AUGUSTUS DE MORGAN, POLYMATH

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Karen Attar, Adrian Rice and Christopher Stray (eds), *Augustus De Morgan, Polymath: New Perspectives on his Life and Legacy*. Cambridge, UK: Open Book Publishers, 2024, https://doi.org/10.11647/OBP.0408

ISBN Paperback: 978-1-80511-326-3
ISBN Hardback: 978-1-80511-327-0
ISBN Digital (PDF): 978-1-80511-328-7
ISBN Digital eBook (EPUB): 978-1-80511-329-4
ISBN HTML: 978-1-80511-330-0
DOI: 10.11647/OBP.0408

Cover image: Portrait of Augustus De Morgan, in Sophia Elizabeth De Morgan, *Memoir of Augustus De Morgan (1882)*, https://commons.wikimedia.org/wiki/File:Augustus_De_Morgan_1850s.jpg. Background: Nico Baum, White round light on gray textile (2020), https://unsplash.com/photos/white-round-light-on-gray-textile-xZroI5V_dxc.
Cover design: Jeevanjot Kaur Nagpal.

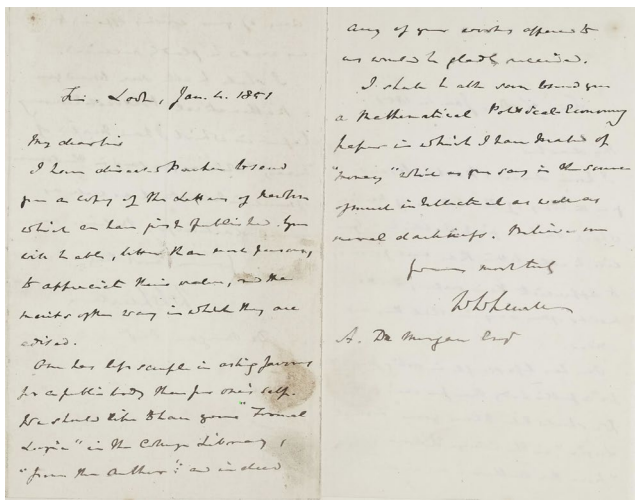

Fig. 7 In this letter from January 1851, tipped in *Correspondence of Sir Isaac Newton and Professor Cotes* (1850), William Whewell tells De Morgan that he has directed the publisher to send him a copy of this recently published edition of Newton's letters, requests a copy of De Morgan's *Formal Logic* for the Trinity College Library 'from the author', and promises to send him a new paper on the subject of money, 'which as you say is the source of much intellectual as well as moral darkness'. ([DeM] L [Newton] SSR, reproduced by permission of Senate House Library, University of London.)

5. Augustus De Morgan: Meta-Scientific Rebel

Lukas M. Verburgt

I shall be amused if you succeed in persuading the world that Bacon had little to do with the modern progress of science.

—William Whewell to Augustus De Morgan[1]

Baconianism and the British Meta-Scientific Tradition

Augustus De Morgan lived in what for science and philosophy were interesting times. During the so-called 'Second Scientific Revolution',[2] natural philosophy transformed into science which, in turn, was slowly divorced from philosophy. Looking at the world scientifically or philosophically eventually became two very different things, except for the polymath, a special kind of scholarly persona which for several decades remained an intellectual possibility, albeit an

1 Letter from William Whewell to Augustus De Morgan, 18 January 1859, Trinity College, Cambridge (henceforth TCC), Whewell Papers, O.15.47/25. Adapted with the permission of the University of Chicago Press from Verburgt, "Scientific Method, Induction, and Probability: The Whewell–De Morgan Debate on Baconianism, 1830s–1850s," published originally in *HOPOS: The Journal of the International Society for the History of Philosophy of Science*. © 2024 International Society for the History of Philosophy of Science. All rights reserved.

2 For more or less obvious reasons—think only of the doubts that have been raised about the meaningfulness of the term 'Scientific Revolution'!—this label has never really stuck or been much used. Still, Enrico Bellone's book on it, published under the general title of *A World on Paper*, contains a lot that is of interest. See Enrico Bellone, *A World on Paper: Studies on the Second Scientific Revolution* (Cambridge, MA: The MIT Press, 1980 [1976]).

 https://doi.org/10.11647/OBP.0408.05

increasingly problematic one.[3] Indeed, with the notion of science not yet a straightforward or finished matter, and philosophical reflection on science not yet separated from the actual practice of science, it is unsurprising that the most important commentaries on science in this period came from polymathic figures, such as Herschel, Brewster and Whewell in Britain, Comte, Bernard and Poincaré in France and Liebig and Helmholtz in Germany. The growing rift between science and philosophy in the first half of the nineteenth century can be seen, for example, in the fact that science was often defined by pitting its virtues against the vices of philosophy. John Herschel, in his wildly popular *Preliminary Discourse on the Study of Natural Philosophy* (1830) praised the 'experimental philosopher' by contrasting him with the 'speculative philosopher', writing that only the former's thinking is 'grounded in the realities of nature' and governed by clear 'principles'.[4] Similar examples of writers contrasting scientists and philosophers can be found in the British, German and French contexts, which arguably stand in need of comparative analysis. It makes for a fascinating chapter of a larger story about the continued entanglement of science and philosophy, even as they were being prised apart in the aftermath of natural philosophy.

Within this sweeping process, the polymathic field of 'meta-science', of which Herschel and William Whewell were the major representatives in Britain, alongside David Brewster, Baden Powell and John Stuart Mill, indeed played an interesting double role. Neither philosophy nor science, but still a little bit of both, meta-science[5] or, as Whewell sometimes called it, philosophy of knowledge, created as much as it filled

3 On the transformation of natural philosophy into the sciences see the essays in David Cahan, ed., *From Natural Philosophy to the Sciences. Writing the History of Nineteenth Century Science* (Chicago and London: The University of Chicago Press, 2003). A history of the downfall of the polymath as a scholarly persona has still to be written. The following works provide useful starting points: Peter Burke, *The Polymath: A Cultural History from Leonardo da Vinci to Susan Sontag* (New Haven and London: Yale University Press, 2020) and Jeroen van Dongen and Herman Paul, eds, *Epistemic Virtues in the Sciences and the Humanities* (Cham: Springer, 2017).

4 John F.W. Herschel, *A Preliminary Discourse on the Study of Natural Philosophy* (London: Longman, Rees, Orme, Brown & Green, and John Taylor, 1830), p. 12.

5 On this term see Richard Yeo, *Defining Science: William Whewell, Natural Knowledge and Public Debate in Early Victorian Britain* (Cambridge: Cambridge University Press, 1993), especially Chapter 3. For a critical note on the term see Steffen Ducheyne, 'Whewell's philosophy of science', in *The Oxford Handbook of British Philosophy in the Nineteenth Century*, ed. by W.J. Mander (Oxford and New York: Oxford University Press, 2014), pp. 71–88 (pp. 84–85).

the vacuum left by natural philosophy between the old ('traditional') philosophy and the new science. In brief, it promoted science as the producer of all 'real', that is, stable and certain knowledge and made it philosophy's (partly epistemological and partly methodological) task to study the nature and conditions of its product. Rather than through a transcendental inquiry, in the good Kantian fashion, which by the early nineteenth century had just crossed the Channel, meta-scientists turned to history to explain the possibility of scientific knowledge. This is to say that philosophical study took the form of systematic reflection on progress in the physical sciences. 'We purpose to collect our doctrines concerning the nature of knowledge, and the best mode of acquiring it,' wrote Whewell, 'from a contemplation of the structure and history of those sciences …, which are universally recognised as the clearest and surest examples of knowledge.'[6] Despite all their underlying disagreements, this is what the towering figures of Herschel and Whewell are believed to have in common: unlike their shared hero Francis Bacon, they were able to ground their philosophy of science on the actual history of the sciences, as these had successfully developed since the time of Isaac Newton. This opportunity came with the demand to pursue meta-science as a combination of philosophically-informed history of science and historically-informed philosophy of science. The programmatic ambition was to renovate Bacon's *Novum Organum* (1620) by first unearthing 'the larger features of [science's] formation', then systematizing these historical features as philosophical principles, and finally showing that these principles were 'exemplified in the history of [science's] progress'.[7] The new category of 'science' was canonised, and the 'scientist' was invented, in the work of the meta-scientists as a result of a historical-philosophical looping effect: philosophy explained what history showed through philosophical reflection on the historical record.

Among meta-scientists, the central feature of science that set it apart from other historical forms of knowledge was widely believed to be its use of a single scientific method. This was not only what made possible science's progress, but also what accounted for science's unity.

6 William Whewell, *History of Scientific Ideas. Volume I* (London: John W. Parker, 1858), p. 8.

7 See Herschel, *Preliminary Discourse*, Part II.

'The advances which have, during the last three centuries, been made in the physical sciences,' wrote Whewell in his 1858 *Novum Organon Renovatum*, 'these are allowed by all to be real, to be great, to be striking;

> may it not be that the steps of progress in these different cases have in them something alike? May it not be that in each advancing moment of such knowledge there is some common principle, some common process? May it not be that discoveries are made by an *Organon* ['Instrument'] which has something uniform in its working?[8]

The scientific method was seen as the very hallmark of science and, consequently, discussions on methodology stood at the heart of the meta-scientific tradition. During what C.S. Peirce once called the 'Age of Methods',[9] meta-scientists across Europe set out to philosophise scientific methodology. For the first time in the long history of philosophy, 'entire books rather than prefaces or chapters'[10] were devoted exclusively to the subject of the principles and rules of scientific inquiry. This large body of work is commonly seen to have been instrumental in the establishment of science as both a specific realm of knowledge and as a dominant way of knowing, teachable to all ('accessible'), common to all branches of science ('single'), and extrapolatable from physical science to any other field ('transferable').[11]

Within Britain in the first half of the nineteenth century, debates on scientific methodology took place against the background of new Bacon scholarship, up to the point of being indistinguishable from it. Bacon was studied almost exclusively as a theorist of method and every account of scientific method—indeed, any new scientific field—had to be at least 'ceremonially Baconian'[12] if it aspired to respectability.

8 William Whewell, *Novum Organon Renovatum* (London: John W. Parker, 1858), p. iv.

9 See Henry Cowles, *The Scientific Method: An Evolution of Thinking from Darwin to Dewey* (Cambridge, MA: Harvard University Press, 2020).

10 Larry Laudan, 'Theories of Scientific Method from Plato to Mach: A Bibliographic Review', *History of Science*, 7:1 (1968), 1–63 (p. 29).

11 See Richard Yeo, 'Scientific Method and the Rhetoric of Science in Britain', in *The Politics and Rhetoric of Scientific Method. Histories Studies*, ed. by John A. Schuster and Richard R. Yeo (Dordrecht & Boston: D. Reidel Publishing Company, 1986), pp. 259–97 (p. 262).

12 Jonathan Smith, *Fact and Feeling: Baconian Science and the Nineteenth-Century Literary Imagination* (Madison: The University of Wisconsin Press, 1994), p. 15.

More precisely, meta-scientific writings on method often amounted to a revision of Bacon's canons of induction, sometimes implicitly and at other times explicitly, as in the case of Whewell's *Novum Organum Renovatum* (1858). This does not merely suggest that particular views on methodology always went hand in hand with, and sometimes even coincided with, specific interpretations of Bacon. It also means that the debates in which these views were put forward were themselves shaped by tacit yet dominant Baconian assumptions about the nature of science and the aims, scope and limits of methodology. Among these assumptions was the fundamental idea that scientific knowledge is acquired through induction. Because everyone also agreed that Bacon's conception of induction was essentially flawed, one main challenge for meta-scientists was that of defining what it actually was.

The reason for the dominance of Baconianism in early nineteenth-century Britain was the all-pervasive influence of Whewell and Herschel, whose commitment to Bacon can be traced back to their student days and whose mature writings can be read as a struggle over who was Bacon's legitimate heir. Other reasons, which tellingly enough have been unearthed largely on the basis of studies of Whewell's and Herschel's life and work, all have to do with the fact that positions on methodology were part of a more wide-ranging set of debates on the nature of science.[13] As such, different takes on Baconian induction as the method of science came with different commitments on broader topics ranging from the organization and religious and social implications of science to the moral character of its practitioners. Or, *vice versa*, as illustrated for instance by Whewell's opposition to the Oxford Noetics, these wider commitments were often defended in terms of abstract methodological considerations.

Perhaps the best example of the dominance of Baconianism is that disagreements within the meta-scientific debates over methodology took place against a shared background of agreement. Put more strongly, even Baconian revisionism, however non-Baconian in appearance, was pursued in terms of a commitment to Baconian induction. Whewell,

13 See, for example, Laura J. Snyder, *Reforming Philosophy: A Victorian Debate on Science and Society* (Chicago: The University of Chicago Press, 2006); and Richard Yeo, *Defining Science: William Whewell, Natural Knowledge and Public Debate in Early Victorian Britain* (Cambridge: Cambridge University Press, 1993).

Herschel and Mill had very different views on what induction is, but while each was convinced of the shortcomings of Bacon's canons of induction, each saw his task as being that of renovating these canons. As always, the force of the *communis opinio* became most apparent when it was challenged. This was already the case when self-declared Baconians like Whewell and Herschel accused each other of diverting too much from Baconian tradition; Whewell when he was being 'too a priori' and Herschel when he allowed for too much speculative hypothesising. It was, of course, even more so in the case of those who self-identified as anti-Baconian.

British Anti-Baconianism

Here, De Morgan enters the picture. Like David Brewster and Charles Babbage before him and William Stanley Jevons after him, De Morgan was among the few prominent British meta-scientists who advocated anti-Baconianism, thereby occupying a somewhat anomalous or rebellious position towards the dominant British meta-scientific tradition. Much like Baconianism itself, as a 'counter-tradition' anti-Baconianism was highly heterogeneous. For example, De Morgan, who was not 'among the strongest supporters of Bacon', defended Bacon on some points, and Richard Whately, who frequently scoffed at Bacon's inductive logic, published an annotated edition of Bacon's *Essays*.[14] Perhaps the best definition that can be given of British anti-Baconianism, one that (luckily!) suffices for the aims of this chapter, is that its members opposed the philosophical core and the historical significance of Baconian induction, understood as a 'new method of arriving at truth'.[15] What followed from this rejection, and what came in its place, is much harder to determine, and differed almost

14 Augustus De Morgan, 'The Progress of the Doctrine of the Earth's Motion between the Times of Copernicus and Galileo, Being Notes on the Ante-Galilean Copernicans', *Companion to the Almanac for 1855*, 5–25 (p. 11); Richard Whately, *Bacon's* Essays*: With Annotations* (London, 1856). Interestingly, several nineteenth-century authors actually saw Whately as a contemporary Bacon. See, for example, William John Fitzpatrick, *Memoirs of Richard Whately, Archbishop of Dublin. Volume I* (London: Richard Bentley, 1864), p. 55 and pp. 325–26.

15 Augustus De Morgan, *Formal Logic: Or, The Calculus of Inference, Necessary and Probable* (London: Taylor & Walton, 1847), p. 216.

from anti-Baconian to anti-Baconian. This probably explains, yet of course does not justify, the lack of any sustained, book-length account of British anti-Baconianism in the early- and mid-nineteenth century. On the basis of primary and secondary sources scattered across time and disciplines—from nineteenth-century logical treatises to recent studies of scientific biography—it should be possible, however, to begin sketching its contours. Doing so is worthwhile for at least two reasons. First, to bring into view an important and, in hindsight, pioneering meta-scientific current in early Victorian Britain. Second, to obtain a fuller and richer understanding of the intellectual landscape in this fascinating period.

A preliminary step to this larger project will be taken in this chapter by focusing on De Morgan as a prominent advocate of anti-Baconianism, and more specifically on his anti-Baconian scientific methodology. Despite the prominence and influence of British anti-Baconians, there exist relatively few studies which engage them as meta-scientists. Menachem Fisch and Simon Schaffer's *William Whewell: A Composite Portrait*, Richard Yeo's *Defining Science*, Pietro Corsi's *Science and Religion*, Nicholas Capaldi's *John Stuart Mill: A Biography*, and Laura Snyder's *Reforming Philosophy* provide in-depth, contextualizing accounts of Whewell, Mill and Baden Powell.[16] No similar books on Whately or Brewster are available yet. The same goes for De Morgan, who stands out even among these men for rarely, if ever, being recognized as a meta-scientist or, to use modern terminology, a historian and philosopher of science. There are myriad papers and chapters on what can be taken to be aspects of De Morgan's meta-scientific outlook on methodology—several on logic, a few on probability theory and statistics, and a small handful on history of science and history of mathematics—but none in which these are brought together.[17] This is not altogether surprising. One reason concerns the current disciplinary boundaries between the

16 It may here be remarked that Babbage's and Herschel's work as meta-scientists, or even as scientific methodologists, is also curiously little studied. But see *The Cambridge Companion to John Herschel*, ed. by Stephen Case and Lukas M. Verburg (Cambridge: Cambridge University Press, 2024).

17 A valuable exception is Joan L. Richards, *Generations of Reason: A Family's Search for Meaning in Post-Newtonian England* (New Haven: Yale University Press, 2021), although its focus seems to be less on De Morgan's meta-scientific than on his personal outlook on science.

history of mathematics, history of logic, history of science and history of philosophy, which need to be crossed in order to bring De Morgan's anti-Baconianism into view. Another reason is that De Morgan himself never wrote a book which combined the meta-scientific elements of his thinking into an integrated outlook on science and its methodology. Within De Morgan's large oeuvre, logic and probability existed adjacent to the history of science without often explicitly intersecting. De Morgan made connections only very occasionally, and when he did, it was mostly in reviews or private correspondence.

The fact that these meta-scientific connections in De Morgan's work exist, and that it is therefore possible and fruitful to think of him as a meta-scientist, can be borne out in a number of ways. The route chosen here is to focus on De Morgan's interactions on topics related to scientific methodology with William Whewell, Master of Trinity College, Cambridge, leading meta-scientist of the early Victorian era, and author of such epoch-making works as *Astronomy and General Physics Considered with Reference to Natural Theology* (1833), *History of the Inductive Sciences* (1837) and *The Philosophy of the Inductive Sciences* (1840).[18] Drawing on their published work and largely unpublished correspondence, several major points of conflict will be identified and interpreted in terms of a friendly clash over Bacon and Baconianism, itself reflective of a larger shift within nineteenth-century debates on scientific method.

De Morgan and Whewell: Scientific Friends, Meta-Scientific Rivals

Whewell's and De Morgan's personal connection began as one of teacher and pupil at Trinity College, Cambridge, where Whewell was fellow

18 Another possible route would be to focus on De Morgan's views on Bacon's and, especially, Newton's personality, which could be contrasted with Whewell's views on this topic. See, in this regard, Richard Yeo, 'Genius, Method, and Morality: Images of Newton in Britain, 1760–1860', *Science in Context*, 2.2 (1988), 257–84. Maria Panteki has also provided a comparative analysis of De Morgan and Whewell, but her account focuses on their respective views on mathematics education. See Maria Panteki, 'French "Logique" and British "Logic": On the Origins of Augustus De Morgan's Early Logical Inquiries, 1805–1835', in Dov M. Gabbay and John Woods, eds, *Handbook of the History of Logic. Volume 4: British Logic in the Nineteenth Century* (Amsterdam: North-Holland, 2008), pp. 381–457.

and head tutor by the time that De Morgan entered as a student there in February 1823. Like Whewell, who had graduated Second Wrangler in 1816, De Morgan began his career conventionally as a (more or less) serious reading man, coming out Fourth Wrangler in 1827. But unlike Whewell, who climbed the ladder at his alma mater all the way from sub-sizar to Knightbridge Professor of Moral Philosophy (1838-55) and Vice-Chancellor (1842-55), De Morgan afterwards pursued an unconventional university career. When in 1827 he rejected the religious tests necessary to obtain a fellowship or a regular post, he knew there was no future for him at Cambridge. Instead, in 1828, De Morgan was appointed to the first Chair of Mathematics at the newly founded, religiously neutral London University, where he rapidly developed into a successful teacher and prolific writer.

During the 1830s–60s, when both men were at the height of their powers, Whewell and De Morgan stood on almost opposite sides on the intellectual, political and social landscape. Whewell was 'a high Tory Anglican' who made it his life's task to defend the 'elite exclusivity' of Oxbridge, whereas De Morgan was a religious radical 'committed to educating all of England's people'.[19] Perhaps the single most telling fact, in this regard, is that Whewell always remained behind the walls of Trinity College, Cambridge and De Morgan, like Babbage before him, moved to metropolitan London. This difference was reflected in the many contrasting aspects of their lives and work, whether it was the kind of mathematics they pursued—traditional British mixed mathematics versus formal Continental analysis—the type of publication venue through which they communicated their views—text-books and relatively expensive treatises versus hundreds of contributions to the cheap *Penny Cyclopaedia*—or the reasons they had for criticising the Royal Society—its inability to guard against scientific charlatans versus its failure to replace aristocratic dilettantes.[20]

19 Richards, *Generations of Reason*, p. 245. See also Crosbie Smith and M. Norton Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989), Chapter 6.

20 The number of essays relevant to these differences is enormous; particularly important are Timothy L. Alborn, 'The Business of Induction: Industry and Genius in the Language of British Scientific Reform, 1820–1840', *History of Science*, 34:1 (1996), 91–121; William J. Ashworth, 'The Calculating Eye: Baily, Herschel, Babbage and the Business of Astronomy', *The British Journal for the History of Science*, 27:4 (1994), 409–41; and Richard Yeo, 'Genius, Method, and Morality'.

Nonetheless, from the early 1830s onwards, Whewell and De Morgan were 'scientific friends' with an epistolary relationship.[21] Their correspondence, which started in 1832 and lasted until 1866, the year of Whewell's death, shows an intellectual kinship based on shared interests in, rather than doctrinaire agreement on, a wide-ranging set of topics, including Kantian philosophy, history of science, especially Newton, and Aristotelian logic. The fact that this kinship could blossom despite all their differences of opinion seems to have been due to two factors. First, De Morgan was ultimately sympathetic to certain viewpoints standing at the heart of Whewell's vision, save for the religious-conservative implications Whewell attached to them: a romantic *idealism* that held truth to be grounded in ideas, seen as products of the genius's mind, a *gradualism* that saw the human understanding of those ideas developing over time, and an advocacy of mathematics as a tool for training the mind to arrive at 'necessary truths' on the basis of clear and precise reasoning.[22] Second, Whewell seems not only to have appreciated De Morgan for his obvious talents, but also to simply have liked him for his wit and humour, which De Morgan felt comfortable enough to let flow freely in his letters to Whewell. This comes out especially strongly when De Morgan's letters are compared with Whewell's correspondence with someone like Robert Leslie Ellis, another former pupil whose vision was much more Whewellian than De Morgan's but who never achieved a similar kind of intimacy.[23] Ellis always closed his letters with a 'your humble servant'; for De Morgan, his initial and surname sufficed.

It is possible to identify some more direct and specific mutual influences between Whewell and De Morgan. However, it is important to recognise from the outset that these should neither be overstretched nor assumed to have been premised on or resulted in any sweeping

21 Isaac Todhunter, *William Whewell D.D., Master of Trinity College, Cambridge. An Account of His Writings. With Selections from His Literary and Scientific Correspondence*, vol. 1 (London: Macmillan, 1876), p. 60.

22 See, for example, Augustus De Morgan to William Whewell, 30 April 1844, TCC, Add.Ms.a.202/100, and Augustus De Morgan to Robert Leslie Ellis, 24 June 1854, TCC, Add.Ms.c.67/111.

23 See Lukas M. Verburgt, ed., *A Prodigy of Universal Genius: Robert Leslie Ellis, 1817–1859* (New York: Springer, 2022), Part II ('Letters').

agreement. As in the case of the 'Cambridge Network', 'Breakfast Club' or 'Analytical Society', of which Whewell and De Morgan are sometimes said to have been like-minded members, the underlying differences matter more than the apparent similarities.[24] One way to start bringing these out is to unearth their clash over (anti-)Baconianism in the history and philosophy of science. Like a nineteenth-century Aristotle and Galileo, to use a good old Kuhnian phrase, Whewell and De Morgan could look at the same thing and see something entirely different. Rather than a pendulum, in their case this became most apparent when they were looking at that thing called 'the scientific method'.

De Morgan, Whewell and Nineteenth-Century British Logic

The history of scientific method in nineteenth-century Britain begins with Richard Whately's widely popular *Elements of Logic*, first published as a book in 1826 and appearing in many reprint editions throughout the nineteenth and early twentieth centuries.[25] By the early nineteenth century, the study of formal (i.e. deductive, Aristotelian or syllogistic)[26] logic in Britain had endured, in the words of Sir William Hamilton, a century and a half of 'perversion and neglect'.[27] Its decline had been

24 This is one of the important takeaways from William J. Ashworth's *The Trinity Circle: Anxiety, Intelligence and Knowledge Creation in Nineteenth-Century England* (Pittsburgh: University of Pittsburgh Press, 2021). On the 'Cambridge Network', 'Breakfast Club' and 'Analytical Society' see, respectively, W.F. Cannon, 'Scientists and Broad Churchmen: An Early Victorian Intellectual Network', *Journal of British Studies*, 4:1 (1964), 65–88; Laura J. Snyder, *The Philosophical Breakfast Club: Four Remarkable Friends Who Transformed Science and Changed the World* (New York: Broadway Books, 2011); P.C. Enros, 'The Analytical Society (1812–13): Precursor of the Renewal of Cambridge Mathematics', *Historia Mathematica*, 10:1 (1983), 24–47.

25 On Whately's *Elements of Logic* see James Van Evra, 'Richard Whately and Logical Theory', in *Handbook of the History of Logic. Volume 4: British Logic in the Nineteenth Century*, ed. by Dov M. Gabbay and John Woods (Amsterdam: North-Holland, 2008), pp. 75–92, and Calvin Jongsma, 'Richard Whately's Revitalization of Syllogistic Logic', in *Aristotle's Syllogism and the Creation of Modern Logic: Between Tradition and Innovation, 1820–1930*, ed. by Lukas M. Verburgt and Matteo Cosci (London: Bloomsbury Academic, 2023).

26 For Whately, deductive reasoning meant syllogistic reasoning only. Hence, in his *Elements of Logic*, logic is synonymous with Aristotle's syllogism.

27 Sir William Hamilton, 'IV. – Logic. In Reference to the Recent English Treatises on that Science', in Sir William Hamilton, *Discussions on Philosophy and Literature,*

due to a complex combination of factors, but a key role was played by Francis Bacon's *The Great Instauration*, alongside John Locke's *An Essay on Human Understanding*.[28] The second part of Bacon's six-part programme, the *Novum Organum*, which took its title from Aristotle's work on logic, the 'Organon', argued that the cornerstone of traditional deductive logic—the Aristotelian syllogism—was useless for the pursuit of natural knowledge. Instead, the *Novum Organum* introduced a system of inductive reasoning to supersede Aristotle's, suitable for the modern age of the 'sciences of nature'. Where Aristotle's old system, based on syllogisms, derived conclusions which were logically consistent with an argument's premises, Bacon's new system investigated the fundamental premises themselves on the basis of inductive inference from the data ('natural histories') of the natural world. Following Locke and Bacon, writing in the seventeenth century, eighteenth-century Scottish Common Sense philosophers like Thomas Reid and Dugald Stewart ridiculed syllogistic logic, finding in Newton's *Principia Mathematica* an exemplar of sound inductive reasoning.[29]

Whately's *Elements of Logic* was successful in reviving the study of deductive logic not because of its positive definition of what it is, but primarily because of its negative description of what it is *not*. The clear and accessible way in which Whately drew logic's boundaries provided him with solid ground on which to argue that the seventeenth- and eighteenth-century objections all resulted from a failure to recognise logic's nature and scope. In brief, 'by representing Logic as furnishing the sole instrument for the *discovery of truth* in all subjects, and as teaching the use of the *intellectual faculties* in general', Bacon, Locke, and the Common Sense philosophers had 'raised expectations which

Education and University Reform. Chiefly from the Edinburgh Review; Corrected, Vindicated, Enlarged, in Notes and Appendices. 2nd edn (London: Longman, Brown, Green & Longmans, 1853), pp. 118–175 (p. 119).

28 For chapters of this history see, for instance, the essays in Marco Sgarbi and Matteo Cosci, eds *The Aftermath of Syllogism: Aristotelian Logical Argument from Avicenna to Hegel* (London: Bloomsbury Academic, 2018).

29 See, in this regard, Larry Laudan, 'Thomas Reid and the Newtonian Turn of British Methodological Thought', in Robert E. Butts and John W. Davis, *The Methodological Heritage of Newton* (Toronto: University of Toronto Press, 2016), pp. 103–131, and Richard S. Olson, *Scottish Philosophy and British Physics, 1740–1870* (Princeton: Princeton University Press, 1975), especially Chapters 9 and 10.

could not be realized'.[30] Consequently, not only did deductive logic come to be regarded as 'utterly futile and empty'; sight was also lost of the 'boundless field' of unexplored territory within logic's 'legitimate limits'.[31] Rather than actually exploring it, Whately took upon himself the task of mapping this territory—that is, 'of completing and properly filling up the masterly sketch' made of it by Aristotle some two thousand years ago.[32] On Whately's definition of logic as both a 'science' and an 'art', deductive logic is not just *a* method of reasoning, but *the* method of analysing the mental process involved in all correct reasoning ('science'); similarly, the syllogism is not just *an* argumentative form, but *the* form to which all correct reasoning may be reduced and which thus serves the purpose of a test to try the validity of any argument ('art'). Moreover, for Whately logic was concerned, rather narrowly, with the process of reasoning, and not with the subject matter reasoned about. This meant that the *Elements of Logic* excluded as 'extra-logical' topics like concepts and judgments, and as 'non-logical' alleged other forms of reasoning, whether it was non-syllogistic deductive or inductive reasoning.

According to Whately, induction referred to two distinct activities: the process of collecting facts so as to obtain or evaluate premises for reasoning, and the process of inferring conclusions from those facts.[33] The first activity, however useful, is not a form of reasoning at all, and thus not within the scope of logic. And as a process of inference, Whately argued contra Aristotle and Bacon, induction is simply a so-called enthymematic deduction—a syllogism with the major premise suppressed. Hence, Whately, enthusiastic as he was to defend deductive logic, went so far as to claim that deductive logic was entirely independent from induction—i.e. that all reasoning is syllogistic—and to deny that induction is a uniquely legitimate form of inference at all, let alone a logic all in itself. This controversial view was expressed famously by John Stuart Mill in his 1828 review of Whately's *Elements of Logic* in the *Westminster Review*: '[T]o *reason by induction* is a recommendation which

30 Richard Whately, *Elements of Logic*. 9th edn (London: J. Mawman, 1848), p. x.
31 Whately, *Elements of Logic*, p. x; Richard Whately, *Elements of Logic*. 2nd edn (London: J. Mawman, 1827), p. 7. Tellingly, the word 'boundless' appearing in the first and second edition was changed into 'extensive' in later editions.
32 Whately, *Elements of Logic*, p. 7.
33 See Whately, *Elements of Logic*, Book IV, Chapter 1.

implies as thorough a misconception of the meaning of the two words, as if the advice were, to *observe by syllogism*.'[34]

Whately's defence of deduction at the expense of induction did not merely inspire some logicians, like Hamilton and De Morgan, to advance deductive logic. It also motivated others, such as Herschel, Whewell and Mill, to show that an inductive logic *was* possible. This led to the emergence of two opposing camps within British logic—the deductive ('formal') and inductive ('scientific')—which were not on speaking terms because they rather literally spoke different languages. What John Venn wrote about logic in the 1870s also applied to the situation in the 1830s-60s:

> It would not be going too far to say that the principal difficulty in the way of a student of Logic at the present day (at any rate in England) consists not so much in the fact that the chief writers upon the subject contradict one another ..., for an opportunity of contradiction implies agreement up to a certain stage, as in the fact that over a large region they really hardly get fairly within reach of one another at all.[35]

Importantly, those belonging to the inductive camp, like Whewell, all carried out their projects in terms of a renovation of Bacon's *Novum Organum*. This meant that British meta-scientific debates on methodology in the first half of the nineteenth century were conducted on the ('anti-Whatelyian') premise that induction was *the* form of scientific reasoning. As a result, the work of those belonging to the deductive camp, like De Morgan, was considered anti-Baconian not so much because it explicitly ridiculed Bacon—Baconians often did that

34 [John Stuart Mill], 'Review of Whately's *Elements of Logic*', in J.M. Robson, *The Collected Works of John Stuart Mill. Volume XI: Essays on Philosophy and the Classics* (Toronto: Toronto University Press, 1978), pp. 3–35 (p. 15). Mill's *System of Logic* famously turned Whately's view on its head, arguing that all deductive reasoning is grounded on induction. See, in this regard, Geoffrey Scarre, *Logic and Reality in the Philosophy of John Stuart Mill* (Dordrecht and Boston: Kluwer Academic Publishers, 1989), Chapters 1-3.

35 John Venn, 'Consistency and Real Inference', *Mind*, 1.1 (1876), 43–52 (p. 43). For a recent and more general discussion of logic in the nineteenth century see Jeremy Heis, 'Attempts to Rethink Logic', in *The Cambridge History of Philosophy in the Nineteenth Century (1790–1870)*, ed. by A.W. Wood and S.S. Hahn (Cambridge: Cambridge University Press, 2012), pp. 95–132.

too, with Whewell even sounding anti-Baconian to De Morgan's ears.[36] Rather, it was deemed anti-Baconian insofar as it was at odds with the conditions on which the search for science's methodology was carried out by Baconians. This becomes clear from the De Morgan-Whewell exchange, and arguably provides a clue as to why De Morgan's ideas on scientific methodology were largely neglected, both in his own time and by historians of Victorian science.

Whewell's Baconianism

It is well known that from his days as an undergraduate at Trinity in the 1810s to his final years as Master of that college in the 1860s, Whewell considered his project to be the reform of Bacon's inductive philosophy, which was to provide the groundwork for the reshaping of science, morality, politics and economics.[37] The task of reforming induction, which Whewell at times called the 'true faith', consisted roughly of two parts. The first was *defining* a 'true idea of induction', a philosophical task which Whewell himself took up; the second was that of *propagating* it as widely as possible through examples from specific sciences, 'to get *the people* into a right way of thinking about induction', for which Whewell solicited the help of others from his circle, such as Richard Jones for political economy and Robert Leslie Ellis for probability theory.[38]

One all-important part of this mission was to battle against those 'downwards mad'[39] who preferred a deductive approach to the sciences, that is, who held it possible to obtain natural knowledge through deductive reasoning. Aristotle himself had already been too 'fascinated & misled by the demonstrating powers of his syllogistic'.[40]

36 For instance, in his 1860 review in *The Athenæum* of Whewell's *On the Philosophy of Discovery*, De Morgan wrote: 'We cannot afford space to illustrate the way in which Dr. Whewell has reinforced our [negative] opinions on the history of Francis Bacon's philosophy'. – Augustus De Morgan, '*The Philosophy of Discovery, Chapters Historical and Philosophical*. By W. Whewell', *The Athenæum*, 1694, 14 April 1860, pp. 501–03 (p. 502).

37 See Snyder, *Reforming Philosophy*, chapter 1.

38 Notebook dated 28 June 1830, TCC, Whewell Papers, R.18.17/12, pp. v–ix.

39 See William Whewell to Richard Jones, 20 January 1833, TCC, Whewell Papers, Add.Ms.c.51/149, and William Whewell to Richard Jones, 22 July 1831, Whewell Papers, TCC, Add.Ms.c.51/110.

40 Richard Jones to William Whewell, 2 March 1831, TCC, Whewell Papers, Add. Ms.c.52/23.

But the most prominent of the 'deductive savages'[41] was undoubtedly Whately – who, as Whewell remarked at one point, was even worse than Aristotle because he was 'far more immersed in verbal trifling'.[42] Early in 1831, Whewell's close friend and collaborator Richard Jones wrote to Whewell after seeing the third edition of the *Elements of Logic*, ridiculing Whately's 'strange notion' that induction was a type of deductive reasoning and dismissing it.[43] Jones considered it yet 'another foolish sneer at those who think that inductive reasoning can ever be reduced to scientific form'.[44] Moreover, in following David Ricardo's theory of political economy, Whately and his fellow 'Oriel Noetics' at Oxford were the ones who were 'overrating [deduction's] pretentions', not someone like Bacon when he passed judgment on Aristotle.[45]

Given the meta-scientific context of the 1830s, Whewell and Jones saw Whately's characterisation of induction as much more than just a technical point of logic. First of all, they worried that if people accepted Whately's view, they might be led to the erroneous conclusion that the sciences—as the Oriel Noetics claimed—are essentially deductive and concerned with deducing conclusions from axioms and principles. Second, and more importantly, they were convinced that this deductive mode of thinking entailed dangerous moral and religious attitudes. Whewell's first reference in print to Whately's work appeared in his widely read Bridgewater treatise of 1833, *Astronomy and General Physics considered With Reference to Natural Theology*. Here, he influentially divided the (meta-)scientific community into two kinds of thinkers, with the deductive type (or 'mere Mathematicians')—the majority—possessing

41 William Whewell to Richard Jones, 19 February 1832, TCC, Whewell Papers, Add. Ms.c.51/129.

42 William Whewell to Richard Jones, 7 April 1843, TCC, Whewell Papers, Add. Ms.c.51/227.

43 Richard Jones to William Whewell, 24 February 1831, TCC, Whewell Papers, Add. Ms.c.52/20.

44 Richard Jones to William Whewell, 24 February 1831, TCC, Whewell Papers, Add. Ms.c.52/20.

45 Richard Jones to William Whewell, 2 March 1831, TCC, Whewell Papers, Add. Ms.c.52/23. On Jones's Baconian views on political economy and Whewell's and Jones's opposition to the 'Oriel Noetics' see, for example, Harro Maas, '"A Hard Battle to Fight": Natural Theology and the Dismal Science, 1820–50', *History of Political Economy*, 40:5 (2008), 143–167, and Paul Oslington, 'Natural Theology, Theodicy, and Political Economy in Nineteenth-century Britain: William Whewell's Struggle', *History of Political Economy*, 49:4 (2017), 575–606.

mental habits that 'impoverished their religious feeling' and their 'ability to appreciate moral evidence', and the inductive type (or 'Discoverers')—an elite group—displaying these virtues.[46]

Whewell illustrated this difference by using Whately's *Elements of Logic* for his own purposes, remarking that 'all which mathematics or logic can do, is to develop and extract those truths, as conclusions, which were in reality involved in the principles on which our reasoning proceeded'.[47] The implication was not just that new knowledge could only be attained on the basis of induction—or, more precisely, that there was a strong distinction between the original discovery of laws of nature by 'Discoverers' and the explication of their consequences and applications by 'mere Mathematicians'. Whewell also deemed the laborious and humbling process of ascending from observation to general principles to be simply more virtuous than the formal and dispassionate work of mathematicians. Euler, Laplace and Lagrange, in dealing with higher-level generalisations (e.g. laws of motion and gravitation), treated these as self-evident. They did not realise that, in discovering these laws, Newton had embarked on a pilgrimage and, hence, were unable to appreciate the moral and spiritual aspects of the proper pursuit of science.[48] For Whewell, the worst of the 'downwards mad' was not Whately but men like Laplace and his British followers, such as Babbage and, quite possibly, De Morgan: not only did they link mathematical deduction to scientific discovery, they also sought to

46 Yeo, *Defining Science*, p. 123. See William Whewell, *Astronomy and General Physics, Considered with Reference to Natural Theology* (London: William Pickering, 1833), Book III, Chapters 5–6. The phrases 'Mere Mathematicians' and 'Discoverers' appear in Hugh James Rose to William Whewell, 27 March 1833, Whewell Papers, TCC, Add.Ms.a.211 /143. On Whewell's inductive-deductive distinction see Joan L. Richards, 'The Probable and the Possible in Early Victorian England', in *Victorian Science in Context*, ed. by Bernard Lightman (Chicago: The University of Chicago Press, 1997), pp. 51–71, especially pp. 57–62.

47 Whewell, *Astronomy and General Physics*, pp. 335–36. Whewell gave the following quote from Whately's *Elements of Logic* in a footnote: 'Since all reasoning may be resolved into syllogisms, and since in a syllogism the premises do virtually assert the conclusions, it follows at once, that no new truth can be elicited by any process of reasoning.' – Whately, *Elements of Logic*, p. 215.

48 Secord has argued that Herschel's *Preliminary Discourse* was read not as a contribution to abstract philosophy but as a 'conduct manual'. See James A. Secord, *Visions of Science: Books and Readers at the Dawn of the Victorian Age* (Oxford: Oxford University Press, 2014), p. 81.

speed up this process through 'mental labor-saving techniques' which increased the 'accessibility of science' and facilitated its progress.[49]

Whewell's argument in his Bridgewater treatise was extraordinary for turning something as dry as induction and deduction into an epoch-making watershed. What it achieved was that promoting inductive or deductive reasoning, or even expressing a view on their relationship, was no longer just a theoretical matter. Instead, to work on deductive logic also meant to implicitly position oneself on much broader meta-scientific themes. Thereby, scientific method effectively became a topic reserved for those who believed in the possibility of an inductive logic. It is indeed telling, in this regard, that none of the British meta-scientists involved in debates on scientific method ever wrote on, or took an active interest in, developments in deductive logic. Despite their rejection of Whately's outlook, both Whewell and Mill were happy to concede deductive logic to Whately, who in turn conceded it pretty much to Aristotle. Rather than deduction *per se*—which for Whewell stood to induction as mathematics to scientific discovery—it was the deductive habit of mechanical formalisation that was fundamentally at odds with Whewell's project of renovating Bacon's inductive philosophy.

At the heart of this project stood Whewell's so-called antithetic epistemology. This said that all human knowledge is obtained through induction and demands the combination of ideas ('ideal') and facts ('empirical'). These ideas, which he called 'Fundamental Ideas' (Space, Time, Cause, etc.), are actively supplied by the human mind itself and not passively received from observations of the world. At the same time, these ideas make it possible to have scientific knowledge of the world outside the mind insofar as they make experience possible by allowing us to give form to our sensations. Because Whewell's Fundamental Ideas closely resembled Kant's forms of intuition and categories, as discussed in the *Critique of Pure Reason*, Whewell was criticised by his contemporaries for trying to import Kant into British philosophy.[50]

49 William J. Ashworth, 'Memory, Efficiency, and Symbolic Analysis. Charles Babbage, John Herschel, and the Industrial Mind', *Isis*, 87:4 (1996), 629–53 (p. 629); Michael Shortland and Richard Yeo, 'Introduction', in *Telling Lives in Science: Essays on Scientific Biography*, ed. by Michael Shortland and Richard Yeo (Cambridge: Cambridge University Press, 1996), pp. 1–44 (p. 20).

50 On the Whewell-Kant connection see, for example, Steffen Ducheyne, 'Kant and Whewell on Bridging Principles Between Metaphysics and Science', *Kant-Studien*,

De Morgan, in his 1840 review of the *Philosophy*, expressed surprise that 'the doctrines of Kant and Transcendental Philosophy are now promulgated in the university which educated Locke'.[51] But Kant was ultimately a metaphysician and Whewell a philosopher of science. Many of Whewell's Fundamental Ideas did not function as conditions of experience but as conditions for having knowledge within specific sciences; although it is possible to experience the world without having the Idea of Chemical Affinity, it is impossible to have knowledge of certain chemical processes without this Idea. Moreover, unlike Kant, Whewell believed that Fundamental Ideas (as well as the 'conceptions' included within them, such as 'force' as a modification of the Idea of Cause) emerged over the course of the development of science. 'The Ideas', he wrote, 'were in the human mind before [experience]; but by the progress of scientific thought they are unfolded into clearness and distinctness.'[52]

On the basis of this philosophical outlook, Whewell developed his inductive scientific methodology, dubbed 'Discoverers' Induction' in a letter to De Morgan from January 1859.[53] It was Baconian in a twofold sense. First, it agreed with what Bacon had said about induction, primarily that induction involved more than simple enumeration of instances, i.e. that it is something else than drawing a general proposition from particular cases. Second, it improved upon Bacon's method on the understanding that Bacon had never completed it and that if he had done so he would have paid more attention to the 'ideal' side of knowledge. At the core of Whewell's account stood the view that, in induction, 'there is a New Element added to the combination [of particular instances or cases] by the very act of thought by which they [are] combined'.[54] This 'act of thought' is a process which Whewell called 'colligation', the mental operation of bringing together a number

102.1 (2012), 22–45.

51 Augustus De Morgan, '*The Philosophy of the Inductive Sciences*. By W. Whewell', *The Athenæum*, 672, 12 September 1840, pp. 707–09 (p. 707). (Herschel, for one, was much harsher in his judgment about Whewell's 'a priorism'.) Whewell replied to De Morgan's anonymous review in a privately printed pamphlet, explaining the novelty of his approach as compared to Kant. See Yeo, *Defining Science*, p. 13.

52 William Whewell, *On the Philosophy of Discovery: Chapters Historical and Critical* (London: John W. Parker, 1860), p. 373.

53 William Whewell to Augustus De Morgan, 18 January 1859, TCC, 0.15.47/25.

54 Whewell, *Philosophy*, II, p. 213.

of facts by 'superinducing' upon them 'a conception of the mind ... which did not exist in any of the observed facts'.[55] Or, in more traditional logical terms:

> It has been usual to say of any general truths, established by the consideration and comparison of several facts, that they are obtained by *Induction*; but the distinctive character of this process has not been well pointed out .... The *Logic of Induction* has not yet been constructed. ... In each inductive process, there is some general idea introduced, which is given, not by the phenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a new generality.[56]

According to Whewell, this is what happened in all scientific discoveries, as the cases of Kepler and Newton showed. What made them great scientists was not their unearthing of new facts, nor their mathematical calculations; it was their explicating of new conceptions needed to colligate these facts into general laws.[57] But how did they arrive at these conceptions? Whewell offered several suggestions, each of which revolved around the decidedly non-Baconian notion of 'sagacity' or 'inventive genius':

> The necessity of a *conception* which must be furnished by the mind ... could hardly have escaped the eye of Bacon, if he had cultivated more carefully the ideal side of his own philosophy. And any attempts which he could have made to construct such conceptions *by mere rule and method*, must have ended in convincing him that *nothing but a peculiar inventive talent* could supply that which was ... contained in the facts, and yet was needed for the discovery.[58]

55 Whewell, *Philosophy*, II, p. 213.

56 William Whewell, 'Remarks on Mathematical Reasoning and on the Logic of Induction', in *The Mechanical Euclid*, 3rd edn (London: J. W. Parker, 1838), pp. 147–87 (pp. 177–78). This passage was reproduced verbatim in Whewell's *Philosophy*.

57 This two-step process is described in Book XI ('Of the Construction of Science') of Whewell's *Philosophy*.

58 Whewell, *Philosophy*, II, p. 402, my emphases. For the uses of 'genius' in nineteenth-century British science and philosophy see Simon Schaffer, 'Genius in Romantic Natural Philosophy', in *Romanticism and the Sciences*, ed. by Andrew Cunningham and Nicholas Jardine (Cambridge: Cambridge University Press, 1990), pp. 82–98, and Richard Yeo, 'Genius, Method and Morality'.

Whewell's point was not just that Bacon had failed to appreciate the 'inventive genius' which all scientific discovery requires; it was that Bacon had mistakenly believed that it was possible to 'supersede' genius by reducing its activities to a '*Technical Form*'.[59] At the same time, Whewell himself insisted that there is nothing 'accidental' about scientific discoveries, and he explicitly opposed David Brewster's competing view that most discoveries are the result of 'pure accident'.[60] The resulting tension, brought out by De Morgan, may be called 'Whewell's paradox': because sparks of creative genius are irreducible to methodological rules, the logic of induction is ultimately not completely logical.

There was no way to solve this paradox, and the best Whewell could offer were suggestions for dissolving it. Rather than giving rules to men of genius, rules might be given for the use they made of their genius. One mark of genius was a certain facility in generating a number of possible options for the appropriate conception. Because this process is not bound to rules, Whewell sometimes used the terms 'guessing' or 'conjecturing' to describe it. Whewell, however, was not the hypothetico-deductivist that some latter-day commentators made of him.[61] Since the selection and application of the appropriate conception often involved a series of different kinds of inferences (especially analogical reasoning), as Whewell argued, this stage of inductive discovery was not a matter of non-rational guesswork. The same obviously held for the next stage, where conceptions—in the form of hypotheses or theories—are confirmed on the basis of several tests, namely prediction, consilience and coherence. But it was undeniable that Whewell, in renovating Bacon, had stretched Baconian inductive logic to its utmost limits: it was now a matter of discoverers having '*good* metaphysics in their heads' and 'binding their metaphysics' to the facts through a process that was rule-governed only to a certain degree.[62]

59 Whewell, *Philosophy*, II, p. 402.

60 See David Brewster, 'On the History of the Inductive Sciences', *Edinburgh Review*, 66 (1837), 110–51 (p. 121).

61 For a critical discussion of twentieth-century readings of Whewell as a hypothetico-deductivist see, for instance, Laura J. Snyder, '"The Whole Box of Tools": William Whewell and the Logic of Induction', in *Handbook of the History of Logic. Volume 4: British Logic in the Nineteenth Century*, ed. by Dov M. Gabbay and John Woods (Amsterdam: North-Holland, 2008), pp. 163–228.

62 William Whewell, *Novum Organon Renovatum*, p. vii.

Some of the meta-scientific implications of Whewell's views on scientific method were equally at odds with Bacon's programme. Perhaps most tellingly, in placing limits on the 'formalisation' of methodology Whewell not only denied that discovery was a mechanical process, but he also undermined the idea that it should be possible at least in principle for anyone who carefully follows the scientific method to achieve scientific breakthroughs.[63] Herschel saw in this a useful corrective to the tendency of recent utilitarian reforms to promote the accessibility of science by ascribing its progress wholly to correct method.[64] It made others wonder what it was that made Whewell still identify as a Baconian. If 'the great Baconian induction' was 'a complete failure', De Morgan wondered, why try to save his programme rather than finally abandoning it for an alternative?[65]

De Morgan's Anti-Baconianism

Unlike Whewell's, De Morgan's oeuvre and career was not an unfolding of a meta-scientific plan cooked up as an undergraduate and self-consciously carried out as the years passed. Neither is it possible, at least not as strongly as in the case of Whewell, to read every single publication of De Morgan, who published even more than Whewell, as a contribution to such a plan. Nonetheless, there is arguably a common thread running throughout De Morgan's wide-ranging writings—books, encyclopedia entries, and reviews—on logic, probability theory, and history of science in regard to scientific methodology. Moreover, when contrasting his views on scientific methodology with those of Whewell it becomes possible to approach De Morgan as a meta-scientist and to see him rebelling against the Baconianism that dominated British meta-science in the first half of the nineteenth century. This has a wider significance because it suggests that, however 'excessively Baconian',

63 See Simon Schaffer, 'Scientific Discoveries and the End of Natural Philosophy', *Social Studies of Science*, 16.3 (1986), 387–420.

64 See [John W.F. Herschel], 'Review of the *History and Philosophy of the Inductive Sciences*', *Quarterly Review*, 135 (June 1841), 96–130.

65 Augustus De Morgan, '*The Philosophy of Discovery*', p. 503.

the 'methodological orthodoxy' in the early Victorian period did not go unchallenged.[66]

De Morgan's views on scientific methodology were anti-Baconian in a twofold sense. First, De Morgan dismissed the historical significance and philosophical correctness of Bacon's methodology, as put forward in the *Novum Organum*. Second, his views conflicted with the Baconianism of Bacon's nineteenth-century heirs. This Baconianism rested on a specific, limited interpretation of the Baconian philosophical corpus, fitted to their meta-scientific agendas. Indeed, in at least one crucial respect De Morgan remained more loyal to Bacon than a Whewell or a Herschel; he continued the search, albeit in a decidedly non-Baconian fashion, for a way to put scientific methodology into a 'Technical Form', to provide a 'machinery' for arriving at natural knowledge. At the core of his anti-Baconianism stood the conviction that Bacon and the Baconians focused too much on observation and too little on logic and mathematics as instruments of scientific discovery. Newton may have been careful at observation, having 'few superiors' in the 'inductive process', but 'it was his power of deduction which made him what he was'.[67] What De Morgan wrote about Bacon in his 1858 review of *The Works of Francis Bacon* also applied to Whewell and other Baconians:

> He averred that logic and mathematics should be the handmaids, not the mistresses, of philosophy. He meant that they should play a subordinate and subsequent part in the dressing of the vast mass of facts by which discovery was to be rendered equally accessible to Newton and to us. Bacon himself was very ignorant of all that had been done by mathematics; and, strange to say, he especially objected to astronomy being handed over to the mathematicians. Leverrier and Adams, calculating an unknown planet into visible existence by enormous heaps of algebra, furnish the last comment of note on this specimen of the goodness of Bacon's views.[68]

66 Charles Gillispie, *The Edge of Objectivity* (Princeton: Princeton University Press, 1960), p. 314; Richard Yeo, 'An Idol of the Market-place: Baconianism in Nineteenth Century Britain', *History of Science*, 23.3 (1985), 251–98 (p. 252).

67 Augustus De Morgan, '*History of the Inductive Sciences from the Earliest to the Present Times*. By W. Whewell', *The Athenæum*, 541, 10 March 1838, pp. 179–81 (p. 180).

68 Augustus De Morgan, '*The Works of Francis Bacon*, ed. by James Spedding, R. Leslie Ellis, and Douglas D. Heath. 5 vols.', *The Athenæum*, 1612, 18 September 1858, pp. 367–68 (p. 367).

These and other historical facts should be philosophically accounted for in scientific method. Doing so meant that Baconianism had to be abandoned, and that something else had to come in its place. Baconians like Whewell were quick to suspect a blatant case of 'downwards' thinking of the worst, Continental kind. But also, for them it was far from clear what De Morgan's vison on science exactly amounted to, let alone how it translated into an alternative scientific methodology or what its wider meta-scientific ramifications were. This is still very much an open question.[69]

De Morgan recognised the inadequacy of Bacon's inductive philosophy as well as the need for an alternative which could overcome its deficiencies. Unlike any of the Baconians, De Morgan was willing to break with British tradition and pursue this search in defiance of even ceremonial Baconianism. Instead, De Morgan thought about the history and philosophy of science in terms not of 'Bacon's rules' but 'Newton's practice'. What does this mean? First, that De Morgan denied that—historically speaking—Newton, in writing the *Principia*, had followed Bacon's inductive canons. Second, that—philosophically speaking—there are no rules for arriving at discoveries, such as that of universal gravitation, and scientific method should not aim to provide them.[70] Taken together: 'If Newton had taken Bacon for his master, not he, but somebody else, would have been Newton.'[71] The same can be put in positive terms. First, De Morgan believed that, despite his own famous 'Hypotheses *non* fingo', Newton had employed hypotheses and this convinced De Morgan that scientific knowledge progressed through

69 The present chapter contributes to taking a first step toward addressing this question. Among the other sources crucial in taking this step are: Laudan, 'Induction and Probability'; Maria Panteki, 'French "Logique" and British "Logic"', especially pp. 400–11 and pp. 423–41; Adrian Rice, 'Augustus De Morgan: Historian of Science', *History of Science*, 34:2 (1996), 201–40; Joan L. Richards, '"In a Rational World all Radicals would be Exterminated": Mathematics, Logic and Secular Thinking in Augustus De Morgan's England', *Science in Context*, 15:1 (2002), 137–64; John V. Strong, 'The Infinite Ballot Box of Nature: De Morgan, Boole, and Jevons on Probability and the Logic of Induction', *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, 1976:1 (1976), 197–211; John Wettersten, *Whewell's Critics: Have They Prevented Him from Doing Good?* (Amsterdam and New York: Rodopi, 2005), Chapter 1 ('Immediate Rejection'); and Richard Yeo, 'Genius, Method and Morality'.

70 See also the section on De Morgan's philosophy of mathematics in Chapter 1 of this volume.

71 De Morgan, '*The Works of Francis Bacon*', p. 367.

deduction, especially mathematical reasoning. Second, De Morgan believed that a new scientific methodology should assist scientists in their practice of hypothesising. This brought him closer to Herschel than Whewell, who had accused Herschel of promoting a spirit of 'gratuitous theorising' in his *Preliminary Discourse* by not cautioning against anticipatory leaps to hypotheses.[72] Whewell and Herschel both made room for hypotheses in scientific methodology, but Herschel adopted a much more flexible stance toward hypothesising.[73] De Morgan's liberality, in this regard, went much farther even than Herschel's, however, as he shunned the principle that hypothetical speculation is only legitimate on inductive grounds.

Interestingly, De Morgan's next step was indebted to Herschel: he turned to the mathematical theory of probability to provide a criterion for choice between scientific hypotheses. De Morgan may have been the one to have imported this theory from the Continent into Britain; it was Herschel who, in a neglected passage in his *Preliminary Discourse*, introduced this 'refined and curious branch of mathematical enquiry'[74] into the British debate on scientific methodology. But Herschel only discussed it in relation to the calculation of observational errors. De Morgan took the bold and pioneering step—in the British context at least—of using probability theory to formalise and justify scientific inference, in the sense of weighing competing hypotheses offered to account for a given set of phenomena. This was anti-Baconian not just in the obvious sense of answering a philosophical question with mathematics. It also went against Baconian orthodoxy in two other, more profound and complexly related, ways—thereby unearthing what this very orthodoxy was. On the one hand, it questioned the idea of an inductive methodology that would necessarily lead to infallible

72 [William Whewell], 'Modern Science – Inductive Philosophy [Review of John F.W. Herschel's *A Preliminary Discourse on the Study of Natural Philosophy*]', *Quarterly Review*, 45 (July 1831), 374–407 (p. 400).

73 For a useful overview of different views on Whewell's and Herschel's views on hypotheses see Aaron D. Cobb, 'Is John F.W. Herschel an Inductivist about Hypothetical Inquiry?' *Perspectives on Science*, 20:4 (2012), 409–39; and Laura J. Snyder, 'Hypotheses in 19th Century British Philosophy of Science: Herschel, Whewell, Mill', in *The Significance of the Hypothetical in Natural Science*, ed. by Michael Heidelberger and Gregor Schiemann (Berlin: Walter de Gruyter, 2009), pp. 59–76.

74 Herschel, *Preliminary Discourse*, p. 217.

scientific knowledge. Firstly, because scientists are creative thinkers, not simply rule-followers; secondly, because induction can never prove the truth of a conclusion; and, finally, because all scientific knowledge is probable, not certain. On the other hand, rather than accepting creative genius as the ruleless core of an otherwise rule-bound methodology, it limited methodology to calculating the probability of the products (i.e. hypotheses) of someone's creativity.

Taken together, De Morgan's anti-Baconianism made scientific methodology revolve around uncertainty, both by accepting its place at the heart of science and by seeking mathematical ways to deal with it as accurately as possible. This points to a beautiful paradox of the nineteenth-century British meta-scientific debate, which may be called 'the paradox of Hume's ghost': those who were the most skeptical about induction, like De Morgan, were also the ones to recognise and confront the limits of inductive inference.

De Morgan Contra Whewell

There are many routes into De Morgan's meta-scientific outlook on methodology—for example via his technical work on formal logic, his contributions to the history of modern science, his involvement in scientific organizations, and his influence on pupils such as Jevons. Any full-blown account will have to explore each of these routes and identify the relevant intersections between them. The modest aim here is to bring out a few more specific aspects of De Morgan's views on scientific methodology by focusing on his exchanges with Whewell, who is taken as a representative of the dominant Baconian orthodoxy. Their interaction took place mostly through letters, some hundred of which have survived, four reviews in *The Athenæum*,[75] and occasional

75 For *The Athenæum*, De Morgan (anonymously) reviewed Whewell's *History of the Inductive Sciences* (1838), *The Philosophy of the Inductive Sciences* (1840), *Novum Organon Renovatum* (1859) and *On the Philosophy of Discovery* (1860)—the latter two being respectively the second and third part of the third edition of *The Philosophy of the Inductive Sciences*. Another review in *The Athenæum* of Whewell's work that has been attributed to De Morgan is of *The Mathematical Works of Isaac Barrow* (1860), edited by Whewell for Trinity College, Cambridge. See, in this context, Sloan Evans Despeaux and Adrian C. Rice, 'Augustus De Morgan's Anonymous Reviews for *The Athenæum*: A Mirror of a Victorian Mathematician', *Historia Mathematica*, 43:2 (2016), 148–71.

references in book chapters. Given this focus, it is unavoidable that some aspects receive more attention than others and that there are aspects which do not come into view at all, such as probability theory. Another reason for this limitation is that the interaction between De Morgan and Whewell was relatively one-directional: for example, there are about four times more letters from De Morgan to Whewell than vice versa.[76] Moreover, De Morgan reviewed Whewell's work but the reverse never occurred. This is interesting insofar as it points to disciplinary boundaries in the field of meta-science, and suggests that in the context of methodological debates De Morgan was even more polymathic than Whewell: as a mathematician, De Morgan was well-versed in history and philosophy of science, but as a mathematician-turned-philosopher, Whewell was not (and did not want to be) expert on mathematical developments in logic.

De Morgan and Whewell on Logic and Induction

One point of conflict between De Morgan and Whewell concerned the nature and scope of logic, more specifically of induction.[77] Their disagreement on this topic surfaced in 1849, when De Morgan complained in a letter that Whewell's notion of induction contained 'more than logic'.[78] It became public in De Morgan's review, written at the request of Whewell himself,[79] of the *Novum Organon Renovatum* of January 1859. Here, De Morgan wrote that: 'though we do not quarrel with any of his [i.e. Whewell's] conclusions'—for example, that every scientific discovery introduces a new conception—'we are entirely opposed to the use which he makes of the words *logic* and *induction*',

76 This ratio is based on the Whewell-De Morgan correspondence held at Trinity College Library, Cambridge. For further information regarding this collection of letters, see Chapter 11 of this volume.

77 The following analysis draws on the following accounts: Wettersten, *Whewell's Critics*, pp. 58–60, and Robert E. Butts, ed., *William Whewell's Theory of Scientific Method* (Pittsburgh: University of Pittsburgh Press, 1968), pp. 24–26.

78 Augustus De Morgan to William Whewell, 20 April 1849, TCC, Whewell Papers, Add.Ms.a.202/114.

79 See William Whewell to Augustus De Morgan, 18 January 1859, TCC, Whewell Papers, O.15.47/25

especially when combined into a '*logic of induction*'.[80] First, De Morgan criticized Whewell's vague, non-formal understanding of 'logic'. What De Morgan, following Whately who, in turn, followed Aristotelian tradition, meant by logic was the study of the logical form of statements and inferences. 'It has nothing to do,' he wrote in his 1839 *First Notions of Logic*, 'with the truth of the facts ... from which an inference is derived; but simply takes care that the inference shall certainly be true, if the premises be true.'[81] On the one hand, by introducing into logic the process by which premises are formed, Whewell made logic '[take] in much which the word excludes'. On the other, by failing to provide a way of showing the validity of conceptions, which bind together facts through generalisation, Whewell made logic 'exclude much which the word takes in'. De Morgan's was an appeal to tradition: Whewell had no right to claim the word 'logic' for something not concerned with logical truth and formal validity. Second, De Morgan criticised Whewell's use of the term 'induction' for taking it beyond its traditional meaning. According to De Morgan, Whewell used it too liberally as including 'the use of the whole box of tools',[82] from the 'old' to the 'new', that is, from the generalisation of particulars to the formation and testing of the general notion under which these particulars are to be brought. Again, De Morgan did not find fault with Whewell's conclusions, but insisted that Whewell had no right to redefine a canonical term to make it suit his own purposes:

> Let induction mean, as it always has done, the generalization by collection of particulars: let the act of the discoverer, by which he divines the general notion under which the particulars can be brought, receive its own proper name. ... We put it to him [Whewell], whether it would not be desirable to restrict the words *logic* and *induction* to the meanings now well agreed upon, and to find better names for the whole process, and also for the particular part which entirely depends on the acumen of the discoverer.[83]

80 [Augustus De Morgan], Review of William Whewell's *Novum Organum Renovatum*, *The Athenæum*, 1628 (8 January 1859), 42–44 (p. 43). De Morgan quotes ('art of discovery ...') from Whewell, *Novum Organon Renovatum*, p. v.

81 Augustus De Morgan, *First Notions of Logic* (*Preparatory to the Study of Geometry*), 2nd edn (London: Taylor & Walton, 1840), p. 3.

82 Augustus De Morgan, *Formal Logic*, p. 216.

83 Augustus De Morgan, '*Novum Organum Renovatum*', p. 44.

De Morgan's position, which distinguished logic and induction from discovery, arguably reflected a clash of underlying outlooks. The following illustrations should suffice here. For Whewell, it was not a criticism at all that his 'logic of induction' did not belong to or sit well with the 'old logic', since it was premised precisely on a Baconian break with that very tradition. As he wrote to De Morgan in a letter from January 1859:

> My object was to analyse ... the method by which scientific discoveries have really been made; and I call this method *Induction*, because all the world seemed to have agreed to call it so, and because the name is not a bad name after all. That it is not exactly the Induction of Aristotle, I know; nor is it that described by Bacon .... I am disposed to call it *Discoverers' Induction* .... I do not wonder at your denying [it] a place in Logic; and you will think me heretical and profane, if I say, *so much the worse for Logic*.[84]

Similarly, De Morgan's argument that Whewell's notion of induction was not logical would not have shocked Whewell, as Whewell disagreed with De Morgan's logical notion of induction. What De Morgan understood by induction was 'Perfect Induction', which can only be done when dealing with a limited number of observed particulars. For example, Kepler discovered that Mars moves in an ellipse, that the earth moves in an ellipse, and so on, and from this he inferred that all the planets move in ellipses. For Whewell, there was no real inference involved here, since the conclusion contained nothing that was not already asserted in the premises. Whewell's discoverers' induction also covered what De Morgan called 'Imperfect Induction', namely the mental process, or '*mysterious step*',[85] of inferring from known to unknown cases. 'So much the worse for Logic' if it excluded this crucial element of human reasoning.

De Morgan and Whewell agreed that induction in the sense of mere summary generalisation from observed particulars played a negligible role in scientific discovery. For De Morgan, this meant that logic had nothing to do with the process of arriving at new knowledge of the world, and that discovery consisted in something else entirely—a 'third

84 William Whewell to Augustus De Morgan, 18 January 1859, TCC, Whewell Papers, O.15.47/25.

85 Whewell, *On the Philosophy of Discovery*, p. 284.

method', one 'not within the ken of Bacon', which revolved around the probability of hypotheses.[86] For Whewell, it meant that logic had to be broadened to include rules for both deductive (i.e. syllogistic) and inductive reasoning:

> By *Logic* has generally been meant a system which teaches us to arrange our reasonings that their truth or falsehood shall be evident in their form. In *deductive* reasonings ... the device [for this] is the *Syllogism* .... [The *Logic of Induction*] in like manner supplies the means of ascertaining the truth of our *inductive* inferences.[87]

Nevertheless, by 1860, Whewell does seem to have bitten the bullet of De Morgan's point that in scientific discovery there is more than what is traditionally called induction. '[T]he philosophy at which I aimed was not the philosophy of Induction, but the *Philosophy of Discovery*' and, as De Morgan was happy to observe in his review of *On the Philosophy of Discovery, Chapters Historical and Philosophical*, 'the title of the book is modified accordingly'.[88]

De Morgan and Whewell on Deduction and Probability

Another major point of conflict remained in place: Whewell's and De Morgan's positions vis-à-vis deductive logic. Like all Baconians, Whewell followed Whately in equating it with syllogism, which he regarded as a completed tool of very limited usefulness. Whewell did publish one ten-page article on Aristotelian logic, if only to attribute to Aristotle the misguided claim that induction *is* a syllogism.[89] De Morgan, instead, went over, under and beyond Whately, taking deductive logic far beyond the syllogism in terms of depth and scope.[90] Despite his

86 De Morgan, '*Novum Organum Renovatum*', p. 44. More on this topic below.

87 Whewell, *Novum Organon Renovatum*, p. 106; Augustus De Morgan, '*The Philosophy of Discovery, Chapters Historical and Critical.* By W. Whewell', *The Athenæum*, 1694, 14 April 1860, pp. 501–03 (p. 503).

88 Whewell, *On the Philosophy of Discovery*, p. v.

89 See William Whewell, 'Criticism of Aristotle's Account of Induction', *Transactions of the Cambridge Philosophical Society*, 10.1 (1850), 63–72. This largely forgotten paper was later published as an Appendix to Whewell's *Philosophy of Discovery* of 1860.

90 On De Morgan as a logical innovator see Daniel D. Merrill, *Augustus De Morgan and the Logic of Relations* (Dordrecht: Kluwer Academic Publishers, 1990) and Michael E. Hobart and Joan L. Richards, 'De Morgan's Logic', in *Handbook of the*

appeal to logical tradition in criticising Whewell, De Morgan was an innovator who obviously did not believe that the 'old logic' could not be improved.[91] Indeed, he did just that in major works such as *Formal Logic* (1847) and *Syllabus of a Proposed System of Logic* (1860), tellingly opening his entry on 'Logic' for the *English Cyclopaedia* with the statement that recent innovations suggested 'that Kant's dictum about the perfection of the Aristotelian logic may possibly be false'.[92] The point of his *ad antiquitatem* was that Whewell's 'logic of induction' could not be considered a contribution to logic in the traditional sense of a formal study of deductive reasoning. Among the innovations which De Morgan did consider legitimate contributions to logic were those that sought to improve this study without thereby breaking away from Aristotle's conception of logic. One example was his own logic of relations, of which he believed the syllogism to be a special case.

A key feature of De Morgan's logical work was the use of mathematics to remove the limitations of the syllogism for deductive logic. More important than this, at least with an eye to unearthing De Morgan's views on scientific method, is his controversial use of one specific branch of mathematics, namely probability theory, in his logical work.[93] 'Many will object to this theory as extralogical', De Morgan wrote:

> But I cannot see on what definition ... the exclusion of it can be maintained. ... I cannot understand why the study of the effect which partial belief of the premises produces with respect to the conclusion, should be separated from that of the consequences of supposing the former to be absolutely true.[94]

History of Logic. Volume 4: British Logic in the Nineteenth Century, ed. by Dov M. Gabbay and John Woods (Amsterdam: North-Holland, 2008), pp. 283–330.

91 See Chapter 2 of this volume.

92 Augustus De Morgan, 'Logic (1860)', in Peter Heath, ed., *On the Syllogism and Other Logical Writings by Augustus De Morgan* (London: Routledge & Kegan Paul, 1966), pp. 247–66 (p. 247).

93 The important works, in this context, are De Morgan's book-length article in the *Encyclopedia Metropolitana* (1837), the volume *An Essay on Probabilities* (1838) and several chapters in *Formal Logic* (1847). For an in-depth discussion of De Morgan's introduction of probability into logic, see Adrian Rice, '"Everybody Makes Errors": The Intersection of De Morgan's Logic and Probability, 1837–1847', *History and Philosophy of Logic*, 24:4 (2003), 289–305.

94 De Morgan, *Formal Logic*, p. v.

On the basis of his new system of the numerically definite syllogism, where all terms are quantified, De Morgan observed that, although in the Aristotelian syllogistic no inference can be drawn from 'Some *X*s are *Y*s' and 'Some *Y*s are *Z*s', the following inference is nonetheless valid: 'Some *X*s are *Y*s, some *Y*s are *Z*s, therefore, there is some probability that some *X*s are *Z*s.' It was here that De Morgan began to apply the techniques of mathematical probability theory to logic, for instance finding the probability that some *Y*s will be both *X*s and *Z*s, given that the distribution of *X*s and *Z*s among the *Y*s is unknown. The point of this endeavour was not to offer a full-blown theory of probable inference; instead, it was to illustrate that innovating deductive logic was not mere trifling—as Whewell believed—but could help model how people of flesh and blood could reason under conditions of uncertainty. More specifically, it suggested that it was possible to calculate what degree of rational belief someone should attach to a conclusion derived from pieces of less than certain knowledge. This points to one crucial sense in which De Morgan did not just innovate but redefined formal deductive logic: however formal, it sought to capture how rational human beings, including scientists, reason.

De Morgan's introduction of probability into logic was connected to his views on scientific methodology—i.e. his 'third method'—via his ideas on inverse probability or probability of causes. This field, which would today be called mathematical statistics,[95] dealt with the evaluation, in terms of probabilities, of competing hypotheses about the unknown causes of observed events. In De Morgan's own words: 'An event has happened, such as might have arisen from different causes: what is the probability that any one specified cause did produce the event, to the exclusion of other causes?'[96] De Morgan, approaching this situation in terms of scientific discovery, rejected the vague eliminative strategies championed by Bacon and his followers: a scientist cannot just 'lay down his this, his that, and his t'other [for example, one or two conceptions], and say, "now, one of these it must be; let us proceed to

95 For a discussion of De Morgan's work on 'statistical hypothesis testing', see Adrian Rice and Eugene Seneta, 'De Morgan in the Prehistory of Statistical Hypothesis Testing', *Journal of the Royal Statistical Society*, 168:3 (2005), 615–627.

96 De Morgan, *An Essay on Probabilities*, p. 53.

try which"'.[97] Rather, the best that could be done in such situations was to provide a quantitative criterion for choice between 'this, that and t'other'. Following a long line of mostly Continental mathematicians who had used probability to introduce scientific method into the realm of mathematics,[98] De Morgan believed that the probabilities of competing hypotheses could be measured and compared, not just with one another but with some standard of certainty (such as 'moral certainty'). This he did on the basis of the inverse probability techniques of Thomas Bayes and Pierre-Simon Laplace.

The core equation—letting h stand for a hypothesis and e for a body of evidence, where the conditional probability of h given e was to be interpreted as the degree of belief in the hypothesis given the evidence[99]—was used to calculate the rate at which the probability of a hypothesis increased with the number of confirming instances. However intuitive, a lot of assumptions, which would soon come to be seen as highly problematic, were needed to make this reduction of induction to deduction work. For example, perhaps most notoriously, in order to assign a value to the probability of the hypothesis before consideration of the data, namely, the prior probability $P(h)$, De Morgan and others made use of the 'Principle of Insufficient Reason'—which said that if there is no reason to favor one hypothesis over another, each should be assigned the same probability. The appeal to prior ignorance or, that is, to equally likely cases, was often confusing enough in simple cases of repeated drawings of balls from an urn with black and white balls, let alone in that of well-specified causes of complex natural events. It caused many to doubt whether a mathematical theory first developed for urn models could easily be extended, if at all, to model scientific reasoning. De Morgan, perhaps the most fervent British advocate of Continental probability, was among those—like Laplace, Condorcet and Poisson—who believed in the project of probabilising scientific method.

97 De Morgan, 'The Works of Francis Bacon', p. 367.

98 See, for instance, Lorraine Daston, *Classical Probability in the Enlightenment* (Princeton: Princeton University Press, 1988), Chapter 5 ('The Probability of Causes').

99 For someone like De Morgan, who treated probability as a branch of logic—and thus applicable to the relationship between propositions—this meant that propositions were assigned a definite numerical probability with respect to a body of data.

Consequently, he shared many of their assumptions and made similar mistakes, as his slightly younger peers George Boole and Robert Leslie Ellis were quick to point out.[100]

Ellis is particularly relevant, as he was one of Whewell's most dedicated protégés. Perhaps because his scientific methodology was so evidently at odds with that on which probability was constructed, Whewell showed little to no interest in probability, and when he used the term, it was often in a colloquial sense. Whewell's *Philosophy* did include discussions of such probabilistic methods as the 'method of means' and 'method of least squares', but these were brief (5 pages) and derivative.[101] It was Ellis who took up the problem of reconciling probability theory with a Whewellian philosophy of science, for which he asked Whewell's written permission.[102] Ellis's central argument was twofold. First, that what mathematicians like Laplace tried to prove mathematically, such as the regularity of nature, was true *a priori*. Second, that probability calculations rested on *a priori* truths, 'supplied by the mind itself'.[103] One implication was that probabilities cannot be said to be the 'measure of any mental state', for instance concerning the truth of an uncertain proposition. Another implication was that the theory's applicability to scientific inference was very limited, insofar as it was inadequate to the way people actually think:

> Our confidence in any inductive result varies with a variety of circumstances; *one* of these is the number of particular cases from which it is deduced. Now the measure of this confidence which the theory professes to give, depends on this number exclusively.

100 See, for example, George Boole, *An Investigation of the Laws of Thought on which are founded the Mathematical Theories of Logic and Probabilities* (London: Walton & Maberly, 1854), pp. 363–68, especially p. 364.

101 See William Whewell, *The Philosophy of the Inductive Sciences, Founded Upon Their History. Volume II* (London: John W. Parker, 1840), Book XIII ('Of Methods Employed in the Formation of Science'), Chapter VII ('Special Methods of Induction Applicable to Quantity'), pp. 550–56.

102 See Robert Leslie Ellis to William Whewell, TCC, Whewell Papers, Add. Ms.c.67/104. For a discussion of Ellis's work on foundations of probability theory, see Richards, 'The Probable and the Possible', pp. 64–65; and Lukas M. Verburgt, 'Robert Leslie Ellis's Work on Philosophy of Science and the Foundations of Probability Theory', *Historia Mathematica*, 40:4 (2013), 423–54.

103 Robert Leslie Ellis, 'On the Foundations of the Theory of Probabilities', *Transactions of the Cambridge Philosophical Society*, 8 (1844), 1–6 (p. 4).

> Yet no one can deny, that the force of the induction may vary, while this number remains unchanged.[104]

Ellis elaborated this point in an attack on one of De Morgan's examples in his 1837 'Theory of Probabilities', where he had calculated the probability that a vessel will have a flag on the basis of the previous ten vessels having one. But, Ellis asked, 'What degree of similarity in this new event to the previous ones, entitles it to be considered a recurrence of the same event?' The fact that this depended not only on the event, but also on the mind which contemplated it, showed that probability theory was too simplistic even to describe such an everyday situation. Likewise, regarding more complex cases based on assuming equal prior probabilities, Ellis wrote: '[M]ere ignorance is no ground for any inference whatever. *Ex nihilo nihil*.'[105] The human mind is a source of knowledge only, and precisely, insofar as it is actively involved in its creation. No doubt Whewell would have agreed.

Afterword

Most commentators have attributed the neglect of De Morgan's anti-Baconian programme either to the broader process of the downfall of classical probability or to technical mistakes. What has so far received little attention is the intellectual context in which it took shape, more specifically the fact that it was based on a meta-scientific vision that challenged the prevailing orthodoxy, represented by Whewell and his fellow Baconians.

First, De Morgan questioned not just the idea that scientific knowledge is obtained by induction alone but also the deeper conviction that it was possible to formulate a non-probabilistic method of scientific inference. Every Baconian, whether Whewell, Herschel or Mill, believed that their rules for inductive reasoning guaranteed the truth of the conclusions to which the application of these rules led. This belief, in turn, was premised on the assumption that there was no significant element of uncertainty attached to the conclusions of induction. Or, more precisely, there was

104 Ellis, 'Foundations', p. 4.

105 Robert Leslie Ellis, 'Remarks on an Alleged Proof of the Method of Least Squares', *Philosophical Magazine*, 37 (November 1850), 321–28 (p. 325).

such an element of uncertainty, but this pertained to the process and not the outcome of discoveries: for instance, whereas for Whewell there were no rules for a genius to arrive at conceptions, these conceptions themselves infallibly led to knowledge of necessary truths. Given such an outlook on science, it seemed an epistemic category mistake at best to even introduce probability techniques into its methodology.

Second, De Morgan went one step further by trying to reduce induction to deduction, not as Whately had done by saying that every induction is a syllogism, but by following Laplace in showing that it is based on inverse probability theory. De Morgan's alternative scientific methodology, which said that discovery is achieved by starting from a hypothesis whose probability increases as the number of confirming observations grows, was deliberately anti-Baconian in its formality. At the same time, it achieved little success—at least for a time—in large part because it failed to satisfy certain pre-formal, typically Baconian, conditions.[106] One of these conditions was that a hypothesis becomes more likely with the addition of confirming observations, but not in a linear fashion: this is because, as Whewell argued,[107] a hypothesis is made more probable by predicting surprising phenomena than by the successful prediction of unsurprising phenomena. The clash of De Morgan's 'Laplacian', quantitative probability with Whewell's 'Baconian', more qualitative view of probability was surprisingly long-standing, evidently touching on conflicting philosophical intuitions about the nature of science.[108] It continued in the 1870s–80s debates between William Stanley Jevons and John Venn, who respectively defended and attacked De Morgan, and C.D. Broad, W.E. Johnson and J.M. Keynes in the 1910s–20s. By that time, the scientific and philosophical landscape had, of course, changed considerably, and Whewell and De Morgan were names remembered only vaguely.

Quite a lot has been written recently on Whewell and his circle. Snyder has put him at the centre of a 'Breakfast Club', also consisting

106 See Laudan, 'Induction and Probability', pp. 193–94.

107 See, in this regard, Larry Laudan, 'William Whewell on the Consilience of Inductions', *The Monist*, 55 (1971), 368–91.

108 For the distinction between Pascalian (or Laplacian) and Baconian probability, see the work of L. Jonathan Cohen, for instance his 'Some Historical Remarks on the Baconian Conception of Probability', in L. Jonathan Cohen, *Knowledge and Language* (Cham: Springer, 1980), pp. 245–59.

of Herschel and Babbage. Ashworth, as a welcome corrective to this narrative, has zoomed in on a 'Trinity Circle', showing that from the 1820s onwards, Whewell's meta-scientific project increasingly diverged from that of Herschel and, especially, Babbage. The present chapter has attempted to add to this line of inquiry, highlighting the differences rather than commonalities between key figures in the early Victorian meta-scientific debates, by introducing De Morgan into the picture. It makes the picture more complex and, hopefully, richer. Much more work needs to be done to think through De Morgan's position vis-à-vis the Baconian tradition and the role of his anti-Baconianism in its demise. What place did he occupy on the fault-lines dividing Whewell from Babbage and Babbage from Herschel, for example? Whatever the specific answer will be, addressing such a question is likely to advance our understanding of the fascinating world of pre-Darwinian science and philosophy, as well as De Morgan's place in that world.

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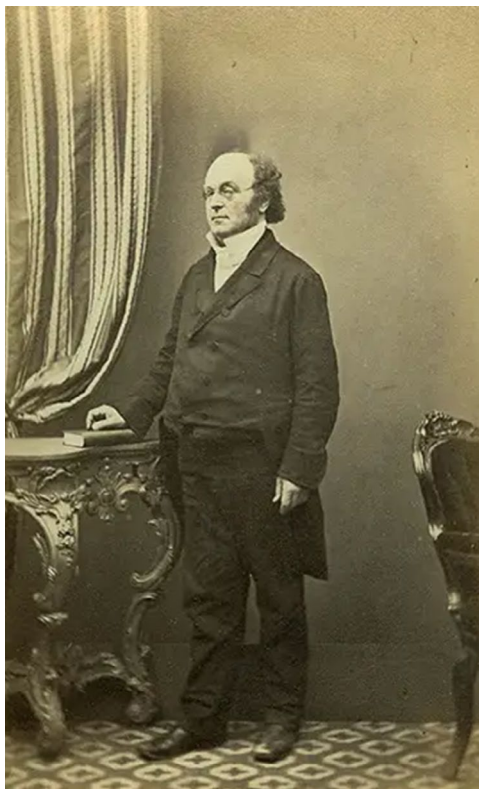

Fig. 8 Augustus De Morgan pictured in the 1860s. (Public domain, via MacTutor, https://mathshistory.st-andrews.ac.uk/Biographies/De_Morgan/pictdisplay/)