave theory of light required no reference to a creator and violated none of the canons of sound inductive reasoning, why, Darwin protests, does natural selection?

Herschel's footnote continues, however, and the second half strikes right at our discussion so far:

> But [intelligent direction] does not, so far as we can see, enter into the formula of his law, and without it we are unable to conceive how the law can have led to the results. On the other hand, we do not mean to deny that such intelligence may act according to a law (that is to say, on a preconceived and definite plan). Such a law, stated in words, would be no other than the actual observed law of organic succession; or one more general, taking that form when applied to our own planet, and including all the links of the chain which have disappeared. But the one law is a necessary supplement to the other, and ought, in all logical propriety, to form a part of its enunciation. Granting this, and with some demur as to the genesis of man, we are far from disposed to repudiate the view taken of this mysterious subject in Mr. Darwin's work.

The objection here seems to be that while Darwin might well have offered a *vera causa*—a cause that is indeed acting and has affected the history of life—he has failed to offer a sufficient adequacy case, as the law cannot, Herschel argues, be believed to have produced the phenomena at issue.

What kind of law would have been acceptable by Herschel's lights? It is unclear from his rather cryptic footnote. Herschel seems to indicate that it would have to encompass, at the very least, all of the laws and causal processes behind the generation of variations. His reference to the "actual observed law of organic succession," and his insistence that such a law would need to include "all the links of the chain which have disappeared," seems almost to imply that

38. Letter dated May 8, [1860]. Darwin Correspondence Project (http://www.darwinproject.ac.uk).

this law would enumerate the entire list of variations that led to divergence throughout the history of life. It is difficult to see how such a law would satisfy Herschel's own canons of sound scientific reasoning, which makes Herschel that much more difficult to interpret here.[39]

This is, however, still a fairly sketchy complaint couched in a vague footnote. Even the extrapolation that Herschel is concerned with laws of variation (however Herschel intends the "actual observed law of organic succession; or one more general") is difficult to support on this thin amount of textual evidence.[40]

Clarity may be gained by turning to Herschel's own marginalia in his copy of the *Origin*, sent to him by Darwin.[41] The *Origin* was the only one of Darwin's books present in Herschel's library at Collingwood, and Herschel marked up the volume fairly extensively, concerned more than anything, it seems, with locating places where Darwin's exposition appears to contradict itself.[42]

In the first few chapters of the *Origin*, we see Herschel examining the worry that would make up his first argument—the location of the active power driving natural selection. In chapter 1, Herschel underlines Darwin's claim that "*nature* gives successive variations; *man adds* them up in certain directions useful to him. In this sense ***he*** *may be said to* ***make*** for himself useful breeds" (30).[43] In the fourth chapter, he marks many instances of Darwin's attribution of activity to nature itself: "*She can act* on every internal organ" (83), "Man selects only for his own good; Nature only for that of the being which *she tends*" (83), "***should plainly bear the stamp of far higher workmanship?***" (84), all of these passages marked with a "C" in the margin that appears to be Herschel's code for "contradiction."

39. See, for a similar point, Bolt (1998, 593–94).

40. An anonymous reviewer notes that it is telling that, unlike much of Herschel's writing, which is among the most graceful and lucid of nineteenth-century British scientific prose, the writing in this footnote is unusually poor, indicative of the difficulty Herschel had in formulating this response.

41. Inscribed "From the Author" on the first page. The volume is now present in the Herschel Family Archive at the Harry Ransom Center, University of Texas at Austin. My thanks to Stephen Case for providing these marginalia, which I have edited and released in the public domain at https://github.com/cpence/herschel-origin-marginalia.

42. Herschel also owned an offprint of one of Darwin's geological papers. As for other reviews of and responses to Darwin, the Collingwood library catalog confirms that he owned (although I cannot say whether he read) Auguste Laguel's review in the *Revue des deux mondes* (1860), Andrew Murray's review in the *Proceedings of the Royal Society of Edinburgh* (1860), Richard Owen's *Palaeontology* (1860), which included a four-page critique of transformationism, and Robert Mackenzie Beverley's critique of Darwin (Beverley 1867), which was printed after Herschel's *Physical Geography* footnote and thus was not likely to have substantially altered Herschel's opinion (Ross 2001, 135, 151, 165, 383).

43. Herschel's single underlines are indicated by italics; his double underlines, by bold and italics.

In the fifth chapter, then, we see Herschel's more substantive complaint. In an annotation beneath the last paragraph of the chapter on page 170, Herschel writes, "D. recognizes an unknown cause of slight individual differences—but claims for 'natural selection' the character of a 'sufficient theory' in regard to the results of those differences." The reference to Darwin's recognizing an unknown cause for variation is not a direct quotation from the *Origin* but a summary of several such claims made by Darwin. For example, Darwin notes at the beginning of chapter 4 "our ignorance of the cause of each particular variation," including both "individual differences" and "monstrosities" (131). Another such claim has been marked by Herschel on the same page as his annotation, in the last paragraph of chapter 5. Herschel underlines Darwin's claim that "*a cause for each* [variation] *must exist*," setting it against the double-underlined claim that natural selection "***gives rise*** to all the more important modifications of structure" (170). Which is it, Herschel seems to ask—the causal production of the variations or natural selection—that holds primary responsibility for the observed outcomes? And if the correct answer is "both," then how can we leave the cause of variation unknown?

Here, I think, we see very clearly the substance of Herschel's objection. In the pivotal fifth chapter, Darwin has turned to the causes and character of variation, so important for his defense of the adequacy of natural selection. Herschel, quite simply, rejects the idea that any theory of organic change could possibly be adequate—that the theory could be believed to have produced the observed phenomena—without a description of how the actual process of variation could have produced the history of life. And to emphasize the point, this critique occurs at exactly the point in the *Origin* where we would expect it on the reading developed here: Herschel objects to Darwin's claim of adequacy for natural selection during the exposition of the evidence for that very adequacy claim, at the end of the fifth chapter.[44]

I should respond to one important objection here before I conclude.[45] Several commentators have argued that Herschel's primary objection to Darwin concerned not the adequacy of Darwin's causal story for variation and adaptation but rather the rejection of design. Ruse (1975, 180), for instance, writes that "[Herschel and Whewell] both felt that Darwin had failed to do what any

44. Note as well that this must be a rejection of the adequacy claim, not the *vera causa* claim, as the *vera causa* principle, as we have seen above, is meant to be a check on wildly speculative causes, while nothing in the tone of Herschel's objections here seems to indicate that he believes that Darwin is engaging in undue speculation.

45. Thanks to an anonymous reviewer for encouraging me to tackle this argument in further detail.

good biological theorist *must* do, pay adequate recognition to the role of God's Design in the formation of organisms."[46]

It is true, of course, that Herschel does refer to the absence of active divine power from Darwin's picture, particularly in the first portion of the *Physical Geography* footnote. But several points weigh against this being the primary way in which we should understand Herschel's rejection of Darwin. First, and perhaps most importantly, it is clear from the remainder of that footnote that Herschel does not actually believe this to be a fatal objection. "We do not believe," Herschel (1861, sec. 11 n.) says, "that Mr. Darwin means to deny the necessity of such intelligent direction," only that he has failed to make its presence sufficiently patent in his presentation of natural selection's action as a secondary cause.

Second, even in the pieces of Herschel's marginalia to the *Origin* where he is discussing natural theology, it does not seem as though his objections are about God's plan for or design of the features of organisms. Most of his annotations concern worries about the location in the causal structure of the power driving natural selection. Darwin's move between the power of man to select characters and produce breeds and the power of an unspecified "Nature" to effect the same change seems prima facie incoherent to Herschel—only an appeal to something like the workings of God underlying that secondary cause (as Herschel himself had already done in the case of Newtonian forces, for example) could successfully ground such an analogy.

Finally, it is far more significant to Herschel that we lack a causal story for variation. Only this worry gets a fully fleshed-out complaint in Herschel's copy of the *Origin*, and Herschel here simply believes Darwin's story to be incomplete. It is impossible for a putative cause of adaptations to be adequate if it requires natural selection to work in concert with variation and yet leaves the causal explanation of variations unspecified.

There is a connection between these two strands of Herschel's thought, of course—for Herschel, limiting ourselves to proper *verae causae* that are in turn adequate to the production of the phenomena and able to explain further observations is how we ensure that we only entertain causes in nature that are acceptable by the lights of Herschel's natural theology.[47] The failure of Darwin's theory to make an adequacy case thus has a theological upshot—as Hull (1973, 61) puts it, "Herschel did not want to deny that evolution might occur by law, but it had to be a law worthy of God." But nothing here changes the

46. The same narrative appears in Hull (2009, 186–88).

47. I thank another anonymous reviewer for noting this connection.

fact that the problem with Darwin's theory is the treatment of variation (or the lack thereof).

6. Conclusions

This, then, is the appropriate way to view the influence of Herschel's methodology on Darwin. In taking Hodge's tripartite reading of the *Origin*, we retain all its advantages, particularly a deeply contextualized reading of the *Origin*'s development, with connections to Darwin's work in the notebooks and the early *Essay* and *Sketch*. But as I have argued above, Hodge founds this tripartite structure for the *Origin* on a misreading of Herschel's philosophy, overemphasizing Herschel's use of the *vera causa* principle and eliding over the distinction Herschel draws between the proposal of a hypothesis (including its satisfying the *vera causa* criterion) and its subsequent verification.

It is, however, impossible to argue that Herschel's thought on methodology did not influence Darwin. We have seen Darwin's argument unfold in precisely the way that we would expect given a desire to hold oneself to Herschel's methodological canons. Darwin begins by proposing a speculative hypothesis, grounded on an extensive analogical basis. He then sequentially follows Herschel's steps for the verification of that hypothesis, first demonstrating its adequacy and then its ability to account for a wide variety of phenomena that it was not originally proposed to explain.

While Herschel, then, has multiple responses to Darwin's theory, the most important of these fits in neatly with this understanding of the structure of the *Origin*. In response to the heart of Darwin's claim that natural selection is adequate to produce the observed phenomena concerning the history of life, Herschel states that no theory could be adequate without one crucial piece that Darwin lacked: a causal understanding of the generation of variations.

Darwin's relationship to the various currents of influence in nineteenth-century thought still remains difficult to elucidate in full detail. Whewell's *History of the Inductive Sciences* may well have been influential late in the development of the *Origin*, and the arguments put forward connecting Darwin to German Romanticism are compelling. The influence of Herschel, supported both historically and textually, however, is undeniable. All the more devastating must Herschel's criticism of Darwin's theory have been—and primarily criticism on methodological grounds, no less. Regardless of Herschel's reaction to Darwin's work, we can see clearly that Darwin intended to structure his argument as acceptable by Herschel's criteria—the criteria of, to reiterate Darwin's own characterization, one of our greatest philosophers.

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