Chapter 4
Controlling Induction: Practices and Reflections in David Brewster's Optical Studies

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4.1 Introduction

The term "induction" has many meanings, although modern philosophical discussions often understand it as enumerative induction. The early nineteenth century, in contrast, had a wider understanding. When speaking of "inductive science," philosopher-scientists such as John Herschel or William Whewell or philosophers such as John Stuart Mill had in mind sciences based on empirical input—as opposed to, for example, mathematics, logic, or metaphysics. Although they had different ideas on how the inductive procedure should work, they shared that general understanding. This is what I mean by induction in this article's title.

My interest has long been to understand different types of learning from experiments or observation, which includes induction in the broad sense. In this chapter, I am interested in how this process of learning has been both conceived and practiced as more or less rigorous and strictly controlled in its various steps. Rigor and control might appear on many levels, such as conceiving and performing the experiment or drawing conclusions from its outcomes. They secure or enhance the reliability of the inductive process and its results.

Here, I shall begin with a specific example of eighteenth-century optical research, and from there shall develop wider considerations. The historical case will serve as illustration for three theses. First, and not surprisingly, experimental control in the physical sciences has different dimensions. These are connected to different experimental traditions. Second, the way experimental control was practiced and reflected in historical cases stems from certain specific epistemic goals. Third and last, in nineteenth-century experimental optics, at least two different traditions of experimental control and rigor intertwined, which gave rise to the most remarkable optical achievements.

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4.2 Optical Research in the Early Nineteenth Century

In the early nineteenth century, optical theory went through a turbulent if not dramatic phase—the debate over the corpuscular and wave theories of light, with the latter's final 'victory.' This process has sometimes been called a scientific revolution.[1] In its first phase, the debate took place mainly in Paris, with prominent researchers such as Jean-Baptiste Biot, François Arago, Augustin Fresnel, Étienne Louis Malus, and Siméon Denis Poisson involved. It involved fierce debates and complicated frontlines that were shaped by personal and institutional relationships.

On the empirical side, one key event was E. L. Malus' analysis of double refraction and his discovery of light's polarization by reflection in 1809. Other arguments cited carefully conducted experiments, often with new findings. As a rule, however, these experiments could support specific positions, but did not rule out others. Hence the debate could find no easy resolution.

Wave theory was also discussed in Britain, of course, with London polymath Thomas Young a key figure.[2] Others pursued the experimental side and took up the question of polarization by reflection, including an unexpected researcher in Edinburgh: David Brewster. Originally a clergyman with scientific interests, he later became professor of physics and an important academic in Britain. He had been working in optics since the century's first decade and began his most intense experimental studies in response to Malus' findings. Over a period of approximately 40 years he published many papers, often in the *Philosophical Transactions,* plus several books on optical topics. He made many optical discoveries and developed new instruments for both scientific and public use, such as the kaleidoscope. He was also active in reorganizing British science. He became Fellow of many learned societies and academies in Europe and received prestigious prizes, such as the Royal Society's Copley Medal (1815), the Annual Prize of the Paris Institut de France/Academy of Sciences (1816), the Royal Society's Rumford Medal (1818) and its Royal Medal (1830).[3] His research had a specific profile, however: he is usually taken as a central figure in optical *experimentation*, while being thought weak with respect to *theory*. Commenting on a statement of B. Airy, Whewell would later call him "Father of Modern Experimental Optics" (Whewell 1859, 133). The wording was deliberate: while highlighting Brewster's outstanding achievements in experimentation, Whewell—a dedicated wave-theory promoter—remained politely silent about the theoretical side.

From the outset, Brewster's approach differed from what he saw in Paris, and deliberately so. His distancing from the Paris approach is illuminating: he emphasized that he was not interested in theory debates, at least for the time being, but rather in finding and establishing laws. To be sure, his stance on theoretical matters

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[1] For profound studies, see Cantor (1983), Buchwald (1989), and Darrigol (2012), among others.

[2] The British discussion has specifically been treated in Cantor (1975), James (1984), and Buchwald (1992).

[3] There is no up-to-date biography of Brewster; for a starting point, see Morse (1973).

was far from neutral: he was convinced of the corpuscular theory (or, more generally, the "selectionist" theory, as Jed Buchwald has appropriately called it). At the same time, however, Brewster was also convinced that the debate was premature, and could be fruitfully conducted only after further advancements: with laws based on empirical research and more 'rigor,' as we would say, than he saw in others' research. This he set out to achieve in his own work.

Given that background, it is instructive to look more closely at Brewster's experimentation and its outcomes. After all, his prominence depended not only on discovering new effects and instruments, but even more on his formulations for numerous optical laws. The hallmark of his research was reasoning from experiments, in order to formulate new laws. His case is thus instructive for the question of how to control induction in order to support those laws, and my purpose for the next sections is to analyze whether and how Brewster realized that ambition.

One remark on terminology before we continue. Brewster did not discuss methodological questions at length, and as far as I can see never used "induction" in his work for procedures that his contemporaries would have described using that term. So when I speak of induction here there is a certain degree of anachronism. But given my remarks above about the meaning of induction as reasoning from experience to general statements, there should be no obstacle.

My analysis of the historical case has three steps. I first ask about the nature of Brewster's claims, and then about how his experimental approach led him to them. I then ask how he himself reflected, mostly in passing, on that approach.

4.3 Brewster's Epistemic Goal

It was characteristic for Brewster that he started his optical publications with a book, in 1813, on new optical instruments. Here, he focused on the tools that he had invented or improved so far, such as micrometers, goniometers, telescopes, microscopes, and instruments for measuring distances, dispersive, and refractive powers, among other things. He described them in detail with ample illustrations. These instruments he used in all his further research. His tool-based focus underscores a point that he often stressed: to make all experiments as secure as possible, and take seriously all irregularities. Central to this point were close attention to the experimental apparatus and measuring instruments, and rigorous analysis of their workings.

In the book Brewster also described the many findings he had achieved with his instruments so far. At the beginning he gives an overview, and a brief look here will be instructive:

> 4. All doubly refracting crystals possess a double dispersive power, the greatest refraction being accompanied with the highest power of dispersion.
> 11. Light is partially polarized when reflected from polished metallic surfaces.
> 12. The light reflected from the clouds, the blue light of the sky, and the light which forms the rainbow, are all polarized. (Brewster 1813a, x–xii)

These are all empirical statements, some more specific and some more general. Some have the logical form of a conditional: if certain conditions obtain, a certain effect will occur. The statements are bold, moreover, often claiming that *all* things of a certain kind behave in the way described. Later I shall discuss a case illustrating the reasons for such confidence. But statements such as these would characterize his results for decades.

In other texts, Brewster reflected explicitly on the goal of his research, expressing it in various ways:

> …to discover several new properties of light, and to establish the laws which regulate the most remarkable of the phenomena. (1814b, 397)

> …my next object was to ascertain the law of the phenomena in relation to the number of plates and the angle of incidence at which the polarization was effected. (1814a, 220–221)

Elsewhere, following optical experiments on double refraction with other substances (carbonate of strontites, carbonate of lead, and chromate of lead), he felt justified to reach a conclusion and to

> establish the general law, that each refraction of crystals which give double images is accompanied with a separate dispersive power. (1813b, 108)

We also find these later formulations:

> … and we obtain the important law, *That when two polarised pencils reflected from the surfaces of a thin plate lying on a reflecting surface of a different refractive power interfere, half an undulation is not lost, and WHITE-centred rings are produced, provided the mutual inclination of their planes of polarisation is greater than* 90°*; and that when this inclination is less than* 90°, *half an undulation is lost, and BLACK -centred rings are produced; when the inclination is exactly* 90°, *the pencils do not interfere, and no rings are produced.* (1841, 50, emphasis in original)

These examples illustrate the character of the claims he was aiming for. Sometimes he also used more methodological language: "[Philosophy] … can *reduce* to a satisfactory generalization the anomalous and capricious phenomena" (1813a, 314, my emphasis). He also said he was able "to *reduce* the results obtained from *glass* under the same principle" (1815, 126, my emphasis).

These statements show that Brewster had a clearly defined epistemic goal through all his research: to find and establish laws, and to "reduce" individual cases to those. He did not explicate what he meant by "reduce," but others often talked of "reducing" particulars to general laws in his time. I shall return to this meaning, because it differs from later ones. In all of this, of course, the core idea was to make experiments the sole foundation for those laws. We must have that goal in mind when I reflect later on the specificities of his inductive procedure.

And, indeed, Brewster was successful in formulating some laws, with the one we still call "Brewster's law" the most prominent example (with which I will deal in detail below). Whewell probably had this in mind when he characterized Brewster as the "Father of Experimental Optics" (Whewell 1859, 133). Whewell had previously compared Brewster's achievements in optics to those of Kepler in astronomy (Whewell 1837, 462). That analogy is significant, because Whewell regarded Kepler as the one who established the laws of planetary motion—not from theoretical considerations, but from empirical data. He saw and acknowledged the same achievement in Brewster.

4.4 Brewster's Experimental Approach: From Measurements to Laws

How did Brewster arrive at those laws, and what did his experimental procedures look like? To answer these questions, I shall analyze one of his more prominent papers in detail.

In 1815 Brewster announced something entirely new. He had begun studying in detail a phenomenon discovered by Malus: when light impinged obliquely on the surface of a transparent body, the reflected part came out fully polarized at a specific angle of incidence. That angle came to be called the polarizing angle. The challenge, of course, was to determine that angle in many substances, and to find a law connecting to the substances' other optical properties. Malus had set out to do exactly this. He had figured the angle for glass and water and had attempted to correlate the results with optical properties before coming to a negative conclusion:

> The polarising angle neither follows the order of the refractive powers, nor that of the dispersive forces. It is a property of bodies independent of the other modes of action which they exercise upon light." (quoted from Brewster 1815, 125/6).[4]

Brewster had been skeptical about that conclusion and wished to check it with more experiments. He expanded the number and variety of materials, going beyond water and glass to precious stones and other objects. He also used great precaution in his instruments and measurements to achieve high-precision results. In his paper he did not describe his experimental procedure in detail, but some passages indicate that he had always tried to determine "the angle at which the intensity of the evanescent pencil is a minimum" (129). He had already established that in this minimum setup, the "pencil" (his name for a beam of light) was fully polarized, but he explained no further. From many experiments, he was led to suggest—contra Malus—that the angle was in fact correlated with the optical properties of the materials. In addition, he formulated a specific law in mathematical terms.

There was one major obstacle, however: the experiments with glass did not fit the law, even though glass was the most important optical material. This was a difficult epistemic situation indeed.

Like Malus had done, Brewster at first gave up. After a year, though, he returned and found that another precious stone followed the law. He then focused on glass again and saw that the experiments were irregular: he got different results on different surfaces of one and the same piece of glass (126). This puzzling result made him consider the possibility of an unknown factor or source of error.

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[4] In this section, all page numbers refer to Brewster (1815), unless noted otherwise.

He set to analyze the surface of the glass plates, because polarization by reflection is a phenomenon of the surface. In a series of original experiments performed with careful scrutiny, he established that some surfaces had undergone chemical changes across long contact with air. He was able to reproduce those changes experimentally and found ways to avoid them. In other words, he had gained control over the changes—i.e., over a previously unknown factor in the experiment that had caused irregular deviations. Now it could be controlled.

In returning to the original question and performing experiments with unchanged glass surfaces, he was finally successful and found that glass followed the same law he had found for all other materials. He was therefore able to triumphantly formulate the law in all generality:

> Having thus ascertained the cause of the anomalies presented by glass, I compared the various angles which I had measured, and found that they were all represented by the following simple law. *The index of refraction is the tangent of the angle of polarisation.* (127, emphasis in original)

Elsewhere he said he was now able "to reduce the results obtained from *glass* under the same principle" (126, emphasis in original).

This episode illustrates, among things, what he meant by "reducing": he had shown that the individual phenomenon was just a special case of the general law. This understanding of "reduction" under a law or principle means to demonstrate that a specific phenomenon is consistently covered by the law or principle. The understanding also fits well with the earlier quotations. And although the terminology might seem strange for us, as reduction has different connotations to us, this sense was not uncommon in Brewster's time. I have found the term with that specific meaning in Ampère's and Faraday's research on electromagnetism, for example (Steinle 2016), but also as early as the eighteenth century with Dufay and d'Alembert. I discuss more details in a forthcoming paper (Steinle forthcoming), but a broader historical picture remains to be completed.

The episode also illustrates how Brewster dealt with "anomalies," or irregular outcomes that gave different measurements even with the same piece of matter. Such an anomaly could occur only, or so he was convinced, when the experimenter had overlooked some experimental factor. The events thus point to a specific aspect of experimental control: ensuring that the experimenter has a complete view of all the experimental conditions with an effect on the outcome. As the above quotation indicates, Brewster regarded these experimental conditions as the "causes" of the result, which suggests an understanding of causes that resonates with what Mill would describe in his 1843 *System of Logic*. Later, I shall return to that aspect of control.

The relation between polarizing angle and refractive index is what we today call "Brewster's law," and the specific angle "Brewster's angle." For this result, Brewster achieved considerable recognition: he was immediately made Fellow of the Royal Society of London and received its prestigious Copley Medal. A short time later, the Paris Institut de France/Académie des Sciences honored the result and its author.

It awarded to him half the annual prize, carrying a significant monetary award, for the most important scientific discovery in the physical sciences.

To support the generalization, Brewster's publication presented his results in a table showing the strikingly varied materials he had used: from water and various sorts of glass to diamond, crystals, and precious stones, as well as mother of pearl and birdlime. It was a vast collection indeed and must have been costly, even though he had no institution funding his experiments (Table 4.1).

In the columns he gave the material (column 1), the polarizing angle as calculated from the refraction index with the tangent law (column 2), the same angle as measured with his instruments (column 3), and the difference between the two numbers (column 4). Column 5 presented the calculated angles for the material's second surface (e.g., the lower surface of a glass plate with two parallel surfaces), which he discussed later in the paper (in section II of his paper, from p. 134 onwards), but this information was not relevant to formulating the law. Giving an argument with tables was characteristic for him, and he often used the strategy in later writings and with other cases to support general claims from a mathematical formula. The table was the central means to support the inductive claim, and it did so in two ways. First, it made obvious that the measured values had a "very remarkable" (128) coincidence with those calculated from the law. Second, it suggested that, because the law held for so many different substances, it could be generalized to all materials without

Table 4.1 Brewster's table of polarizing angles for various materials (Brewster 1815, 128)

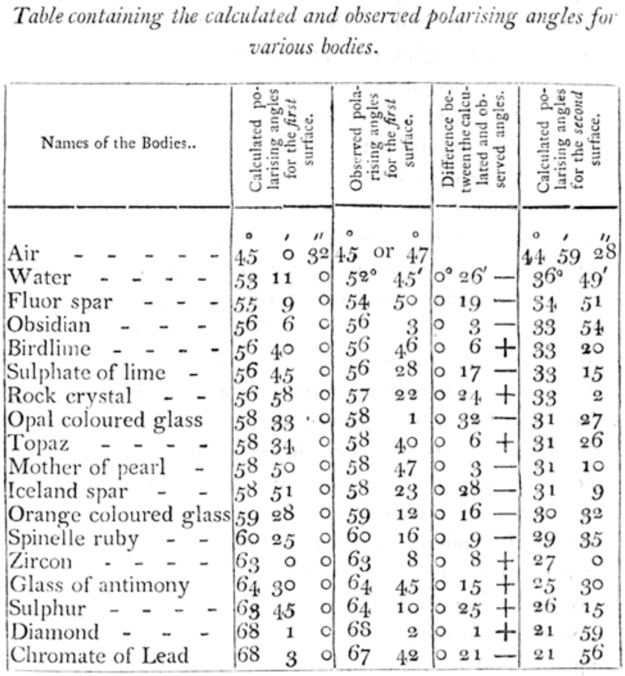

Table containing the calculated and observed polarising angles for various bodies.

Names of the Bodies..	Calculated polarising angles for the *first* surface.			Observed polarising angles for the *first* surface.		Difference between the calculated and observed angles.			Calculated polarising angles for the *second* surface.		
	°	′	″	°	°				°	′	″
Air	45	0	32	45 or	47				44	59	28
Water	53	11	0	52°	45′	0°	26′	—	36°	49′	
Fluor spar	55	9	0	54	50	0	19	—	34	51	
Obsidian	56	6	0	56	3	0	3	—	33	54	
Birdlime	56	40	0	56	46	0	6	+	33	20	
Sulphate of lime	56	45	0	56	28	0	17	—	33	15	
Rock crystal	56	58	0	57	22	0	24	+	33	2	
Opal coloured glass	58	33	0	58	1	0	32	—	31	27	
Topaz	58	34	0	58	40	0	6	+	31	26	
Mother of pearl	58	50	0	58	47	0	3	—	31	10	
Iceland spar	58	51	0	58	23	0	28	—	31	9	
Orange coloured glass	59	28	0	59	12	0	16	—	30	32	
Spinelle ruby	60	25	0	60	16	0	9	—	29	35	
Zircon	63	0	0	63	8	0	8	+	27	0	
Glass of antimony	64	30	0	64	45	0	15	+	25	30	
Sulphur	63	45	0	64	10	0	25	+	26	15	
Diamond	68	1	0	68	2	0	1	+	21	59	
Chromate of Lead	68	3	0	67	42	0	21	—	21	56	

much risk. Hence it justified the bold inductive step. That step was also supported by another aspect of the specific case: even the material that had appeared at first to contradict the law could, under careful scrutiny, be resolved by controlling a hitherto unknown experimental factor. With that factor controlled, the material could be subsumed under the law.

Of course, the procedure of using tables to support general claims, and to compare measured and calculated (or deduced) values, was not new. It had been used in astronomy for centuries and in physical sciences since at least the seventeenth century, with Boyle arguing for the inverse relation of volume and air pressure, for example (the relation later called “Boyle’s law”: Boyle 1662, 59sqq.). Brewster, however, pursued the strategy with particular intensity, always basing it on comprehensive experimentation.

Given my focus on the inductive process, it is significant that Brewster went a step further. To underscore the law’s reliability and precision, he undertook to evaluate the discrepancies between calculated and measured values. While there existed no established procedure at the time to quantify the agreement or disagreement of those values—mathematical error analysis came only later—Brewster still wished to understand them in more detail. He accordingly discussed them in various ways. First, he took a quantitative approach, for one: he added the absolute values of the discrepancies in his measurements and calculated a mean discrepancy of 15′ of an arc. Moreover, he found an asymmetry: the total amount of negative discrepancies was roughly twice that of positive ones. This evaluation of error may seem quite crude to us, but we must remember that he performed it at a time when error analysis in physical measurements had not (yet) been refined. This was true both in Britain and in Paris, where the program of precision measurement had its stronghold. The method of least squares, presented by Gauss in 1809, had been developed and used only in astronomy.

Second, and with greater intensity, Brewster focused on the discrepancies’ possible sources. To explain them generally he pointed to the difficulties of measuring both the index of refraction, which constituted an important numerical factor in the law, and the angle of minimum intensity of the reflected beam, or the polarizing angle. We might surmise that he attributed the mean discrepancy or error of 15′ to these two difficulties, and to the ensuing uncertainties. But this would not have explained the asymmetry between positive and negative discrepancies. For this reason he drove his analysis further and identified two specific sources of uncertainty in measuring the polarizing angle: the practical conditions of observability, and the variations of the angle with color combined with the varying intensities of different colors. Both factors, he concluded, favored the observed tendency to negative discrepancies. With these considerations he could at least qualitatively account for the asymmetry between positive and negative discrepancies.

(As a side note, we might see here a first intimation of what was later called the difference between statistical and systematic errors. Brewster gave only a general explanation for the occurrence of the mean error, but a much more specific one for the asymmetry between positive and negative discrepancies.)

Based on these results, Brewster understood more deeply why and how errors occurred in his measurements. He therefore trusted the empirical law even more now. He emphasized that "the law of the polarisation of light by reflexion [had been] thus experimentally established" (130). In modern terms, this is a significant case of inductive generalization: the researcher knew about the boldness of the inductive step and did everything he could to justify it as much as possible. Key points here were fully grasping *all* relevant experimental conditions and precisely controlling them. In addition, there came at least a qualitative understanding of the remaining "errors," or the deviations between measurement and the law's predictions. Step by step he had succeeded in overcoming those challenges, and hence was able to include even those cases that had not initially fit the law.

At that point Brewster was so confident of the law's validity and generality that he made a most significant epistemic switch: he changed the status of the law in the text from an empirical rule to an unquestioned scientific principle. "It will thus be seen," he wrote, "that the subject assumes a scientific form, and that we can calculate *a priori,* the result of every experiment" (130). While he did not explicate the phrase "scientific form," the subsequent text makes his meaning clear: he no longer regarded the law as a matter of empirical doubt but instead ascribed to it a fundamental degree of certainty. It was certainty so great that the law could, from now on, be an unquestioned starting point for all further investigations. Brewster's change was also manifest in the text's structure: from that point onwards he arranged it in a Euclidian manner, with numbered propositions followed by a sort of proof. The proofs were no longer experimental, but rather just gave the "geometrical consequences" (130) of the law he was now using as a principle.

In sum, we see an impressive pathway. It begins with carefully conducted individual experiments and brings them together in a series, and then rigorously analyzes the relevant experimental conditions. It also offers at least a qualitative understanding of the remaining "errors," or deviations between experiment and expectation. All this leads to a general empirical law. Most strikingly, the end involves an epistemic step whose boldness cannot be overstated—Brewster was so confident in the validity of the empirical law that he raised its status to that of a principle. Thereafter he treated it like a geometrical axiom, and used it as a physical principle for all sorts of geometric deductions. As such, in his mind at least, the principle was no longer subject to empirical test; it was to be taken as absolute, as an axiom. We see the pathway from provisional law hypothesis to full and absolute certainty. I know only few instances in the history of empirical science where a researcher consciously went as far as this last step. Kepler, with whom Whewell compared Brewster, provides a case from astronomy. Crucial elements of the pathway include the procedures of broad experimentation and leaving nothing out: in the included factors, in the breadth of experimental materials, and in analysis of the remaining discrepancies or errors between expectation and experiment. Every step was based on careful experimental scrutiny—highly controlled, and rigorously carried out.

4.5 Brewster's Reflections on How to Support Induction

Before discussing this procedure in a wider context, I shall examine Brewster's own methodological reflections. He was not an epistemologist and did not give methodological rules, but we can still discern his approach. We see it in his practice, in scattered side remarks, and (indirectly) in his criticism of others' procedures.

One striking example is his analysis of previous researchers' failure. After reporting Malus' claim of the non-relation of polarizing angle and optical properties of the materials, he analyzed the background of that failure:

> This premature generalisation of a few imperfectly ascertained facts, is perhaps equalled only by the mistake of Sir Isaac Newton, who pronounced the construction of an achromatic telescope to be incompatible with the known principles of optics. Like Newton, too, Malus himself abandoned the enquiry; and even his learned associates in the Institute, to whom he bequeathed the prosecution of his views, have sought for fame in the investigation of other properties of polarised light. (126).

The critique occurs on many levels. The facts had been too few and they had not been well ascertained; as a result, the generalization was premature. From these points we can see what he thought of as a good, or mature, generalization. The criteria would be:

1. *Many* facts or experiments are needed.
2. Each fact must be *well ascertained*.

The example I have discussed illustrates these points, and how he used the requisite facts to generalize. I shall return to this in a moment.

It is also interesting to note that he included a social aspect: he criticized Malus' generalization as premature, but also criticized others for accepting it too easily. They did not care, he thought, since more "fame" could be gained elsewhere. One could assume that, in Brewster's view, there was not much fame to be won in Paris by the meticulous work it would have taken to improve the earlier failures. To give it yet another twist: looking for fame, perhaps particularly in Paris, might sometimes work against the quality of experimental work and the control of generalizations associated with it. This could be true both for the researcher himself and for his academic fellows, at least if the local academic culture was strongly shaped by specific ideals (such as mathematization) at the expense of others (such as experimental broadness). This remark, concerning the impact that local academic culture (as we might call it) and competition for fame had on the research process, has become a pressing topic in our times. It strikes me as a remarkable observation and critique in Brewster's period. That Brewster made the remark with Paris in mind had probably to do with the historical situation: academic physics in Paris, much more so than elsewhere in Europe, had a dominant epistemic ideal. Even those who no longer followed a strict Laplacian program shared the ideal of mathematization, often at the cost of broad experimentation. There was little chance for visibility in Paris physics without following that ideal. It should also be noted that Brewster, despite his critique of Malus' premature conclusion, expressed deep respect at the end of the paper for Malus as a productive researcher (159).

We must also address the role of theory in Brewster's experiments, as part of his epistemic approach. On the one hand, Brewster claimed to do his experiments independently of any "hypothetical assumption" (158), probably having in mind the debate between the wave and corpuscular theories of light. Indeed, his experimental reasoning did not discuss that question at all. He did not position his findings within that debate, nor do we see his experiments designed with the debate in mind. As Hacking and his co-author Everitt famously characterized it, Brewster just analyzed "how light behaved" (1983, 157). Even if such an expression may sound naïve—there is no 'innocent' analysis of how things behave—it highlights the absence of theory in guiding experiments. Brewster emphasized that optics could advance only "when discovery shall have accumulated a greater number of facts, and connected them together by general laws" (158). Only then could it invent "better names" (158), that is, a more fitting terminology, and "speculate respecting the cause of those wonderful phenomena" (158–159). He thus gave the epistemic process a clear sequence: first facts, then laws, and after that, theories about physical causes like waves and particles.

At the same time, however, as an admirer of Newton (he published a biography in 1831 and more papers on him in 1855), Brewster was convinced of the corpuscular theory. To put it more precisely, he was convinced of the "selectionist' approach to light that Newton had developed from the background of the corpuscular view.[5] At its core were several assumptions: that every beam (or "pencil") of light could be understood as a multitude of individual rays; that all its properties could be reduced (in the meaning sketched above) to the properties of those rays; and that the interaction between a beam and a surface could always be understood in terms of selecting certain rays from the multitude of the beam. As Buchwald has pointed out, Brewster's commitment to this framework did not affect his experimental design, but it manifested in the terminology he used to describe experiments and results. He often spoke of rays and used that framework without much discussing it. He obviously was not aware of all the philosophical baggage such an approach brought with it. Sometimes it was difficult to formulate his findings within that framework, and the result could be contorted expressions that Buchwald described as "hodgepodge" (Buchwald 1989, xix, also p. 259 or 449). However, Brewster did not question the framework.[6]

This is a case of an experimental approach not oriented toward theory or driven by theoretical goals, even while others around Brewster were obsessed by them. At the same time, it was not fully separate from Brewster's own theoretical preferences. Those preferences left linguistic traces in concepts and terminology, and Brewster did not choose or develop a more neutral language. While the above quotation shows Brewster sensing the need for a more appropriate language, he did not invent one. This inflexibility for basic concepts makes Brewster's case differ from others, and most significantly from Faraday's, in Brewster's own day. Faraday knew the importance and laden-ness of terminology, and kept it as flexible as possible (Ross 1961).

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[5] I rely here on the excellent analysis given in Buchwald (1989).

[6] Buchwald (1992) gives a profound analysis.

4.6 Dimensions of Experimental Control

While Brewster himself scarcely spoke of rigor or control—almost no one in the physical sciences did at the time—we can use these analytic concepts to understand and contextualize the historical case. The case indicates that experimental control can be exerted in different dimensions and that it comes in degrees. With Brewster's own experimental practice and reflections thereon in mind, I identify four dimensions with which we can characterize his strict inductive procedure via control and rigor.

The first dimension concerns the reliability and precision, in the sense of precise and well-ascertained numerical outcomes, for *each and every individual experiment or measuremen*t. As Brewster's first book made clear, he regarded this criterion as the foundation for any reliable experiment in optics, and in this respect he criticized Malus for being too sloppy. Every optical experiment must be controlled carefully to allow for the utmost reliability and precision, both in arranging the apparatus and in conducting measurements. Interestingly, Brewster did not mention simple repetition of experiments and measurements; we do not know whether his measurement results, like those given in the table above, were the outcome of single measurements or multiple. In his time discussions about those issues were not happening, although in astronomy they were about to start. Given the difficulties Brewster describes for measuring the polarizing angle, however (129–130, see my discussion above), it is plausible to surmise that he did, at least sometimes, measure more than once. Whether he obtained differing results in those cases, and how he might have calculated the final value for the table, we do not know.

However, Brewster's silence on the issue of repeating measurements and observations indicates a more general point: in most of the physical sciences of his day, repeating experiments was not an important issue. The focus was on controlling relevant experimental conditions so carefully that the outcomes were well-determined and stable even when repeated. This resonates with what Caterina Schürch (this volume) reports as the methodological reflection of Albrecht Thaer in 1809. Thaer noted a difference between those sciences that could fully control their experimental subjects in the closed space of the laboratory—he was thinking of chemistry and perhaps also of physics—and those that could never achieve that control. The most obvious of the latter would be those involving living beings, like plants or animals. The corresponding experimental strategies were described differently: in the first group, "completely perfect and pure experiments" could be performed, probably without needing repetition or comparative experiments. But the second group needed that method. While in physics, with increasing importance of precision measurement, such a view on experimentation would change in the decades to come, it might still have been possible around 1810.

The second dimension of control in Brewster concerns the goal of knowing about *all relevant* experimental factors, i.e. all those that affect the result, and to be able to control them, i.e. is to keep them constant or to vary them at will. A puzzling moment in Brewster's experiments occurred when he realized that he obtained different outcomes for the polarizing angle, even when he used the same specimen of glass but observed different surfaces of it. This result made him aware that the outcome was determined not only by the type of glass and its known optical properties. There had to be another, unknown factor, belonging to the different surfaces of the glass, even if all shared the same refractive index. He identified that factor successfully and thus regained full control of the experimental situation—that is, he knew all the experimental parameters required to determine the polarizing angle. As for terminology, Brewster did not speak of an "error" at all in this situation; rather, he spoke of "anomalies." Such anomalous results should not occur once the experimenter knew all the relevant experimental parameters.

Of course, this dimension is in one respect very common to experimental work. The strategy of varying parameters systematically, so important in experimenting, intends to discover which experimental parameters are relevant for the effect in question, and which are not. Coko (this volume) provides another striking example and explicates the strategy. What has seldom been studied, by contrast, is another way researchers might become aware of the problem: by obtaining results that are "anomalous" in Brewster's sense, or results that should not occur if the experimenter already knew all relevant parameters. When confronted with such results, experimenters might wonder about and initiate the search for unrecognized but relevant experimental factors.

A third dimension of control concerns error analysis. Brewster spoke of "errors" with a specific meaning: they were discrepancies between the results expected from the (perhaps still hypothetical) law, and those obtained from actual measurement. As I described above, he discussed possible sources of those discrepancies and arrived at least at a qualitative understanding of their occurrence and distribution. It is important to note that the factors he identified were all based on actual procedures and the conditions of observation and measurement. As I have suggested, he needed this understanding to take the bold inductive step after that discussion: to promote the law from an empirical statement to a principle that would no longer be subject to empirical uncertainty. Understanding error sources enhanced his control over the experiment and so was essential for induction.

We ought to remember that these three dimensions of experimental control were not unfamiliar at the time. Both the ideal of precision measurement and of analysis on measurement error had originated centuries earlier in astronomy, where Tycho Brahe is a striking example. Both started to be introduced into the experimental sciences during the final decades of the eighteenth century. Nevertheless, they were still not common in Brewster's day. Even in Paris, where a mathematical approach to physics had been thriving[7] and hence the issue was most pressing, there were no

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[7] For a "locus classicus" see Robert Fox's characterization of Laplacian physics (Fox 1974), and Norton Wise's collection on the "Values of precision" (Wise 1995).

common procedures for analyzing possible measurement errors.[8] In his optical experiments, Brewster knew what could be achieved with reliable precision and error analysis, and he was among the first to practice them in Britain. What he criticized in his Paris colleagues was not the lack of control, but the degree, insufficient in his eyes, to which researchers had implemented the three mentioned dimensions in optical research on polarization. To use our terms here, his criticism concerned insufficient rigor in implementing control procedures. That insufficiency itself was, as he suggested indirectly, probably attributable to the heated atmosphere in Paris academia, which did not reward such rigor. His own optical research, by contrast, provides a striking case of the success of those ideals. It appears not only in the polarizing angle but also in other achievements as well, including what came to be called “Fresnel’s formulas.” On the empirical side, those were the outcomes of Brewster’s meticulous measurements.

However, Brewster’s case also points to a fourth dimension by which experimental research can be well-controlled and rigorous. It deals less with individual experiments and more with how to arrange them as a group. What Brewster did was use the same experimental procedure—measuring the angle of polarization—and apply it to as many materials as he could (as long as they were appropriate; they needed reflecting surfaces, for example). Determining the polarizing angle was in itself a procedure far from trivial, and it required strict control in the three dimensions listed above. But what he did (and required as part of his practice) was to use that procedure on a broad range of materials while leaving other parameters unchanged. Only with such variation, or so he claimed, could one build the inductive argument needed to formulate a law. In other words, it would be impossible to base the law on a small sample or individual collection of experiments. It could come only from a group of experiments that was well-structured, coherent, and as large as possible. Within a group like that, everything remained the same except one parameter that researchers systematically varied—in this case, the material to be analyzed. That variation made the group coherent and gave the central epistemic argument for the induction process. Not the individual experiment, but only the whole group, designed to be internally coherent, could serve as a basis for the inductive step.

This dimension of experimental control is hugely important, and I shall add some observations. First, presenting those experimental results in a table aligns with that dimension and its procedure: for each line in the table, the basic situation is the same. Only one parameter—the material—is varied, and for each variation there is a new line, with the parameter in the first column and the results in the others. To be sure, not all experimental groups in Brewster’s research were presented in such

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[8] To my knowledge, we still do not have a comprehensive picture of how those procedures made their way into physical and chemical experiment. Some of the articles in Wise (1995) touch on the topic; see also Hoffmann (2006). Astronomy is better studied here; for examples, see Schaffer (1988) and Hoffmann (2007). A workshop in Dresden (September 2021) on “Promises of Precision—Questioning ‘Precision’ in Precision Instruments,” organized by Sibylle Gluch, made another attempt at the project, but no publications have yet resulted.

tables, as when there were no measurements or numbers involved. But the appearance of the tables suggests that a group of that kind had been created.

Second, and with respect to the history, it is obvious that this dimension of control was not present in Paris in his time. Indeed, the lack of such experimentation was one of the central critiques Brewster posed to his Paris contemporaries. And his intriguing remark about the local practices probably hit a crucial point: in Paris, with its intense atmosphere of mathematizing ever-new domains, such meticulous work was less honored than was finding new effects and mathematizing them.

Third, this dimension of experimental control is intimately connected to the epistemic goal of establishing regularities and laws from empirical (usually experimental) research. This happens in cases in which explanatory theory is either not available or deliberately kept excluded (e.g., because it is thought premature). While such a goal might resemble the general empiricist ideal, formulated repeatedly since Bacon, of basing scientific insight on broad empirical input, this one is much sharper: the type of scientific insight is clearly defined as laws, in contrast to explanatory theories. The procedure is also clearly spelled out. The empirical foundation is not just a collection of experiments, but a highly structured and well-ordered one, often in the form of an experimental group, as described above.

In the history of the physical sciences, we find many cases of just such a connection between an epistemic goal and this dimension of experimental control. I shall note further cases below. When and where exactly that connection had its first historical appearance is difficult to say, but it may already have existed in the seventeenth century in Mariotte (see Steinle forthcoming). Here we find a tradition more bound to specific epistemic goals than to local cultures. At the same time, the claim of basing laws on empirical findings has not always involved that specific type of control. There are many cases in which the argument for a law's empirical validity had a different structure, and the tradition into which Brewster's approach fits is not identical with the more general tradition of looking for empirical laws. Hence, In my final section I shall discuss these considerations in more detail.

4.7 Experimental Control and Empirical Laws

The epistemic goal of establishing empirical laws, in contrast to the search for causes or explanatory theories, has been formulated and practiced at least since the early modern period.[9] Those laws have taken different forms, including mathematical proportions or formulas like the sine law of optical refraction or Hooke's law of force of the elastic spring; they have also appeared in non-mathematical if-then statements, such as Dufay's law of electric attraction and repulsion. The process leading to those laws has often been connected to the idea of induction, and in many

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[9] The concept and terminology of laws of nature has itself a complex history, but came into common acquaintance in the seventeenth century; see the contributions in Daston and Stolleis (2008).

cases experiment was the central means of research. However, views on how exactly to understand that induction process, and how to conduct and control experiment, differed widely. I shall sketch just two specific examples, which probably lie at the two ends of a spectrum.

One can be seen in Brewster's research. It has the general plan of creating a series (or several series) of closely connected experiments, covering as much empirical ground as possible. It also involves systematically varying parameters as the core of the experimental procedure. Only sufficiently broad arrangements were regarded as solid bases for both the inductive step and for the law. In these cases, theoretical explanations—or even just strong conceptual frameworks—were typically not available, or were deliberately excluded from the process (as happened with Brewster). The experimental approach is inherent in what I have elsewhere described as exploratory experimentation (Steinle 2016, ch.7, among others). As prominent historical cases, one could include here, among others, Hooke's law of 1678 (although we have little documentation for how he arrived at it), or Dufay's research on electricity. Dufay had explicitly postponed all questions of theoretical explanation and focused solely on laws, whereby he had formulated the law of electric attraction and repulsion, among others (1742; see Steinle 2006). One might also include the law of definite proportions in chemistry, formulated by Proust in 1797, or Faraday's research on electromagnetic induction with the resulting law in 1832 (see Steinle 1996). This approach was explicitly addressed by different authors; d'Alembert spoke, perhaps with someone like Dufay in mind, of the need to "multiply" (vary) phenomena in experimental physics, and to make them "a chain with as few missing links as possible" (d'Alembert 1756, 301). Faraday described the core of his experimental procedure in a letter to Ampère as "facts closely placed together" (James 1991, letter 179). Brewster might have been the first to follow such a procedure in a domain based on precision measurement.

On the other hand, we find very different constellations, viz. those in which the law was strongly suggested by more general considerations. It was often framed by an overall theory and then "confirmed" by a small number of selected experiments. One prominent case might be Coulomb's force-law of electric repulsion (1785; see Heering 1994): in support of this law he published exactly three experimental data from one measurement with his torsion balance. Another case might be the law of electromagnetic action, presented by Biot and Savart in 1820. It was the result of few but highly delicate experimental measurements (see Steinle 2016, ch.3). We could also add Malus' research: while he shared Brewster's goal of establishing a law for the polarizing angle, he tested only two materials and gave up when one did not give the expected result. The idea of widening the scope and including more materials had obviously not been part of his approach for an empirical law.

In all of these cases, experimental control was very different from control in the first group. While these strongly emphasized the precision and reliability of one or few experiments, there was no intent to embed them in a broader field of connected experiments. The very idea of using a single experiment or an otherwise small sample as "proof" of a law or a general statement had been most prominently presented by Newton, in the first book of his 1704 *Opticks*. We see the same even earlier in discussions about his 1672 “new theory” of light and colors, where he insisted on setting aside further experiments and put all the weight on the *experimentum crucis* (letter to Oldenburg of 16 May 1676, in Turnbull 1960, 79). Induction was understood here in a very different way, one much less exploratory and systematic. The focus of experimental control was likewise substantially different.

To illustrate these differences I shall use a case in which we see the inductive approach shift in a single researcher within a short period. The type of experimental control also shifted. The case is instructive because we also see how these two approaches, and the shift between them, connect to specific epistemic goals (which can also switch). The episode concerns A. M. Ampère's reaction to the surprising discovery of electricity's action on magnetism, communicated by Ørsted in July 1820.

I have elsewhere elaborated the case in more detail (Steinle 2016, chs. 3 and 4), and shall here focus on just one aspect. The new effect challenged established thinking because it involved complex spatial issues; at first it was impossible to grasp it in traditional terms of attraction and repulsion, i.e. with the concept of central forces as it had been so successfully mathematized in Paris. In this situation of deep conceptual uncertainty, Ampère started out looking for laws—he also spoke of “general facts” (“faits généraux”), to which all the other phenomena should be “reduced.” To find them he performed broad experiments with relevant instruments, and the centerpiece of experimental control was a systematic and broad variation of experimental parameters. With his core result he could formulate two “general facts,” which gave the necessary and sufficient conditions for electromagnetic action to occur. He also detailed the direction of that action, captured in what was later called “Ampère's swimmer rule.”

Before finishing, however, he abruptly changed his research agenda. Not only had he discovered a totally new effect—the interaction of currents without magnetism—but he became also quickly convinced of two things. First, this new effect could explain also electromagnetic effects and, second, its ultimate cause was the interaction of infinitesimal current elements, much in the mode of central forces. All his effort then turned toward demonstrating the first point and, for the second point, toward finding a mathematical law for that force. His experimental approach therefore changed completely: he designed few and very specific experiments for just these two purposes. When after great pains he succeeded, those few experiments corroborated not only the general thesis but also the specific mathematical law that he had framed from various non-experimental considerations. When he presented the law to the Paris academy in December 1820, its empirical supported consisted of a few different and well-selected experiments. Of his former approach, which covered a broad range of experiments and established “general facts,” nothing was left.

The episode strikingly shows the connection between experimental procedure and epistemic goals. It also nicely illustrates the general preferences among Paris scientists of the period, which Brewster had so sharply criticized: Ampère had left his broad, exploratory work at a point where he knew it was not finished and had many questions outstanding. However, the approach of formulating a mathematical law for the new electrodynamic action was much more promising in the heated Paris environment than an approach of further solidifying the laws (or "general facts") of electromagnetism, or of broadening their empirical basis.

4.8 Epilogue

Returning to the introduction, I hope to have illustrated and substantiated three claims. We see experimental research in Brewster's case that is highly controlled and rigorous throughout, but we can also differentiate between at least four dimensions of experimental control. These are (1) securing every individual experimental outcome, (2) embracing all relevant experimental factors and leaving none out, (3) rigorously analyzing the sources of observation error and measurement error, and (4) creating a whole field of closely connected experiments to provide the central means for supporting a law. Brewster's story also makes clear how the specific invocation of experimental control relates to the epistemic goals in question; I have mentioned other historical cases to develop that point further. Finally, with a focus on a specific historical context, we see how, in nineteenth century experimental optics, two different traditions of experimental control and rigor united, resulting in remarkable optical achievements.

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