CHARLES H. PENCE

11

John Herschel's Methodology in the Scientific Community

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 commentators have emphasized, its content and the circumstances of its publication indicate 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 *Preliminary Discourse* 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.[3] 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.[4] The *Preliminary Discourse* was printed and bound inexpensively, widely sold, and frequently reprinted.[5] Given its history, it seems likely that philosophers of science have been too quick in reading this work primarily 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, one of the first and most significant of such treatises to have appeared in decades in English and one that was cited – at least by other philosophers of the day, such as William Whewell (1794–1866) and John Stuart Mill (1806–73) – as having reinvigorated the exploration of what would come to be called "the philosophy of science."[6] Discussions

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among these three and others initiated a tradition that would lead, among other destinations, to the early positivism of Karl Pearson (1857–1936) and, across the Atlantic, to reflections on the scientific method by Charles Sanders Peirce (1839–1914). Peirce praised Herschel, Whewell, and Mill for together offering "some of the finest accounts of the methods of thought in science."[7]

Given the high esteem in which Herschel's colleagues held his *scientific* work, it certainly seems reasonable to assume his treatise on methodology would have had an impact on his peers. As other chapters in this volume attest, Herschel's name was a byword for scientific authority in astronomy, physics, geology, and beyond. Susan Cannon (1925–81) did not exaggerate when she wrote that through the middle of the nineteenth century "one answer to the question of how to be scientific ... might be, 'Be as much like Herschel as possible.'"[8] But since the philosophy of science was a novel endeavor, and scientists always have a contested relationship with philosophy, we should expect this impact to be diffuse and difficult to pin down.

This injunction to be "like Herschel" went beyond philosophy as well, being more than merely instruction 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 overlapped more than might now seem apparent. In Victorian Britain, Bellon 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."[9] Scientific methodology was certainly a matter of proposing and evaluating putative scientific explanations in the correct way, following sound canons of experimentation, and so on, but it was also a question of cultivating the right kinds of epistemic virtues as a practicing scientist. A list of such virtues that Bellon draws 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*."[10] In understanding Herschel's influence, then, it is important to look not only at scientific practice but also at scientific character.

The task of investigating Herschel's influence on scientists of his day thus begins to take on a different form. Herschel, with many others, was

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fashioning a new discipline, one whose relationship to science was still a matter for debate and the extent of which was taken to go far beyond what we would today call the philosophy of science (even shading off into practical advice). What we see in Herschel's relationship to other scientists is not a story of direct influence or the *Preliminary Discourse* being used as a manual for scientific practice (though several examples will come close). More accurately, Herschel's work is a central contributor to a shared context of mid-nineteenth-century scientific methodology, one that was elaborated across scientific disciplines by a host of figures. This context was shaped by the *Discourse*, to be sure, as well as in other ways, including correspondence and networks of personal connection and influence. Book reviews were also particularly important in this period, and this was no different in the case of Herschel's work. What I will call "Herschelian" philosophy of science, then, is both an element and a product of this broader context, both created by the *Discourse* and altered by the interaction between those ideas and Herschel's colleagues.

To explore the way this philosophy was put into practice, 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 each, we see a different way in which Herschel's work informed their intellectual perspective and vice versa. We will also see that the *Preliminary Discourse* was taken seriously by figures who went on to exert massive influence in a variety of different disciplines, both in how these natural philosophers conducted their science and the manner in which they believed it was important to behave as scientists. After 1830, natural science would forever be – to at least some degree – Herschelian.

Lyell and the *Principles*

One reason a straightforward exploration of Herschel's "influence" is too simplistic to capture the landscape surrounding discussions of methodology in this period is encapsulated by his relationship with the eminent geologist Charles Lyell (1797–1875). Herschel was five years Lyell's senior, and the *Discourse* included an example drawn directly from the first volume of Lyell's famous *Principles of Geology*

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(also published in 1830; the next two volumes would follow in 1832 and 1833). 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. Herschel and Lyell thus form a perfect example of the kind of contribution that Herschelian philosophy of science made: Herschel's claims about scientific methodology both reflected and shaped one of the most important scientific works of the mid-nineteenth century.

Lyell's geology arose from a rich context of controversy between two schools of geological thought, which Herschel's close friend William Whewell would baptize as the "catastrophists" and "uniformitarians." According to the catastrophists, the evidence of geology – especially 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 (possibly, for some, including the Noachian deluge) entirely different in kind from those that we witness today. The uniformitarians, on the other hand – represented initially by the work of James Hutton (1726–97), commonly read in the abridged version of Hutton's thought presented by John Playfair (1748–1819) – argued that the causes we see working around us at present, like erosion, earthquakes, subsidence, and so on, would be enough to produce all the geological changes we observe, if they were only given enough time to operate.[11] As Lyell summarized 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.[12]

Lyell placed great stock in what he called the "undeviating uniformity of secondary causes" as a feature that develops in any sufficiently advanced scientific theory, implicitly consigning catastrophes like the

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biblical flood to the same dustbin with "demons, ghosts, witches, and other immaterial and supernatural agents."[13]

Herschel lights on precisely this aspect of Lyell's theorizing in the *Preliminary Discourse* when introducing his own understanding of a *vera causa*, a feature often taken to be central to Herschel's methodology (and about which more in the next section, when we turn to Darwin). Herschel argued 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 significantly different other observed phenomena as well), then they 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."[14]

As he turned 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), Herschel 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 constructing an explanation of this fact, for we lack the requisite experience of a planet cooling from a molten state or 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 had demonstrated this explanation's 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." In contrast to catastrophism, this means that "we have, at least, a cause on which a philosopher may consent to reason."[15] We do not 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,

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Herschel argues – but we do know that this is the *kind* of thing that could be legitimately admitted into scientific theorizing, because its action can be confirmed by direct inspection.

The affinity, then, between the approaches of the two men should be obvious. 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. The two corresponded regularly during the years immediately prior to the appearance of their books, and informal opportunities for the sharing of ideas were of course manifest in the tightly knit community of Victorian British science. Lyell's work would often be consulted for its (we might say Herschelian) methodological tenets, and Herschel was respected for his geological field work, the results of which he shared with Lyell.[16]

A few years later, during his time at the Cape in 1836, Herschel wrote to Lyell that the latter's approach to geology was 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] As we have seen, Herschel's praise was not based on idle speculation about the nature of geology and Lyell's contribution to it. He told Lyell that he had read all three volumes of Lyell's *Principles* (more than 1,400 pages) no fewer than three times and offered an array of suggestions, comments, and critiques in domains as disparate as the geophysical, the geographical, and the botanical. Herschel's interest was not merely a matter of making obeisance to a renowned fellow scientist.

Lyell replied with a long letter of thanks, professing 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 wrote to Whewell, now explicitly describing his theory in the same terms that Herschel used.[19] He argued there that his critics, who 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."[20] Of course, an invocation of the notion of *vera causa* was not necessarily Herschelian; the concept was coined by Newton and also famously defended by Thomas Reid (1710–96).[21] But again, we can see Herschel and Lyell both working to reinforce the importance of Herschelian philosophy of science to geological practice in the 1830s. Admiration between them was mutual and based in no small part on a shared commitment to the same tenets of high-caliber scientific method.

Darwin and the *Origin*

In the letter Herschel sent to Lyell, quoted above, Herschel offered 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. Though the nature of the laws governing the production of species remained obscure, there must assuredly be such laws, 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 wrote, 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 Virgil's *Aeneid* or Walter Scott's *The Monastery*. "Of course I allude," he clarified, "to that mystery of mysteries the replacement of extinct species by others."[22]

Lyell must have shared the letter with a young naturalist whom he knew was working on similar questions: Charles Darwin (1809–82) (Figure 11.1). Cannon noted that the mere existence of Herschel's speculation on naturalistic causes for the creation of species must have been liberating. The young Darwin, she writes, "was able to be almost completely insensitive to theological considerations concerning the origin of species."[23] Indeed, on the very first page of the *Origin of Species*, Darwin wrote that the biogeography of South America he

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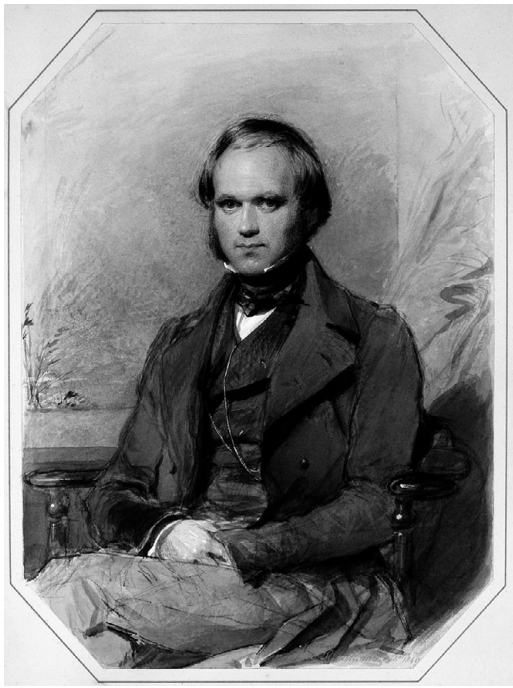

Figure 11.1 The young Charles Darwin, in a watercolor portrait painted by George Richmond in the late 1830s (Public domain)

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."[24] 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."[25]

Herschel has proven 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 of these is that a host of other nineteenth-century figures have been equally illuminating, including Whewell, Mill, and Auguste Comte (1798–1857). Finding Darwin's position within the landscape of methodological insight in this period is thus challenging.[26] One aspect is certain enough, though: Darwin took from Herschel's *Discourse* exactly the kind of ascription of personal virtues arising from the practice of science that Bellon has

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highlighted. 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.[27]

But did Darwin in fact learn anything substantive from Herschel "more complicated than," as Cannon provocatively puts it, "that it would be wonderful to be a scientist"?[28]

The answer to this 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 was crystallizing the theory of natural selection and beginning to think of it as a piece of public, presentable science.[29] And if one regards the *Origin* through Herschelian lenses, a consistent reading emerges. Darwin begins his work 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 that 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 in 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 the action of which we have 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.[30] Of course, these are phenomena *similar* to natural selection and not natural selection itself. But Herschel had made space for exactly this move, and he had done so in exactly the way that Darwin would. "If the analogy of two phenomena be very close and striking," Herschel wrote, "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."[31] Darwin's demonstration that these phenomena of variation and "selection" (whether in

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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* only makes it "a cause on which a philosopher may consent to reason." We then have more work to do: what Herschel called 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 argued, "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."[32] Chapters 4 through 9 of Darwin's *Origin* give 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 well-known a decade later – Herschel argued 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 [and] pushing the application of our laws to extreme cases."[33] This is precisely what Darwin does in the last four chapters of the *Origin* (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 a theory designed precisely to satisfy Herschel's methodological precepts. Herschel's *Discourse* laid 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. Darwin offered arguments corresponding to each of these steps in the

order and arrangement 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 contention concerning this view. At a fine-grained level, nearly any way of making it precise can be contested, as there are a number of incompatible ways to see how the various parts of Darwin's argument constitute a Herschelian story – which chapters contribute to which facets of the defense of natural selection.[34] 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.[35] It is thus perhaps doubtful that Darwin indeed took 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 he took to be a *vera causa* or how he considered the many causes involved with natural selection to interact.[36] Speaking more generally, one of course need not have a sophisticated and consistent causal interpretation of natural selection to support evolutionary theory.[37] It is precisely these details that are difficult to discern in any particular case of "influence" and that make the indisputable proof of any such influence so hard to come by.

But for our purposes, an effort to use Herschel's philosophy of science – even if it were a heavy-handed and perhaps clumsy attempt, lacking the sophistication of a contemporary reading of the *Discourse* – still has Darwin drawing on the mid-nineteenth-century methodological context that was so strongly shaped by Herschel's work. 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 we have an example of Herschel's direct influence on nineteenth-century scientific practice.

Another major problem with a rosy interpretation of the Herschel-Darwin relationship that merits mention here is Herschel's reaction to

Darwin's *Origin*. Herschel famously rejected the theory of evolution, though it can be hard to understand why, as he discussed it only 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."[38] Herschel's objection, however, need not have been made on methodological grounds. Herschel did not think any theory of evolutionary change could be considered adequate to produce evolutionary phenomena unless it could encompass a theory of the generation of the variation that is the raw material on which natural selection works. Since Darwin could not provide such a theory, his adequacy case for natural selection failed for want of evidence.[39] But this does not imply that Herschel believed 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.

Darwin's critics, Herschel included, thus did not often frame their arguments against him in Herschelian-methodological terms. But his supporters did, at least occasionally, explicitly defend his work in this way. Writing in *The Geologist*, Frederick Hutton (1836–1905), who would go on to offer evolutionary accounts of the flora and fauna of New Zealand, argued it was self-evident that "natural selection is a 'vera causa,'" and, closing his article by citing the *Preliminary Discourse*, wrote that though "I know that it rests at present on presumptive evidence alone . . . in the words of Sir John Herschel, 'are we to be deterred from framing hypotheses and constructing theories, because we meet with such dilemmas, and find ourselves frequently beyond our depths? Undoubtedly not.'"[40]

To sum up, there is a consistent reading of the *Origin* (and further evidence from the notebooks, Darwin's correspondence, and his peers) in which Darwin had in mind, in his presentation of and argument for natural selection, the structure contained in Herschel's *Discourse* for proposing, evaluating, and verifying a causal claim in natural science. This evidence is, however, somewhat mixed.[41] But even with this equivocal evaluation of the case, it seems clear Darwin took

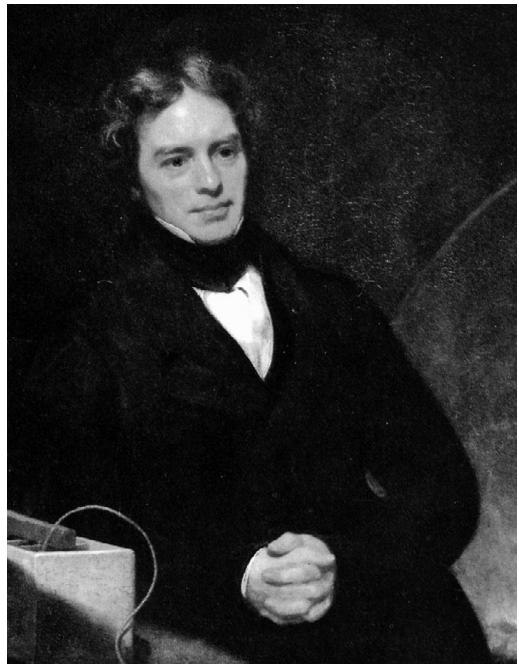

Figure 11.2 Michael Faraday, painted in 1842 by Thomas Phillips (Public domain)

Herschel's philosophy of science very seriously, and Herschel's own appraisal of natural selection did not differ with it on methodological grounds.

Michael Faraday and Experiments on Light

One aspect of the relationship between Herschel and Darwin that makes it difficult to analyze is that Darwin did not spend much time directly discussing his philosophical debts. On the contrary, this is not a problem when we turn to the work of the renowned physicist Michael Faraday (1791–1867) (Figure 11.2). In 1832, Herschel had written to Faraday praising him for his recent experimental work.[42] Faraday wrote 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

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> 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.[43]

Once again, this was not 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 (1733–1804). 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 went 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."[44]

Faraday took 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" – that he believed made Priestley's thought so valuable. The admiration 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.[45]

Following the reconstruction by David Gooding of another episode in their relationship, we can explore the connection between Herschel and Faraday in greater detail.[46] In late 1845, Faraday announced he had discovered what has since come to be referred to as 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 is nearly a direct demonstration that light is, in

fact, an electromagnetic phenomenon – a claim Faraday had long supported but not yet confirmed. Herschel wrote Faraday another congratulatory letter a few months later, after the public announcement of the discovery. This letter was tinged with a bit of scientific regret: Herschel had himself attempted to find evidence for the same phenomenon. "It is now a great many years ago," he wrote, "that I tried to bring this to the test of experiment," 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. The experiment had failed. Herschel had no intention of questioning Faraday's priority – "for," he wrote, "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" – precisely because of the crucial role that he gives to experiment in his newly developed philosophy of science.[47]

Gooding argues that this shows an interesting divergence between Herschel's philosophy and his own actual scientific practice. Whereas Faraday, Gooding 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 [his position expressed in the *Preliminary Discourse*] 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

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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 wrote that he preferred to call these types of observations *passive observation* and *active observation*, to underline the idea that both, though 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, the ability to refine our observational techniques in order to bring astronomy closer to the category of experimental science, and the ability to test its claims (especially in contemporary observational astronomy, see Chapters 3 and 6), that has enabled its recent and impressive advancement, Herschel claims.

Why does Herschel believe experiment has this privileged role in scientific practice? As he argued 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 conceived 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 most direct example of Herschel's role in mid-nineteenth-century developments in scientific methodology. As between Herschel and Lyell, there was a deep and abiding mutual admiration between Herschel and Faraday, focused in no small part on precisely these questions of methodology, and, for Faraday as for Darwin, there was an explicit reliance on Herschel's *Discourse*. Faraday's admiration is even more clearly expressed though, and the reliance on the *Discourse* can be traced not only through oblique references and circumstantial evidence but also through Faraday's experimental practice itself and his discussion with Herschel on the physical effect that now bears his name.

Conclusion

As Susan Cannon has argued, John Herschel set the bar for what it meant to do science in the mid-nineteenth century. In one sense, this was due to the sterling example of his own 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.

> In the England of the 1830s, "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 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.

To close, I want to expand our view of the intellectual context to which Herschel contributed by returning to a point noted in the introduction. In addition to his methodological norms, Herschel 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 surveyed 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 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 finally

capable of, "with a sagacity, perseverance, and success," profiting from "a fresh outbreak of the spirit of theorizing among our geologists."[56] Whewell wrote 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) and his epistemic virtue ensure that even a speculative geological work will be worth our effort.

Turning to Darwin, Bellon notes that part of his triumph in convincing others of his new theory of evolution by natural selection was 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 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 (1800–84) and the chemist Charles Daubeny (1795–1867), stated publicly that this demonstration of virtue did much for their opinion not only of Darwin but of his theorizing more generally.

When Faraday linked Herschel's work to the types of desirable features 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* was crucial to the quality of the experimental results he had been able to produce.

Both Herschel's standards for scientific methodology and his closely related model for scientific character and epistemic virtue were 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. These same figures, in turn, adopted, adapted, and advocated their own views, shaping the fertile environment of theorizing about science during this period. 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 the change in philosophical perspective that took place during this period – a change of which Herschel was one of the primary architects.

Notes

1 John F. W. Herschel, *A Preliminary Discourse on the Study of Natural Philosophy*, 1st ed. (London: Longman, Rees, Orme, Brown, & Green, 1830). See Chapter 1 for a discussion.
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 In order to avoid the complex landscape and shifting definitions of "science" and "natural philosophy" (as well as "scientist" and "natural philosopher") in this period, I have chosen to adopt "science" and "scientist" in roughly their contemporary sense here. The further intricacy that such a change in terminology would cause outweighs the increase in terminological accuracy. Herschel himself tended to use science, natural science, and natural philosophy as synonyms.
4 Herschel, *Preliminary Discourse*, 16, §12.
5 Secord, "The Conduct of Everyday Life," 81, 87.
6 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*, 45.90 (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.
7 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.
8 W. F. Cannon, "John Herschel and the Idea of Science," *Journal of the History of Ideas*, 22.2 (1961): 215–39, on 219.
9 Richard Bellon, "Sacrifice in Service to Truth: The Epistemic Virtues of Victorian British Science," in Emanuele Ratti and Thomas A. Stapleford (eds.), *Science, Technology, and Virtues* (New York: Oxford University Press, 2021), 17–36, on 18.

10 Bellon, "Sacrifice in Service to Truth," 18.
11 John Playfair, *Illustrations of the Huttonian Theory of the Earth* (Edinburgh: Cadell and Davies, 1802); Martin J. S. Rudwick, "Lyell and the Principles of Geology," *Geological Society, London, Special Publications*, 143.1 (January 1998): 1–15.
12 Charles Lyell, *Principles of Geology*, vol. I (London: John Murray, 1830), 75.
13 Lyell, *Principles of Geology*, 76.
14 Herschel, *Preliminary Discourse*, 144, §138.
15 Herschel, *Preliminary Discourse*, 147, §149.
16 Gregory A. Good, "John Herschel's Geology: The Cape of Good Hope in the 1830s," in Jed Buchwald and Larry Stewart (eds.), *The Romance of Science: Essays in Honour of Trevor H. Levere* (Cham: Springer, 2017), 135–50.
17 W. F. Cannon, "The Impact of Uniformitarianism: Two Letters from John Herschel to Charles Lyell, 1836–1837," *Proceedings of the American Philosophical Society*, 105.3 (1961): 307–8.
18 Cannon, "The Impact of Uniformitarianism," 311.
19 See also Whewell's review of Lyell's *Principles*, which demonstrates Whewell's familiarity with and approval of Lyell's work more generally (and about which more in the concluding section); 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).
20 Katharine Murray Lyell, *Life, Letters and Journals of Sir Charles Lyell, Bart.* (London: John Murray, 1881), 2:3.
21 Thomas Reid, *Essays on the Intellectual Powers of Man*, ed. A. D. Woozley (London: Macmillan, 1941), 34–35. See also Rachel Laudan, "The Role of Methodology in Lyell's Geology," *Studies in History and Philosophy of Science*, 13.3 (1982): 215–49.
22 Cannon, "The Impact of Uniformitarianism," 305.
23 Cannon, "The Impact of Uniformitarianism," 302.
24 Charles Darwin, *On the Origin of Species*, 1st ed. (London: John Murray, 1859), 1.
25 She also gives the example of Mary Somerville. Cannon, "John Herschel and the Idea of Science," 218.
26 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*, 8.1 (2018): 108–40.
27 Charles Darwin, *The Autobiography of Charles Darwin, 1809–1882, with Original Omissions Restored*, ed. Nora Barlow (London: Collins, 1958), 67–68.

28 W. F. Cannon, "Charles Lyell, Radical Actualism, and Theory," *British Journal for the History of Science*, 9.2 (1976): 104–20, on 118.
29 Charles Darwin, 'Books to Be Read' and 'Books Read' Notebook (1838–51). CUL-DAR119, ed. Kees Rookmaker (http://darwin-online.org.uk/), fol. 4v.
30 Darwin, *On the Origin of Species*, 34.
31 Herschel, *Preliminary Discourse*, 149, §142.
32 Herschel, *Preliminary Discourse*, 165, §172.
33 Herschel, *Preliminary Discourse*, 167, §176.
34 Compare, for instance, my account in Pence, "Sir John F. W. Herschel and Charles Darwin" with that of M. J. S. Hodge, "Darwin's Argument in the Origin," *Philosophy of Science*, 59.3 (1992): 461–64.
35 Richard Yeo, "Reviewing Herschel's Discourse," *Studies in History and Philosophy of Science*, 20.4 (1989): 541–52; Marvin Paul Bolt, "John Herschel's Natural Philosophy: On the Knowing of Nature and the Nature of Knowing in Early-Nineteenth-Century Britain" (PhD Thesis, University of Notre Dame, 1998).
36 Ben Bradley, "Natural Selection according to Darwin: Cause or Effect?," *History and Philosophy of the Life Sciences*, 44.2 (2022): 13.
37 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).
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39 Pence, "Sir John F. W. Herschel and Charles Darwin," 130–35.
40 Frederick Wollaston Hutton, "Some Remarks on Mr. Darwin's Theory," *The Geologist*, 4 (1861): 188. Hutton's quote was from Herschel, *Preliminary Discourse*, 196, §208.
41 However much I may have argued in support of the contrary point in the past.
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46 David Gooding, "'He Who Proves, Discovers': John Herschel, William Pepys and the Faraday Effect," *Notes and Records of the Royal Society of London*, 39.2 (April 30, 1985): 229–44.

47 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. Accessed February 6, 2023.
48 Gooding, 'He Who Proves, Discovers,' 231.
49 Herschel, *Preliminary Discourse*, 76, §67.
50 Herschel, *Preliminary Discourse*, 76, §67.
51 Herschel, *Preliminary Discourse*, 77, §67.
52 Herschel, *Preliminary Discourse*, 77, §67.
53 Herschel, *Preliminary Discourse*, 155, §156.
54 Gooding, 'He Who Proves, Discovers,' 234.
55 Cannon, "John Herschel and the Idea of Science," 238.
56 Whewell, "[Review of] Principles of Geology," 180, 184.
57 Whewell, "[Review of] Principles of Geology," 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*, 47.4 (December 2009): 373–406.

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