Charles Darwin and Sir John F. W. Herschel: Nineteenth-Century Science and its Methodology

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Whatever else one might say about Charles Darwin's *On the Origin of Species* (1859), it has been claimed time and again that a large part of its impact was methodological. James 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 busy 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 was incredibly multi-faceted (Sloan 2009; Manier 1978), 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 (Hodge 1977; Ruse 1975), that Darwin was not influenced by Herschel (Desmond and Moore 1992; Thagard 1977; Cannon 1976a, 1976b), that Darwin was influenced by Whewell (Ruse 2000, 1978; Curtis 1987), that Darwin was not influenced by Whewell (Hodge 1989, 1991, 2000), or that Darwin was influenced by Lyell (Hodge 1983a, 1983b, 1990, 2009), Comte (Schweber 1997), or German Romanticism (Richards 2002, 2008; Sloan 2001). Of course, these various claims (or at least the non-contradictory ones) are not necessarily mutually exclusive. It suffices to say, however, that the waters in this realm are rather muddied.

In this essay, I will endeavor to clarify at least one of these relationships – that between Darwin and Sir John Herschel. The central question, of course, 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 three issues. First, what can we say for certain, historically, 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. Second, what was Herschel's philosophy of science, at least with regard to the formation and verification of

hypotheses and theories like Darwin's? On this point, there has been surprisingly little scholarship. My reconstruction will look at 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. Finally, can we see any evidence of the 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* derived from the work of M. J. S. Hodge. The form of this influence, however, is notably different from that previously ascribed in work on Darwin.

1. Darwin's Exposure to Herschel

In 1831, as Darwin was finishing the residency requirements for his Cambridge degree, he picked up a copy of John Herschel's *A Preliminary Discourse on Natural Philosophy* (Herschel 1830), probably on the advice of his teacher, mentor, and friend John Stevens Henslow (Ruse 1975, 164; Sloan 2009, 27). On the 15th of February, he enthusiastically wrote to his cousin William Darwin Fox that "If you have not read Herschel in Lardners Cyclo – read it directly."[1] Although we must be cautious to take Darwin's later reminiscences from his *Autobiography* with a healthy serving of salt, he fondly remembered this 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, 67-8)

Of course, this recollection is consistent with the view that, as Cannon put it, Darwin learned nothing from Herschel "more complicated than that it would be wonderful to be a scientist" (Cannon 1976b, 118). For this we must look later.

The fall and winter of 1838 was a pivotal time for Darwin.[2] He famously read Malthus on the 28th of September, as we see in his D notebook (D 134-5, Darwin 1838c), drawing from him the important principle of superfecundity – the geometric multiplication of populations, faster than any possible growth in available food and space. A few months later, on the 27th of November, he first writes about his twin

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[1] Darwin Correspondence Project Database, Letter 94, 15 Feb. 1831. Herschel's *Preliminary Discourse* was published as the first volume in the *Cabinet Cyclopædia,* edited by Dionysius Lardner.
[2] For a more detailed version of this chronology, see Hodge (2009, 1983a).

mechanisms of differential reproduction and the inheritance of acquired characters (in his N notebook), which would remain two central elements of his description of natural selection.[3] Only seven pages later, 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'" (N 49, Darwin 1838a).[4] 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 12th of 1838, and across from the entry he notes "2d time of reading" (Darwin 1838b, 4v).

Only a few weeks afterward, on or just before the 2nd of December, 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 (E 58, Darwin 1838d)

And, finally, somewhere between the 5th and 16th of December, he compares predatory dogs with sporting dogs, the initial use of the analogy between artificial and natural selection: "If nature had the picking she would make ~~them~~ such a variety far more easily than man, – though *man's practised* judgment even without time can do much" (E 63, Darwin 1838d). 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 re-reads Herschel's *Preliminary Discourse* in the middle of this crucial period.

Lastly, 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 in 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

[3] "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" (N 42, Darwin 1838a).
[4] No date appears on this page; it must be from shortly after the 27th of November.

> by man's selection. (3) & chiefly from this view connecting under an intelligible point of view a host of facts.[5]

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.*[6]

It is thus certain that 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. Let us consider what exactly Darwin might have found in Herschel's work.

2. 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 next to Newton's, and it is a fair 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* (1859, 1), he is referring to Herschel.[7] Unfortunately, philosophical scholarship on Herschel is profoundly lacking, and has occurred mainly obliquely, in relationship to Darwin studies (Ruse 1975, 1976, 1978, 2000; Schweber 1983; Recker 1987; Lennox 2005; Hull 2009; Hodge 1977, 1992) and in the context of several Ph.D. theses (Bolt 1998; Kavaloski 1974; two notable exceptions are Ducasse 1960 and Wilson 1974). 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 (Agassi 1981).

Marvin 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" (Bolt 1998,

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[5] Darwin Correspondence Project Database, Letter 4176, 22 May 1863.

[6] As is common in Darwin studies, there is complicating evidence here. In a letter to Hooker, 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 Database, Letter 2696, 14 Feb. [1860]). Thagard (1977, 356) has argued that this means that Darwin was not in fact influenced by Herschel. I believe that this letter should, as I will discuss with regard to the philosophy of Herschel, be read as emphasizing that the *vera causa* principle is only a *very minimal* criterion that any putative cause must meet. For more information, see the discussion of the *vera causa* principle in section 2.2.

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

41). That is, Herschel emphasized the "careful *interplay* between the inductive and deductive moments...deriving consequences from the generalizations however derived, either via inductive generalizations, which he advocated, or via hypothesized theories, which he also encouraged" (Bolt 1998, 47). As Bolt notes (1998, 287), such an acceptance of hypotheses 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 hypothesis that Newton barred us from framing. Let us see, then, what Herschel considered to be the steps in proposing and evaluating a causal explanation.[8]

2.1. 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" (*PD* §129), regulated and made more sophisticated by an extensive list of inductive methods which Herschel provides us (*PD* §§145-162). But if such a method is not available, simply arbitrarily proposing a hypothesis is acceptable. "[W]e 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" (*PD* §170).[9]

2.2. 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, 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" (Newton 1999, 794). 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:

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[8] 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" (*PD* §83). I will cite the *Preliminary Discourse* (Herschel 1830) from the text of the first edition, which Darwin owned (Darwin 1990), by section number, which is constant across the various reprintings of the *Discourse.*

[9] Note that this provides a substantial difference between the *vera causa* principle of Herschel and that of 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).

> 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 modifications, 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*... (*PD* §138)[10]

A *vera causa* is thus one that has, on this definition, been shown (i) to exist, and (ii) to have produced *other* phenomena than those which originally led to its proposal. We see the same definition of a *vera causa* when Herschel returns again to the subject later in the *PD:*

> [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 which we shall deduce from them, with facts. (*PD* §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 which is analogous to a known cause and not "contrary to our experience." We will return to this point later.

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., Recker 1987, 161-2; Hodge 1992, 462; Ruse 1976, 122). This is simply incorrect, as we can see by turning to Herschel's examples.[11] 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

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[10] 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).

[11] I lack the space here to pursue an interesting and provocative claim by Bolt (1998, 527-8) and Kavaloski (1974) that, because most of these examples in §§138ff. are quite similar to those in Lyell's *Principles of Geology* (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).

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" (*PD* §140). This drop in eccentricity *is* a true cause, but it is far from a complete or verified explanation – we haven't 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. As Bolt notes, a given *vera causa* "may not be causal for the phenomenon in question, and may not be sufficient for the production of that phenomenon either in terms of its intensity or even in terms of the direction of its action" (Bolt 1998, 383-4). Indeed, Herschel at one point describes our probability of success in the ascription of true causes almost as though we were merely checking 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 "on 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" (*PD* §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?* When elaborating the connection between Darwin and Herschel, the latter is often sloganized as focusing only on the importance of analogies for scientific explanation (Ruse 1975, 175; 1978, 2000). Analogies are clearly important in the development of a hypothesis for Herschel. After all, the use of the *vera causa* principle requires us to determine whether or not the causes in any given instance may be *analogous* to some other, already known, *vera causa,* and our successfully finding a cause in some particular circumstance depends, as just quoted, on the "analogous instances" we have at our disposal. Ruse, for example, stops the argument at this point, claiming that "the key to a *vera causa* was an *analogy*" (1978, 324), 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, quick though it was, that this is to substantially shortchange Herschel's philosophy of science. For the *vera causa* principle functions as a sort of check on wild speculation (and nothing more). As Wilson notes, "analogies were valuable when true causes were manifestly beyond reach" (1974, 94). Or, to quote a longer passage from Bolt,

> analogous instances played an important role for the production of hypotheses, but the fertility of an hypothesis – the ability of an hypothesis to predict and to apply to novel phenomena – as well as the consilience of an hypothesis – the ability of an hypothesis to embrace or to explain a wide-ranging set of phenomena – provided warrant not

> only for the hypothesis in question but for the very method of invoking hypotheses. (Bolt 1998, 405)

Analogy therefore features in this step just described, where we propose and evaluate the basic suitability of a hypothesis. It is not, by any stretch of the imagination, the lynchpin of Herschel's philosophy of science.

2.3. Verification: Adequacy

How, then, are we to test a hypothesis, having deemed it to satisfy this criterion? Our first step in verification is quite simple. Herschel 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 its occurrence, in order to satisfy ourselves that they are explicable by our cause.... (*PD* §172)

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 which we have proposed it to explain. 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 depiction of Herschel as constantly alternating between the "inductive" and "deductive" modes of theory construction. Despite Bacon's having tarred overreliance on induction as one of the "idols," a hypothesis was, or at least could have been, initially formulated by inductive means. Herschel believes he has justified this use of induction, by a combination of his *vera causa* principle and his rules for inductive method. The next step, then, is to turn to the deductive mode, and attempt to derive from this new hypothesis the desired consequences.

2.4. Verification: Consilience

Further, our deduction of consequences 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. ...[O]ur 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. (*PD* §176)[12]

Universality is evidently an essential requirement for novel causes in Herschel's philosophy.

More than simple universality, however, this sounds much like the concept of consilience, which deserves a fuller explanation 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* (1847 [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 [1840], 65)

Though Herschel does not apply Whewell's term (as he could not, Whewell's work not being published until a decade after his),[13] 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" (*PD* §138), and later, at this second step in the verification of a hypothesis. 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 proposal. 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 which 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

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[12] 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 which he annotated (Darwin 1990).

[13] 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 proposes that we re-christen them "Herschel's Habits" (1998, 398).

other and surprising data. If we have survived all these tests, we have produced a genuinely acceptable scientific explanation, on Herschel's lights.

If these are Herschel's methodological maxims, what are we to say about their application to Darwin's work? We must begin by considering the structure of Darwin's argument.

3. Darwin's Argument in the *Origin*

Darwin famously claimed that the *Origin* constituted "one long argument" (1859, 459), and there have been several attempts to clarify what we might consider to be the structure 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 – 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; Ruse 1975; Schweber 1983; Sober 1983). 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 chapters 1-3 of the *Origin*). He next attempts to derive from these laws both the claim that natural selection *must* occur (chapter 4), and further conclusions for other branches of the sciences (chapters 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, 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" (1975, 166). He therefore consciously structured his theory in accordance with the hypothetico-deductive ideal.

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 argues (1987, 151), 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 which ought to constitute the most fundamental axioms of a hypothetico-deductive presentation of

natural selection.[14] 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 "[w]hether 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" (Darwin 1859, 127).[15]

Secondly, 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 which would have had such a structure, and then abandons it. Hodge 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. (Hodge 2009, 54)

Such an undertaking, then, would clearly have had a hypothetico-deductive structure, modeled as it was on explicitly Newtonian precedents. 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.... Such considerations may have ended Darwin's aim of emulating the Newtonian consummation of Kepler's nomic legacy" (Hodge 2009, 68). We can thus demonstrate that Darwin dabbled in the 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 colleagues. Herschel, as noted in the last section, has occasionally been glossed as concerned above all with analogies. The traditional slogan for Whewell, on the other hand, is that he stands concerned first and foremost with the "consilience

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[14] Darwin's theory of pangenesis was probably worked out relatively early (Sloan 1983, 1986; Hodge 1983c), but he clearly saw no need to publish this theory in the *Origin* – it was not published until nine years later in *The Variation of Animals and Plants under Domestication.*

[15] 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 (1983).

of inductions" – the demonstration that many disparate lines of evidence may be unified under a single putative cause (Recker 1987; Ruse, 1978). We can find both an analogy (with artificial selection, in chapters 1-4) and a consilience (across many areas of science, in chapters 10-13) in the *Origin,* and here we have an explanation for the structure of its argument.[16] Ruse 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" (1978, 328).

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 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 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 "[n]o 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 (Whewell 1847 [1840], 283). Thus, Whewell's method would (at least on his own reading) disqualify Darwin's Herschellian analogy.

Next, this analysis seems to shortchange both Herschel and Whewell. To reduce Herschel to the single concept of "analogy," as was argued in the last section, is to choose only one (and one relatively unimportant, at that) of Herschel's criteria for the proposal and verification of a hypothesis. Though 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.[17]

Finally, and most importantly, we have a problem of anachronism here. The argumentative structure of the *Origin* laid down by Darwin in the E and N notebooks over the course of late 1838 would remain relatively constant throughout the *Essay,*

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[16] 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 (2008).

[17] Wilson (1974, 80-83) presents the case quickly and straightforwardly. See also Ducasse (1960, 180).

the *Sketch,* and the *Origin,* and Whewell coined his notion of consilience in his *Philosophy,* which was not published until 1840. Let us turn to a more fruitful analysis of the *Origin,* one which 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, M. J. S. 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; Lewens 2008; Waters 2009; Hull 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 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. (Hodge 1977, 239)[18]

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 one through three. We know that the selective modification of species by artificial breeding is incredibly effective – this is a cause which we know to exist. Further, we can locate three features in the natural world – hereditary variation, differential reproduction, and the Malthusian pressure on population – which are sufficient to instantiate a similar selective breeding process in the wild. Thus, natural selection *exists.*

Second, the primary portion of the *adequacy* case comes in chapter four 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 which natural selection has to work and the precision with which it can act, to produce new species which breed true. Then, chapters five through eight attempt to undercut obvious objections to this adequacy thesis.

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[18] Precisely the same division is clearly expressed in Hodge (1992).

Third and finally, chapters nine through thirteen of the *Origin* constitute what Hodge 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" (Hodge 1992, 463). Darwin's aim in these sections, 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 re-reading 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 Darwin's methodological or philosophical enthusiasm for Lyell (Hull 1983; Hodge 1987). All these positive cases, I agree, are quite valid.

However, Hodge misunderstands Darwin's motivations, especially in the "responsibility" phase of the argument – and this misunderstanding is due to his having significantly misread 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. His three phases of argumentation, insofar as they come out of Herschel, are supposed to derive from a straightforward explication of Herschel's use of *verae causae.* Hodge 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" (Hodge 1992, 239).[19]

These three phases are thus, on Hodge's reading of Herschel, *elements* of what it is to claim that something is a *vera causa.* That is, 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 2, I am quite doubtful that this is an accurate reading of Herschel's methodological prescriptions. First, as has already been

[19] 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 chapters 1-3, and then "natural selection as, on balance, probably responsible for species" in the rest of the book (Hodge 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.

discussed at length in section 2.2, 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, recall, writes of this drop in eccentricity that "[w]e 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" (*PD* §140).

Third and finally, *responsibility* in Hodge's sense is not a necessary requirement for the postulation of a hypothetical cause in Herschel's philosophy. It seems that the reference Hodge has in mind behind his responsibility requirement comes from the first definition of *vera causa* in §138, 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*" (emph. added).

Herschel's account of explanation, as we have seen it worked out in his examples, is less strict than the emphasized portion of this quote 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. But we need not have this sort of consilience when we *propose* a given causal agent. As we 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" (*PD* §209). The first of these two possibilities corresponds to Hodge's *responsibility* and Herschel's quote from §138. The second, I argue, is not only different, but is the sense in which Darwin intended to propose natural selection.

Hodge therefore seems to confuse the issue with regard to Herschel. As I argued above, however, his tripartite reading of the *Origin* seems correct. He thus arrives at the right conclusion via the wrong argument; let us now try to construct 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, correcting the places where Hodge misreads Herschel's philosophy of science.

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" (*PD* §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" (*PD* §141).

His 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 argues (2002), 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 argued at one point that "[i]f 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" (*PD* §142). Darwin seemed to earnestly desire to present this forceful 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 tells us, "its adequacy is another question" (*PD* §140). I agree with Hodge that this is the goal of the fourth through ninth chapters[20] – though, *contra* Hodge, 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 three and four. By the end of chapter three, 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, though he only elaborates on this claim very minimally, agrees, arguing that the first chapters

[20] Here lies a further disagreement with Hodge – 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.

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, 122-3).

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 which 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, 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 from the evolution of highly specialized organs, instinct, hybrids, and the geological record, among other things. 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.* 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. "[C]ases not originally contemplated" (*PD* §176), as Herschel 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.

We can see as well an echo of a piece of advice from Herschel which we have not yet had occasion to discuss. Herschel 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]" (*PD* §180). Ruse (1975, 2000) and Hull (2009) have noted that this explains several of Darwin's 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.[21] It should indeed be seen as weighty, on Herschel's criterion: embryology, as it was understood in Darwin's day (with focus on the progression inherent in development), was generally read as favoring a Lamarckian view of species development as opposed to a Darwinian one.

[21] Darwin Correspondence Project Database, Letter 2583, 14 Dec. 1859.

5. 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 mistaken reading 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 which it was not originally proposed to explain.

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, 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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