Herschel's Methodology in the Scientific Community

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The *Preliminary Discourse on the Study of Natural Philosophy,* which Herschel published in Dionysius Lardner's *Cabinet Cyclopaedia* series in 1830, can be a difficult book to interpret.[1] As has been emphasized by a wide variety of commentators, its content and the circumstances of its publication can lead us to think that it is perhaps better understood not (or at least, not merely) as a technical treatise on scientific inference and methodology, but rather in the tradition of "conduct manuals," a popular genre that offered readers insight into how they might elevate and refine their character.[2] The work invokes not just prescriptions for scientific practice, but also the epistemic and personal virtues of a good scientist, and even (perhaps especially) the merits of careful observation and the study of science for the layman. Science, Herschel writes, is exceptional in "filling us, as from an inward spring, with a sense of nobleness and power which enables us to rise superior to" the circumstances of our lives.[3] The book was printed and bound inexpensively, and widely sold and frequently reprinted.[4] Given its history, it thus seems likely that philosophers of science (and I include myself in this critique) have been too quick in reading this work only through the lens of its contributions to the epistemology of science.

That said, it remains the case that the second and largest part of the *Discourse* was dedicated to a detailed study of scientific methodology (see chapter 5), one of the first and most significant of such treatises to have appeared in decades in English, and one which was regularly cited—at least by other philosophers of the day, such as William Whewell and John Stuart Mill—as having reinvigorated the exploration of what would increasingly come to be called "the philosophy of science."[5] Discussions among these three and others would kick off a tradition that would lead, among other destinations, to Karl Pearson's early positivism and, across the Atlantic, to Charles Saunders Peirce's reflections on the scientific method. Peirce praised the three men for offering "some of the finest accounts of the methods of thought in science."[6]

It would be shocking, then, if it were the case that Herschel's work on methodology had made no impact whatsoever on practice within the scientific community, even if one acknowledges that the relationship between science and the philosophy of science was no less troubled in 1830 than it is today. This was all the more likely given the high esteem in which Herschel's *scientific* work was held by his colleagues. As other chapters in this volume attest (see chapters 3 and 7–9), Herschel's name was a byword for scientific authority in astronomy, physics, geology, and beyond. Susan Cannon did not exaggerate when she wrote that, through the middle of the nineteenth century, "one answer to the question of how to be scientific, then, might be, 'Be as much like Herschel as possible.'"[7]

This injunction, to be sure, was not merely one to follow Herschelian rules for inductive inference. As Richard Bellon has noted, in this period, the genres of popular manual of conduct and textbook of scientific methodology would have overlapped more than might now seem apparent to a contemporary observer. In Victorian Britain, he writes, "scientific discovery was a moral process, not an isolated event," and "scientists deployed a long list of words to imbue favored scientific research with moral authority."[8] Scientific methodology was certainly a matter of proposing and evaluating putative scientific explanations in the right way, following sound canons of experimentation, and so on, but it was also a question of cultivating the right kinds of epistemic virtues in practicing scientists. A list which Bellon draws only (!) from a collection of Herschel's published articles includes "*ardent*, *arduous*, *careful*, *diligent*, *disinterested*, *humble*, *impartial*, *indefatigable*, *industrious*, *laborious*, *methodical*, *painstaking*, *patient*, *perseverant*, *scrupulous*, and *zealous*."[9] It behooves us, therefore, to look not only at scientific practice, but also at scientific character.

To chart the impact that Herschel's vision of science might have had on the practice of nineteenth-century science itself, this chapter will briefly chronicle Herschel's relationship with three important figures from three different branches of natural science of the day: Charles Lyell, Charles Darwin, and Michael Faraday. In doing so, I hope to demonstrate that, for all that Herschel's *Discourse* is indeed a complex work, with a difficult (even at times confusing) philosophy of science, it was nonetheless taken seriously by figures who would go on to have massive significance in a variety of different disciplines, both in how these men did science, and the manner in which they believed it was important to behave as scientists.[10] After 1830, natural science would forever be—to at least some degree—Herschelian.

Among the Geologists: Lyell and the *Principles*

Any discussion of the "influence" of Herschel on the eminent geologist Charles Lyell must necessarily be nuanced. Lyell was five years Herschel's senior, and the *Discourse* included an example drawn directly from the first volume of Lyell's famous *Principles of Geology* (also published in 1830; the next two volumes would follow in 1832 and 1833). The direction of influence between the two men is thus difficult to establish: Herschel was preparing his book while in regular contact with Lyell, and, as we will see, Lyell and Herschel's thoughts on the role of *veræ causæ* are so close as to be nearly indistinguishable. In that sense, whether a particular claim is due to Herschel alone or includes instead intellectual contributions from both Herschel and Lyell, we nonetheless have a compelling instance of Herschelian philosophy of science put to use in one of the most important scientific works of the mid-nineteenth century.

Lyell's geology arises from a rich context of a controversy between two schools of geological thought: what would become known as the catastrophists and the uniformitarians. According to the catastrophists, the evidence of geology—especially evidence of massive upheavals and subsidence, broken and disarrayed geological strata, and so forth—demonstrates that the major features of the geological record have been shaped by massive, catastrophic geological events, entirely different in kind from those that we witness today (possibly, according to some, including the Noachian deluge). The uniformitarians, on the other hand—represented initially by the work of James Hutton, commonly read in the abridged version of Hutton's thought presented by John Playfair[11]—argued that the causes we can see working around us at present, like erosion, earthquakes, subsidence, and so on, would be enough to produce all of the geological changes we observe, if they were only given enough time to operate.[12] As Lyell himself summarizes the history of the dispute:

> We have seen that, during the progress of geology, there have been great fluctuations of opinion respecting the nature of the causes to which all former changes of the earth's surface are referrible [*sic*]. The first observers conceived that…there have been causes in action distinct in kind or degree from those now forming part of the economy of nature…. [Others, more recently,] infer that there has never been any interruption to the same uniform order of physical events. The same assemblage of general causes, they conceive, may have been sufficient to

> produce, by their various combinations, the endless diversity of effects, of which the shell of the earth has preserved the memorials….[13]

Lyell places great stock in what he calls the "undeviating uniformity of secondary causes," as a feature that develops in a theory whenever we sufficiently advance in scientific understanding, implicitly consigning catastrophes like the biblical flood to the same dustbin with "demons, ghosts, witches, and other immaterial and supernatural agents."[14]

Herschel lights on precisely this aspect of Lyell's theorizing when introducing his own understanding of a *vera causa*. A feature taken by a variety of commentators to be central to Herschel's methodology (and about which more in the next section, when we turn to Darwin), Herschel argues at length that successful scientific progress is about building a stock of proximate causes known to exist and to act in the world around us. If we confirm their action in the proper way (showing, for instance, that they could give rise not only to the phenomena for which we developed them in the first place, but others besides), then they can receive the stamp of scientific legitimacy. "To such causes," Herschel writes, "Newton has applied the term *veræ causæ*; that is, causes recognized as having a real existence in nature, and not being mere hypotheses or figments of the mind."[15]

As he turns to providing examples, after a toy case in which he rejects the possibility that "plastic virtue" of the soil could be responsible for the formation of fossils (compared with the *vera causa* of the death of a shelled animal and the deposition of that shell on the seabed), he raises a more complex case: the fact that the surface of the earth has cooled over geologic time. We do not, he claims, have a *vera causa* to which we can appeal in an inference to any explanation of this fact, for we lack the requisite experience with a planet cooling from a molten state, or with the circulation of heat from the center of the earth to its surface. But what we do have, thanks to Lyell, is a *vera-causa*–compatible explanation for the change in the distribution of land and sea over time. Lyell has demonstrated its *bona fides*, Herschel claims, with "the degradation of the old continents, and the elevation of new, being a demonstrated fact; and the influence of such a change on the climates of particular regions, if not of the whole globe, being a perfectly fair conclusion, from what we know of continental, insular, and oceanic climates by actual observation." Unlike in catastrophism, this means that "we have, at least, a cause on which a philosopher may consent to reason."[16] We don't yet have the evidence we need to say that Lyell has given

us the sole, correct explanation for continental change—that will take more evidence and evaluation, Herschel argues—but we do know that this is the *kind* of thing that could be legitimately admitted into scientific theorizing.

The affinity, then, between the approaches of the two men should be evident enough. Precisely the feature of Lyell's geology that he believed distinguished it from its predecessors—its reliance on highly confirmed, observed causes at work in the world around us—was taken by Herschel to be one of the defining characteristics of acceptable scientific theorizing. Again, whether this was a result of Herschel's influencing Lyell or Lyell's influencing Herschel is hard to say. The two of them corresponded regularly during the years immediately prior to the appearance of their two books, and informal opportunities for the sharing of ideas were of course manifest in the tightly knit community of Victorian British science.

A few years later, during his time at the Cape in 1836, Herschel would write to Lyell that his approach to geology was literally exemplary for the future development of science. "I hope your example will be followed in other sciences," he told Lyell, "of trying what *can* be done by existing causes, in place of giving way to the indolent weakness of a priori dogmatism—and as the basis of all further procedure enquiring what existing causes really are doing."[17] And, as we have already seen, Herschel's praise was not based upon idle speculation about the nature of geology and Lyell's contribution to it: he tells Lyell that he has read all three volumes of Lyell's *Principles* (more than 1,400 pages) no fewer than three times, and offers an array of suggestions, comments, and critiques, in domains as disparate as the geophysical, the geographical, and the botanical. Herschel's interest is not merely a matter of making obeisances to a well renowned fellow scientist.

Lyell would reply with a long letter of thanks, effusively saying that "I may truly say that when the Royal Society voted me a medal for my book, I was not more gratified nor more encouraged than by your full and interesting comments which have given me a feeling of strength and confidence in myself, which will assist me in my future studies."[18] The following year, after his return to Britain, he would write to Whewell, describing his theory now explicitly in the same terms that Herschel had used. He argues there that his critics, who had accused him of naively taking on an over-broad uniformity of nature as an assumption rather than arguing for it, were mistaken. Rather, "the reiteration of minor convulsions and changes, is, I contend, a *vera causa*, a force and mode of operation which we know to be true."[19] Of course, an invocation of the notion of *vera causa* is not necessarily Herschelian—the concept is coined by Newton and also famously

defended by Thomas Reid. But given the deep and abiding links between Herschel and Lyell, I don't think it's out of the question at all to see the success of Lyell's theory as a demonstration of the importance of Herschelian philosophy of science to geological practice in the 1830s. Admiration between the two was mutual, and based in no small part on a shared commitment to the same tenets of high-caliber scientific method.

Among the Biologists: Darwin and the *Origin*

In the letter that Herschel sent to Lyell, quoted above, Herschel offers a long-winded concurrence with yet another feature of Lyell's argument (defending it in fact even more strongly than Lyell himself had in the *Principles*): his naturalistic account of the creation of new species. While we do not know how the laws governing the production of species work, there must assuredly be some, Lyell had asserted, as the process of extinction is clearly at work in the world around us, and yet the number of species on the globe seems to have remained roughly constant over geologic time. We should value, Herschel writes, Lyell's "unveiling a dim glimpse of a region of speculation connected with it where it seems impossible to venture without experiencing some degree of that mysterious awe" described in the *Aeneid* or Walter Scott's *The Monastery*—"of course I allude," he clarifies, "to that mystery of mysteries the replacement of extinct species by others."[20]

Figure 1. The young Charles Darwin, in a watercolor portrait painted by George Richmond in the late 1830s. Public domain.

Lyell must have shared the letter with a young naturalist whom he knew was working on similar questions: Charles Darwin. Cannon notes that the mere existence of Herschel's speculation on naturalistic causes for the creation of species must have been liberating. The young "Darwin was able," she writes, "to be almost completely insensitive to theological considerations

concerning the origin of species, so much so that he did not even understand what the phrase 'the creation of species' meant" to authors like Whewell, never taking very seriously supernatural explanations for these phenomena.[21] Indeed, on the very first page of the *Origin of Species*, Darwin would write that the biogeography of South America which he observed on the *HMS Beagle* "seemed to me to throw some light on the origin of species —that mystery of mysteries, as it has been called by one of our greatest philosophers."[22] As Cannon has noted, "when an early Victorian writer says, for example, that 'one of the most profound philosophers and elegant writers of modern times' has stated such-and-such, the chances are good that the reference is to John Herschel."[23]

In the literature since, Herschel has proved to be a fruitful source for interpreting Darwin's project as it was laid down in the *Origin*—though not a source without its share of difficulties. The first is that a host of other nineteenth-century figures have been equally illuminating, including at the very least Whewell, Mill, and Comte. As in the case of Lyell, then, disentangling exactly what impact Herschel had on Darwin can be a bit of a challenge.[24] One aspect is certain enough: Darwin took from Herschel's *Discourse* exactly the kind of ascription of uplifting personal virtues arising from the practice of science that I discussed in introducing the chapter. As Darwin described his educational development in his *Autobiography*,

> 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 *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.[25]

But the question, clearly, goes beyond this: did Darwin in fact learn anything substantive from Herschel, as Cannon provocatively puts it, "more complicated than that it would be wonderful to be a scientist"?[26]

The answer to this question turns on the interpretation of the structure of the *Origin*'s central argument. We know that Darwin read the *Discourse* (for the second time) in late 1838, just as he's crystallizing the theory of natural selection and beginning to think of it as a piece of public, presentable science.[27] And if one regards the *Origin* through Herschelian lenses, a consistent reading emerges. Darwin begins with three chapters delineating

variation in domesticated plants and animals, as well as in the wild, then arguing for the presence of a struggle for existence which leads to the production of far more offspring than can ever possibly survive. We can interpret this as roughly akin to establishing natural selection as a *vera causa*. As we saw above in discussing Herschel's example drawn from Lyell, this is a very minimal criterion: we have to show that natural selection operates in ways similar to other causes whose action we've demonstrated in other contexts—in this case, things like domestic breeding and the tendency of "the lowest savages" to protect and reproduce their best animals over generations, thus "unconsciously" improving the quality of their stock over time.[28] Of course, these are phenomena *similar* to natural selection and not natural selection itself. But Herschel had made space for exactly this kind of move, and he had done so in exactly the way of which Darwin availed himself. "If the analogy of two phenomena be very close and striking," Herschel writes, "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."[29] Darwin's demonstration that these phenomena of variation and "selection" (whether in conscious breeding or unconscious herd-tending) are analogous to natural selection thus directly follows Herschel's playbook for the introduction of a *vera causa*.

But—as we also saw above—showing that something is a *vera causa* is only to make it "a cause on which a philosopher may consent to reason." We then have more work to do—what Herschel calls establishing the "adequacy" of a cause to produce the effects demanded of it. "Whenever, therefore, we think we have been led by induction to the knowledge of the proximate cause of a phenomenon," he argues, "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…."[30] Chapters four through nine of Darwin's *Origin* give us this kind of argument, describing how natural selection can produce different species, genera, and higher groups with wildly different characters, as well as various traits of organisms that readers were likely to see as refutations of natural selection—like highly or precisely adapted organs (such as eyes), instincts (and other mental or cognitive capacities), sterile hybrids, and so forth.

And lastly—anticipating the development of consilience, for which Whewell would become famous a decade later[31]—Herschel argues that we must not rest content with establishing adequacy, since adequacy involves primarily testing against phenomena that we had in mind while developing our theory. We must then turn to "*extending* its application to cases not

originally contemplated…studiously varying the circumstances under which our causes act, with a view to ascertain whether their effect is general… pushing the application of our laws to extreme cases."[32] This, then, is precisely what Darwin does in the last four chapters (before the summary conclusion), where he shows that adopting an evolutionary perspective can shed light on geology, biogeography, taxonomy, morphology, embryology, and the existence of rudimentary organs.

A motivated reader, then, can analyze the structure of Darwin's *Origin* and see precisely a theory designed to satisfy Herschel's methodological precepts. Herschel's *Discourse* lays out the steps that one ought to take in the course of developing, proposing, and evaluating a new cause to be added to the stock of those available in natural science. It seems that Darwin has offered us arguments corresponding to each of these steps, in what appears to be the order and arrangement that Herschel would have wanted. On this reading, Darwin's template or standard for what a piece of quality, publishable, public scientific theorizing should look like was drawn directly from Herschel's methodological maxims in the *Discourse*.

That said, there is widespread contention concerning this view. At the more fine-grained level, there are a number of ways to see how the various parts of Darwin's argument elaborate the Herschelian story—which chapters contribute to which facets of the defense of natural selection.[33] But also, we might ask ourselves to what extent we have overestimated Darwin's own philosophical sophistication. Herschel's approach to the proposition and validation of *veræ causæ* is extremely subtle, and has been subject to a variety of disagreements and misreadings in the philosophical community over the two centuries since Herschel set it down.[34] It's thus perhaps doubtful that Darwin had indeed taken away from the *Discourse* the detailed structure for the introduction of a causal theory that commentators have argued is evident in the *Origin*. The uses that Darwin made of the *vera causa* concept across his various letters and notebooks, to take just one example, do not make it entirely clear what it is that he took to be a *vera causa* or how he considered the many causes involved with natural selection to interact.[35] Speaking more generally, one of course need not have a sophisticated and consistent causal interpretation of natural selection to support evolutionary theory.[36]

But for our purposes here, an effort to use Herschel's philosophy of science—even if it were to be a heavy-handed and perhaps clumsy attempt, lacking the sophistication of a contemporary reading of the *Discourse*—is still an effort to implement Herschelian ideas in scientific practice. The available circumstantial evidence, such as Darwin's having reread the

Discourse just as he was attempting to structure his nascent thoughts about natural selection, offers us good reason to think that we have in any case an important example of Herschel's influence on nineteenth-century scientific practice.

Another major problem with a rosy interpretation of the Herschel-Darwin relationship which merits mention here is Herschel's own reaction to Darwin's *Origin*. Herschel famously rejected the theory of evolution, though he only discussed it very rarely in print. Darwin lamented in a letter to Lyell that "I have heard by round about channel that Herschel says my Book 'is the law of higgledy-piggledy.'—What this exactly means I do not know, but it is evidently very contemptuous.—If true this is great blow & discouragement."[37] This objection, however, need not be made on methodological grounds, and I don't believe that it was. Herschel, as it turns out, does not think that any theory of evolutionary change can be considered adequate to produce evolutionary phenomena, unless it can encompass a theory of the generation of the variation that is the raw material on which natural selection works. Since Darwin couldn't provide such a theory, his adequacy case for natural selection had simply failed for want of evidence.[38] But this isn't to say that Herschel believed that Darwin had somehow misapplied his canons of good methodology; we do not have any documentary evidence that, for instance, Herschel thought the *Origin* was somehow non-scientific or badly argued. Insufficiently empirically supported science, Herschel might have said, is still science.

To sum up, there is a consistent reading of the *Origin* (and some further evidence from the notebooks and Darwin's correspondence) on which Darwin had in mind, in his presentation of and argument for natural selection, something like the structure for proposing, evaluating, and verifying a causal claim in natural science contained in Herschel's *Discourse*. This evidence is, however, somewhat mixed, and it's not clear how sophisticated a reading of the *Discourse* would have actually been in play for Darwin.[39] But even with this equivocal evaluation of the case, it seems clear that Darwin took Herschel's philosophy of science very seriously, and Herschel's own appraisal of natural selection did not differ with it on methodological grounds; Darwin does seem to have been shaped in important ways by Herschelian philosophy of science.

Figure 2. Michael Faraday, painted in 1842 by Thomas Phillips. Public domain.

Among the Physicists: Michael Faraday and Experiments on Light

One issue in the relationship between Herschel and Darwin that makes it difficult to analyze is that Darwin didn't spend much time directly discussing his philosophical debts. On the contrary, this is not a problem we have when we turn to the work of the renowned physicist Michael Faraday. In 1832, Herschel had written to Faraday, among other things praising him for his recent experimental work.[40] Faraday writes back that he was particularly touched:

> I have the more pleasure in receiving your commendation than that of another person – not merely because there are few whose approbation I should compare with yours but for another circumstance. When your work on the study of Nat. Phil. [the *Discourse*] came out, I read it as all others did with delight. I took it as a school book for philosophers and I feel that it has made me a better reasoner & even experimenter and has altogether heightened my character and made me if I may be permitted to say so a better philosopher.
>
> In my last investigations I continually endeavored to think of that book and to reason & investigate according to the principles there laid down.[41]

Once again, this was not merely idle praise for an important and influential colleague. A few months later, on March 25, 1833, Faraday found himself the *de facto* representative of the Royal Institution at a dinner in honor of the centenary of the birth of Joseph Priestley. Demurring that "I have no reason why I should be distinguished with this mark of your favor…except that of the absence of my superior," he would go on to use the opportunity to reiterate, this time publicly, his praise for Herschel's *Discourse*:

> For my own part I must acknowledge that I cannot but attribute much of my late experimental success to an endeavour to follow the candid method of investigation pursued by Priestley, and to apply the principles of philosophical logic which I found in Sir John Herschel's "Preliminary Discourse."[42]

Faraday takes Herschel's work, then, to champion some of the same kinds of character traits—like "freedom of mind," "independence of dogma and of

preconceived notions," and "observation of facts which result from natural causes working before us"[43]—that he believed made Priestley's work so valuable. The admiration between the two was mutual; as Sydney Ross details, Herschel stood up for Faraday as an equal member of the scientific community and supported his (fiercely contested) membership in the Royal Society.[44]

Following the detailed reconstruction by David Gooding of an episode in their relationship, we can explore the connection between the two men in a bit greater detail.[45] In late 1845, Faraday announced that he had discovered what has since come to be called the Faraday effect: that the polarization plane of a beam of polarized light can be rotated under the influence of a magnetic field, proportional to the strength of the magnetic force. This comes close to an airtight demonstration that light is, in fact, an electromagnetic phenomenon—a claim that Faraday had long supported but not yet confirmed. Herschel wrote Faraday a letter of congratulations a few months later, after the public announcement of the discovery. His letter was tinged with a bit of scientific regret—he had himself attempted to find evidence for the same phenomenon. "It is now a great many years ago," he writes, "that I tried to bring this to the test of experiment (I think it was between 1822 and 1825)," when he had attempted to use "a great magnetic display by Mr Pepys at the London Institution" to show the same kind of effect of magnetism on polarization.[46] The experiment had failed. Herschel has no intention of questioning Faraday's priority—"for," he writes, "though I may regret that I did not prosecute a train of enquiry which seemed so promising up to a decisive fact I consider it honour enough to have entertained a conception which your researches have converted into a reality"[47]—precisely because of the crucial role that he gives to experiment in his newly developed philosophy of science.

Gooding argues that this shows us an interesting divergence between Herschel's philosophy and his own actual scientific practice. While Faraday, he writes, "never underestimated the difficulty of extracting the 'natural fact' from the phenomenal artefacts produced by his instruments," Herschel's approach to the question of magnetism and polarization

> reveals a discrepancy between his experimental practice and his methodology. According to the latter, experiment was primary. Thus, discoveries are awarded to the experimentalists who demonstrate them. Yet experiment was not actually as important to Herschel [in his scientific practice] as this view implies.[48]

If Herschel had successfully carried through his own precepts as laid down in the *Discourse*, he would have worked harder at repeating the hastily conducted experiment that he had performed using Pepys's battery (varying at least the two major possible explanations for failure, the battery's low charge and the medium in which the light was transmitted).

To see how Gooding's explanation might be supported, let's look at the way Herschel talks about the very idea of experiment in the *Discourse*. Collective and accumulated experience, he writes, is "the great, and indeed, only ultimate source of our knowledge of nature and its laws."[49] But experience can be generated in two different ways: observation, which simply consists of "noticing facts as they occur," and experiment, which results from "putting in action causes and agents over which we have control, and purposely varying their combinations, and noticing what effects take place."[50] Herschel writes that he prefers to call them *passive observation* and *active observation*, to underline the idea that both, while they refer to different approaches and different states of mind, result in the end in the collection of facts from the world around us. But the inductive credentials of experiment across the history of science are impressive, and are what distinguish it from passive observation. He draws out the case in a long analogy with testimonial evidence. We can either listen to the story that a witness tells us (often regretting later that we failed to pay attention to some important detail), or, by contrast,

> we cross-examine our witness, and by comparing one part of his evidence with the other, while he is yet before us, and reasoning upon it in his presence, are enabled to put pointed and searching questions, the answer to which may at once enable us to make up our minds.[51]

This grounds a substantial difference in power between experimental and observational sciences:

> Accordingly it has been found invariably, that in those departments of physics where the phenomena are beyond our control, or into which experimental enquiry, from other causes, has not been carried, the progress of knowledge has been slow, uncertain, and irregular; while in such as admit of experiment, and in which mankind have agreed to its adoption, it has been rapid, sure, and steady.[52]

These are strong words, especially coming from a scientist who has made his name in the family business of astronomy—exactly, one might think, the kind of department of physics where the phenomena are beyond our control. But it is the incorporation of astronomy as a branch of mechanics, and the ability to test its claims in the context of contemporary observational astronomy, that has enabled its recent and impressive advancement, Herschel claims.

Why does Herschel believe that experiment has this privileged role in scientific practice? As he argues later, perhaps the most important reason for its superiority is the fact that "in nature, it is comparatively rare to find instances pointedly differing in one circumstance agreeing in every other; but when we call experiment to our aid, it is easy to produce them…."[53] Experimentation thus gives us the ability to systematically vary the conditions that lead to a given phenomenon, in the effort to confirm that a proposed cause is indeed the one responsible for it. And, as Gooding reconstructs the methodology found in Faraday's notebooks, this is exactly the way in which Faraday conceives of the nature and role of his experimental work. In investigating some phenomenon,

> it is impossible to predict the whole set of necessary conditions. These have to be learned by systematically varying the parameters in order to discover the relevant parameters. […] Most of the work recorded in Faraday's laboratory *Diary* and (to a lesser extent) in his published *Researches*, is about this sort of problem-solving.[54]

In that sense, then, Faraday has out-Herscheled Herschel: Herschel didn't have the tenacity (or, one might demur, the time and access to high-quality equipment) to experiment further following his own guidelines for testing the effect of magnetism on light. But he did immediately recognize that the existence of that very tenacity—the fact that Faraday had adhered so precisely to the experimental method laid down in the *Discourse*—offered a clear confirmation of Faraday's legitimate priority (and virtue) in the discovery of the effect.

Faraday thus serves as perhaps the clearest example of the kind of influence that Herschel's philosophy of science had in the scientific community. Like Lyell, there was a deep and abiding mutual admiration between the two men, focused in no small part on precisely these questions of methodology, and, like Darwin, there was an explicit reliance on

Herschel's *Discourse*. For Faraday, though, the admiration is even more clearly expressed, and the reliance on the *Discourse* can be traced not only through oblique references and circumstantial evidence, but through Faraday's experimental practice itself and his discussion with Herschel on the discovery of the Faraday effect.

Conclusion

As Susan Cannon has argued, John Herschel set the bar for what it meant to be doing science in the mid-nineteenth century. In one sense, this was due to the sterling example of his scientific work. Herschel's astronomy served as the model, at the very least, for reasoning within the physical sciences, and likely, at least implicitly, for sciences far beyond physics.

> …as the chemist Charles Daubeny told the British Association in 1836, all of the physical sciences had as "the summit of their ambition, and the ultimate aim of the efforts of their votaries…to obtain their recognition as the worthy sisters of the noblest of these sciences—Physical Astronomy." In the England of the 1830's, "to be scientific" meant "to be like physical astronomy." To be quite specific, it meant "to be like John Herschel's extension of physical astronomy to the sidereal regions by his observations and then calculations of double-star orbits."[55]

But this exemplary role was also a result of his methodological and philosophical claims. As we have seen, his presentation of the precepts for introducing and proposing *veræ causæ* were influential, at the very least, on Lyell and Darwin; privately, his willingness to entertain a naturalistic explanation for the origin of species was important for Darwin as well; and his approach to experiment and observation, especially surrounding the persistent, systematic variation of the conditions under which a putative cause takes place, was a guiding principle for the experimental work of Faraday (by his own recounting, at least).

To close, I want to return to a point that I noted in the introduction. In addition to these direct methodological norms, Herschel—like a host of other nineteenth-century philosophers of science—advocated for a collection of epistemic virtues that could define what it meant to be a good *scientist*, not just to engage in good scientific practice. Of course, detecting the presence of these virtues in the works (or, perhaps better, in the lives) of nineteenth-century researchers is a challenge of a different order. But we can

get some glimpses of what these qualities might look like for each of the three figures that I've canvassed here.

To see the most explicit epistemic-virtue defense of the work of Lyell, we must briefly leave Herschel's writings and turn to Whewell's review of the first volume of Lyell's *Principles*—though the description we find there is entirely consonant with what we would find in Herschel's work. Because, Whewell writes, "a mass of knowledge has now been collected, most remarkable both in its quantity and its kind," we are capable finally of, "with a sagacity, perseverance, and success," profiting from "a fresh outbreak of the spirit of theorizing among our geologists."[56] Whewell writes that "the book has in truth a higher character; for it is so constructed, that the reader may avail himself of Mr. Lyell's aid, his rich and pregnant observation, his sound and well-pondered comparison…"[57] In short, Lyell's empirical grounding (in the body of carefully collected geological evidence that was now available) and his epistemic virtue ensure that even a speculative geological work will be worth our effort.

Turing to Darwin, Bellon notes that part of his triumph in convincing others of his new theory of evolution by natural selection is his having demonstrated precisely that he possessed such virtues—in addition to what might have been perceived as the rash theorizing present in a work like the *Origin*, he also had already published, and would go on to publish, a host of other, more methodical works on barnacles, orchids, earthworms, plant fertilization, insectivorous plants, and so forth.[58] Multiple commentators, including the botanist George Bentham and the chemist Charles Daubeny, stated publicly that this demonstration of virtue did much for their opinion not only of Darwin, but also of his theorizing more generally.

As we saw above, when Faraday linked Herschel's work to the types of desirable features that he had seen in the paragon Priestley, he did so largely in epistemic-virtue terms: Priestley was unimpeded by preconceived notions and dogmas, which gave him the right kind of "freedom of mind" for scientific work. Faraday presumably had these sorts of criteria in mind when he wrote that having attempted to follow both Priestley's example *and* Herschel's *Discourse* were crucial to the quality of the experimental results that he had been able to produce.

Both Herschel's standards for scientific methodology and his closely related model for scientific character and epistemic virtue are, therefore, instantiated by some of the leading figures of the nineteenth-century scientific community, in disciplines as diverse as geology, natural history, and (non-astronomical) physical science. Whether the *Discourse* is read more

narrowly as a work describing the epistemology of science and inductive inference; more moderately as a book about the kinds of epistemic virtues that practicing scientists needed to exemplify; or more broadly as a manual for good conduct both within and beyond the scientific community, it is clear that the history of science was indelibly marked by Herschel's influence.

1 John F. W. Herschel, A Preliminary Discourse on the Study of Natural Philosophy, 1st ed. (London: Longman, Rees, Orme, Brown, & Green, 1830).
2 James A. Secord, "The Conduct of Everyday Life: John Herschel's Preliminary Discourse on the Study of Natural Philosophy," in Visions of Science: Books and Readers at the Dawn of the Victorian Age (Chicago: University of Chicago Press, 2014), 80–106.
3 Herschel, A Preliminary Discourse on the Study of Natural Philosophy, 16, §12.
4 Secord, "The Conduct of Everyday Life: John Herschel's Preliminary Discourse on the Study of Natural Philosophy," 81, 87.
5 William Whewell, "[Review of] A Preliminary Discourse on the Study of Natural Philosophy. By J. F. W. Herschel, Esq., M.A. of St. John's College, Cambridge.," The Quarterly Review 45, no. 90 (1831): 374–407; John Stuart Mill, "Herschel's Preliminary Discourse," The Examiner, March 20, 1831; John F. W. Herschel, "Address of the President," in Report of the Fifteenth Meeting of the British Association for the Advancement of Science (London: John Murray, 1846), xl.
6 Karl Pearson, The Grammar of Science, 1st ed. (London: Walter Scott, 1892); Charles Sanders Peirce, Collected Papers of Charles Sanders Peirce, Vol. 1: Principles of Philosophy, ed. Charles Hartshorne and Paul Weiss (Cambridge, MA: Harvard University Press, 1931), CP 1.29.
7 W. F. Cannon, "John Herschel and the Idea of Science," Journal of the History of Ideas 22, no. 2 (1961): 219.
8 Richard Bellon, "Sacrifice in Service to Truth: The Epistemic Virtues of Victorian British Science," in Science, Technology, and Virtues, ed. Emanuele Ratti and Thomas A. Stapleford (New York: Oxford University Press, 2021), 18, https://doi.org/10.1093/oso/9780190081713.003.0002.
9 Bellon, 18.
10 On the difficulty of interpreting Herschel, see Marvin Paul Bolt, "John Herschel's Natural Philosophy: On the Knowing of Nature and the Nature of Knowing in Early-Nineteenth-Century Britain" (Ph.D. Thesis, University of Notre Dame, 1998).
11 John Playfair, Illustrations of the Huttonian Theory of the Earth (Edinburgh: Cadell and Davies, 1802).
12 Martin J. S. Rudwick, "Lyell and the Principles of Geology," Geological Society, London, Special Publications 143, no. 1 (January 1998): 1–15, https://doi.org/10.1144/GSL.SP.1998.143.01.02.
13 Charles Lyell, Principles of Geology, Vol. I (London: John Murray, 1830), 75.
14 Lyell, 76.
15 Herschel, A Preliminary Discourse on the Study of Natural Philosophy, 144, §138.
16 Herschel, 147, §149.
17 W. F. Cannon, "The Impact of Uniformitarianism: Two Letters from John Herschel to Charles Lyell, 1836-1837," Proceedings of the American Philosophical Society 105, no. 3 (1961): 307–8.
18 Cannon, 311.
19 Katharine Murray Lyell, Life, Letters and Journals of Sir Charles Lyell, Bart. (London: John Murray, 1881), 2:3.
20 Cannon, "The Impact of Uniformitarianism: Two Letters from John Herschel to Charles Lyell, 1836-1837," 305.
21 Cannon, 302.
22 Charles Darwin, On the Origin of Species, 1st ed. (London: John Murray, 1859), 1.
23 She also gives the example of Mary Somerville, in addition to Darwin; Cannon, "John Herschel and the Idea of Science," 218.
24 In this section, I follow portions of my previous analysis, though my opinion has shifted somewhat in the intervening years; Charles H. Pence, "Sir John F. W. Herschel and Charles Darwin: Nineteenth-Century Science and Its Methodology," HOPOS 8, no. 1 (2018): 108–40, https://doi.org/10.1086/695719.
25 Charles Darwin, The Autobiography of Charles Darwin, 1809-1882, with Original Omissions Restored, ed. Nora Barlow (London: Collins, 1958), 67–68.
26 W. F. Cannon, "Charles Lyell, Radical Actualism, and Theory," British Journal for the History of Science 9, no. 2 (1976): 118.
27 Charles Darwin, 'Books to Be Read' and 'Books Read' Notebook (1838–1851). CUL-DAR119, ed. Kees Rookmaker (URL: http://darwin-online.org.uk/: Darwin Online, 1838), fol. 4v.
28 Darwin, On the Origin of Species, 34.
29 Herschel, A Preliminary Discourse on the Study of Natural Philosophy, 149, §142.
30 Herschel, 165, §172.

31 William Whewell, The Philosophy of the Inductive Sciences, Founded upon Their History, Volume I, 2nd ed. (London: John W. Parker, 1847).
32 Herschel, A Preliminary Discourse on the Study of Natural Philosophy, 167, §176.
33 Compare, for instance, my account in: Pence, "Sir John F. W. Herschel and Charles Darwin: Nineteenth-Century Science and Its Methodology"; with that of M. J. S. Hodge, "Darwin's Argument in the Origin," Philosophy of Science 59, no. 3 (1992): 461–64, https://doi.org/10.1086/289682.
34 Richard Yeo, "Reviewing Herschel's Discourse," Studies in History and Philosophy of Science 20, no. 4 (December 1, 1989): 541–52, https://doi.org/10.1016/0039-3681(89)90023-X; Bolt, "John Herschel's Natural Philosophy: On the Knowing of Nature and the Nature of Knowing in Early-Nineteenth-Century Britain."
35 Ben Bradley, "Natural Selection According to Darwin: Cause or Effect?," History and Philosophy of the Life Sciences 44, no. 2 (June 2022): 13, https://doi.org/10.1007/s40656-022-00485-z.
36 A fact made all the more salient by the fact that, no such interpretation currently receives philosophical consensus; see Charles H. Pence, The Causal Structure of Natural Selection (Cambridge: Cambridge University Press, 2021).
37 Charles Darwin to Charles Lyell, "Letter 2575 – Darwin, C. R. to Lyell, Charles, [10 Dec. 1859]," December 10, 1859, https://www.darwinproject.ac.uk/letter/entry-2575.
38 Pence, "Sir John F. W. Herschel and Charles Darwin: Nineteenth-Century Science and Its Methodology," 130–35.
39 However much I may have argued in support of the contrary point in the past.
40 Michael Faraday, Experimental Researches in Electricity (London: J. M. Dent and Sons, 1914), secs. 1–2.
41 Michael Faraday to John F. W. Herschel, "Letter Faraday0623, from Michael Faraday to John Frederick William Herschel," November 10, 1832, https://epsilon.ac.uk/view/faraday/letters/Faraday0623.
42 William Babington et al., "Commemoration of the Centenary of the Birth of Dr. Priestley," Philosophical Magazine 2, no. 11 (May 1833): 390–91, https://doi.org/10.1080/14786443308648067.
43 Babington et al., 390.
44 Sydney Ross, "John Herschel on Faraday and on Science," Notes and Records of the Royal Society of London 33, no. 1 (1978): 77–82.
45 David Gooding, "'He Who Proves, Discovers': John Herschel, William Pepys and the Faraday Effect," Notes and Records of the Royal Society of London 39, no. 2 (April 30, 1985): 229–44, https://doi.org/10.1098/rsnr.1985.0011.
46 John F. W. Herschel to Michael Faraday, "Letter Faraday1783, from John Frederick William Herschel to Michael Faraday," November 9, 1845, https://epsilon.ac.uk/view/faraday/letters/Faraday1783.
47 Herschel to Faraday.
48 Gooding, "'He Who Proves, Discovers,'" 231.
49 Herschel, A Preliminary Discourse on the Study of Natural Philosophy, 76, §67.
50 Herschel, 76, §67.
51 Herschel, 77, §67.
52 Herschel, 77, §67.
53 Herschel, 155, §156.
54 Gooding, "'He Who Proves, Discovers,'" 234.
55 Cannon, "John Herschel and the Idea of Science," 238.
56 William Whewell, "[Review of] Principles of Geology; Being an Attempt to Explain the Former Changes of the Earth's Surface by Reference to Causes Now in Operation. By Charles Lyell, Esq. F. R. S. For. Sec. to the Geol. Soc., &c. In 2 Vols. Vol. I," The British Critic 9 (1831): 180, 184.
57 Whewell, 186.
58 Bellon, "Sacrifice in Service to Truth," 30–31; Richard Bellon, "Charles Darwin Solves the 'Riddle of the Flower'; or, Why Don't Historians of Biology Know about the Birds and the Bees?," History of Science 47, no. 4 (December 2009): 373–406, https://doi.org/10.1177/007327530904700402.