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The Great Instauration of the Eighteenth Century

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Abstract: This paper argues that there took place in the eighteenth century a specific, distinctive and essential phase in the emergence of modern science, a phase which can be characterised as "the Great Instauration" in that it witnessed the large-scale realisation of Francis Bacon's earlier vision—albeit not, for the most part, through the specific means which Bacon had proposed. That claim is exemplified in three fields—the "physico-mathematical sciences," chemistry and electricity—each of which yielded dramatic and permanent advances in knowledge; and an attempt is then made to render those advances intelligible in terms of specific social and technical themes. The paper proposes that the eighteenth-century Great Instauration arose from the development of an international natural-philosophical community, made possible by new institutions and especially by new publication media. And it suggests that what made this social development epistemologically fruitful was an inherently progressive process which had been anticipated by Bacon, namely what Sophie Weeks has called his "cybernetic" account of knowledge-making—the refinement of both questions and techniques in the light of Nature's response to investigation.

Keywords: Eighteenth century, Great Instauration, Francis Bacon, physico-mathematical sciences, chemistry, electricity, natural philosophy, modern science

Introduction

When (and how) did modern science come into being? In the seventeenth century, with the "Scientific Revolution" (astronomy, physics, experiment, Newton)? Or in the early nineteenth century, with the "end of natural philosophy" and the "invention of science" (specialization, institutionalization, secularization)? Between these two answers to the question there has now been an impasse since the mid-1990s, an impasse which shows no sign of being resolved. Yet on one thing the two positions agree: eighteenth-century developments played no significant role in the process.[1] That seems strange, given that everyone knows that the eighteenth century witnessed the creation of important new sciences (such as electricity, geology, heat, meteorology) and a vast range of advances in old ones (for instance Linnaean botany, oxygen, the planet Uranus, photosynthesis); yet such is the hidden consensus which, it might be suspected, fixes the impasse in place. Thus the "eighteenth-century problem" has not only persisted, but has actually intensified, since Geoffrey Cantor coined that phrase in 1982.[2]

I contend that both of those views are right, and that both are wrong. Right, between them, to regard *both* the seventeenth century *and* the early nineteenth century as contributing in essential ways to the creation of modern science. Wrong, each of them, in eliding the contribution of the other to that process.[3] And above all wrong, both of them, in dismissing the eighteenth century from consideration.[4] For what I shall argue is that modern science was brought into being by not one, not two, but three transformations, taking place in the seventeenth, eighteenth and nineteenth centuries; that each of these was a distinctive

[1] For Henry, "the revolution was complete" by 1720; for Cunningham and Williams, "something of great importance happened… with respect to the investigation of nature" in the seventeenth century, and science was invented in the decades after 1760, but what took place in between is not part of the picture. John Henry, *The Scientific Revolution and the Origins of Modern Science* (1997), Basingstoke: Macmillan, 2008, p. 114; Andrew Cunningham and Perry Williams, "De-centring the 'big picture': *The Origins of Modern Science* and the modern origins of science," *The British Journal for The History of Science* 26:4 (1993), pp.407-432, at p. 417.

[2] Geoffrey Cantor, "Essay review: the eighteenth century problem: *The Ferment of Knowledge: studies in the historiography of eighteenth century science*," *History of Science* 20 (1982), pp. 44-63.

[3] For example, Cunningham and Williams, having said that "something of great importance" happened in the seventeenth century, add: "We are not going to make any statement here about what that something might have been. We will, however, put forward our recommendation that whatever-it-is should not be referred to as 'the scientific revolution'." ("De-centring the 'big picture'," p. 417). Conversely Scientific Revolution textbooks, by ending with Newton as almost all of them do, erase nineteenth-century developments (along with those of the eighteenth century).

[4] The picture just sketched is admittedly an oversimplification, but not, I submit, a distortion, as emerges from a consideration of the six "master theses" which Michael Bycroft has outlined in the introductory essay to the present issue. From the present perspective, these can be divided into three groups. (a) The First and Second Scientific Revolutions assimilate the eighteenth century to what went before or came after, thereby erasing the very possibility of its having a distinctive character. (b) Conversely, both the old and tired Enlightenment conception and the much more promising Classification picture treat the period as self-contained and thus fail to connect it with the wider narrative of science's origins. (c) Far more fruitful are discipline-formation and natural philosophy—but neither of these has received the attention that both of them deserve.

and indispensable part of the process; and that the eighteenth-century transformation—which I shall call the Great Instauration—built upon that of the seventeenth century and, by the same token, became the foundation for that of the nineteenth.[5]

I shall support this claim with three examples: one of ancient provenance, namely the physico-mathematical sciences, as I shall call them; one stemming originally from Islamic culture and much developed in Europe from the Middle Ages to the seventeenth century (chemistry); and a third that was new in the eighteenth century (electricity). Between these I shall use different expository strategies: exemplification for the "physico-mathematical sciences," historiography for chemistry, a selective overview for electricity. From each example I shall draw out two explanatory "themes," one social, the other technical; those themes—all of which pertain not only to the specific fields which I use to exemplify them, but to all three fields, and indeed beyond them—will be taken up and discussed in a further section. Finally, a brief conclusion will indicate some of the limitations of what this essay has covered and what it has argued. First, however, a note about periodisation and three of them about terminology.

Although it's convenient to use centuries as periods with respect to the (supposed) origins of modern science, these are of course merely arbitrary eras and I use them here merely as rough indicators. This kind of usage has become conventional: for example, it is widely agreed that the already-mentioned "Scientific Revolution" began, if it had a discernible beginning at all, in the sixteenth century, yet it is also common practice to say by way of shorthand (as I am doing here) that it took place "in" the seventeenth. Thus nothing hangs on the way I shall be using the phrase "the eighteenth century"; indeed it will eventually emerge that if a watershed is to be sought, it can best be located in the 1660s, though there has not been space to argue that suggestion as fully as it warrants.

The phrase "The Great Instauration" was of course Francis Bacon's, and referred both to his never-completed *magnum opus* and to the transformation of knowledge which he hoped to bring about. Historiographically, it was famously—and aptly—used by Charles Webster as the title of his 1975 book which showed (building on R. F. Jones's *Ancients and Moderns*) that during the three decades after Bacon's death in 1626, his programme had to a considerable

[5] Melhado observed in 1989 that the eighteenth century "may be broadly conceived as a middle stage between the great revolutions of the seventeenth century in such fields as astronomy, mechanics, optics, and mathematics, and the flourishing in the nineteenth of a cluster of disciplines, many of them quite new, in the context of the university," and added: "The links between these two periods remain to be delineated." Evan Melhado, "Toward an understanding of the chemical revolution," *Knowledge and Society* 8 (1989), pp. 123-137, at p. 127.

degree been implemented in England, by a circle of millenarian Puritans led by Samuel Hartlib and inspired not only by Bacon but also by his disciple the exiled Moravian philosopher John Amos Comenius.[6] Webster's book concluded by demonstrating that the Hartlib circle of the 1640s and '50s fed directly into the politically very different Royal Society that was created just two years after the Restoration of 1660. Thereafter, of course—though Webster did not explore the subsequent developments—Baconian ideas and projects mostly lost their earlier, initial association with millenarianism and with the radical projects that it had spawned. And although Bacon was the official hero of the Royal Society, and effectively also of the French *Académie des Sciences* created in 1666, he is seldom regarded as having much relevance to eighteenth-century investigations of Nature—despite the fact that, as is well known but seldom discussed, the famous *Encyclopédie* of 1751-72 was entirely organized in Baconian categories. Rather, the big name of the eighteenth century is always held to be Newton, both for his *Principia* of 1687 and for his *Opticks* of 1704.[7] Thus at first glance it appears perverse to use the term, as I propose to do, to characterize eighteenth-century developments; all that follows is an attempt to dispel that apparent difficulty.

A second term that should be mentioned is "Revolution," referring to developments in science both large ("the Scientific Revolution," "the Chemical Revolution" of the eighteenth century) and small (the myriad "invisible" revolutions which Thomas Kuhn posited as punctuating the history of all the sciences). I shall *not* be using that word to denominate eighteenth-century developments, even though I shall be claiming that those developments were at least as consequential as those of the preceding century and a half that continue to be summarised as "the Scientific Revolution." Now it could well be argued that "revolution" is apt as a suitably dramatic summary of the developments that will be imperfectly and incompletely sketched in the pages that follow, so why not use the word? Because it is both inappropriate and empty of significant content, as emerges from a brief review of its two main usages. (i) The word was used, from Lavoisier onwards and continuing to the present day, by those investigating Nature—from the natural philosophers of the eighteenth century to the "scientists" of the twentieth—sometimes to describe what they hoped to achieve (Lavoisier), more often referring to what they felt had already been

[6] R. F. Jones, *Ancients and Moderns: a study of the background of the battle of the books*, St. Louis: Washington University Studies, 1936; Charles Webster, *The Great Instauration: science, medicine and reform 1626-1660,* Duckworth, 1975.

[7] A crude but not unrepresentative indication: Thomas Hankins, *Science and the Enlightenment*, Cambridge: Cambridge University Press, 1985, includes 26 citations of Newton, 8 of Bacon (of which three on the *Encyclopédie*).

achieved by others, always with the simple meaning of a once-for-all, irreversible, progressive shift in understanding, in procedures, or in both. This usage is always discipline-specific, which makes it inappropriate for a trans-disciplinary shift of the kind that I am claiming took place in the eighteenth century. (ii) From the mid-twentieth century it came to be used by historians in the ways already indicated—often with pernicious effects. In particular, from Kuhn onwards it began to be suggested that there either was, or should be, something in common between "revolutions" in science and political "revolutions," a claim that is simply nonsensical.[8] In this context, to plant the label "revolution" on the developments of the eighteenth century would be to add nothing and to invite misunderstanding.[9]

Third, the terms that we are apt to use for eighteenth-century activities present a whole cluster of dangers, not all of which I have succeeded in avoiding. I shall be speaking of three "fields," a metaphor which implies clear and stable boundaries—but no such boundaries either informed or inhibited eighteenth-century investigations of Nature, and indeed the absence of such boundaries was one of the important characteristics of those investigations. For instance, "physics" initially, and for most of the century, meant "natural philosophy";[10] Boerhaave's concept of fire played a giant part not only in chemistry (as we shall see) but

[8] Kuhn said that the two phenomena were similar, without troubling to look for any supporting evidence; I. B. Cohen claimed to support this, though his arguments were weak (see the next note). Roy Porter argued that the usage in respect to science should be narrowed down to those large-scale cases which resembled political revolutions (not that he made this fully explicit, but that was the effect of his various criteria for what should count as a "revolution" in science). Betty Jo Teeter Dobbs insisted, wrongly, that the word "revolution" was a "metaphor" taken from political history. More recently, Heilbron has proposed that the entire shape of the "Scientific Revolution" matches that of the French Revolution, for instance with Newton as Napoleon. Thomas S. Kuhn, *The Structure of Scientific Revolutions,* Chicago: University of Chicago press, 1962; Roy Porter, "The Scientific Revolution: a spoke in the wheel?," in Roy Porter and Mikulas Teich (eds.), *Revolution in History*, Cambridge: Cambridge University Press, 1986, pp. 290-316; B. J. T. Dobbs, "Newton as Final Cause and First Mover," *Isis* 85:4 (1994), pp. 633-643; John Heilbron, "Coming to Terms with the Scientific Revolution," *European Review* 15:4 (2007), pp. 473-489.

[9] What is lacking is a treatment of "revolution" as an actors' category, from the kind of perspective that Augustine Brannigan applied so effectively to "discovery" in *The Social Basis of Scientific Discoveries*, Cambridge: Cambridge University Press, 1981. Contrast I.B. Cohen's ill-conceived *Revolution in Science*, Cambridge, Mass.: Belknap Press, 1985, whose founding contradiction was identified by Ian Hacking, "Science turned upside down," *The New York Review of Books* 33:3 (1986), pp. 21-26 (I owe this reference to Greg Radick).

[10] *OED*, s.v. Particularly telling is the *OED*'s quotation from Harris's *Lexicon Technicum* of 1704: "Physicks, or Natural Philosophy, is the Speculative Knowledge of all Natural Bodies (and Mr. Lock thinks, That God, Angels, Spirits &c. which usually are accounted as the Subject of Metaphysicks, should come into this Science), and of their proper Natures, Constitutions, Powers, and Operations."

also in the theory of electricity;[11] natural history was not confined, as we might think, to description and classification, but could also embrace experiment;[12] and indeed chemistry, the inherently experimental activity, was part of natural history.[13] This difficulty (which, so far as I know, no historian has ever satisfactorily resolved) is itself a clue as to how to approach the "eighteenth-century problem":[14] the first step is to recognize how very *different* eighteenth-century investigations of Nature were from modern "science." Throughout, therefore, I have tried to avoid the anachronistic use of "science."

The Physico-Mathematical Sciences

Since this is going to become rather technical, let's begin on a lighter note. It's early December 1725, night-time, and we're watching a distinguished young gentleman courtier, the Honourable Samuel Molyneux—associate of the Duke of Marlborough, secretary to the Prince of Wales, member of both the English and Irish Parliaments—lying flat on his back and looking up through a 24-feet-long telescope which passes through a hole in the roof of his house.[15] What necessitates both the hole in the roof and the Hon. Samuel's recumbent posture is the fact that the telescope has to point straight upwards, or very nearly so, in order to minimise the distorting effects of atmospheric refraction. The reason for such fastidiousness is that Molyneux is aiming to detect—by collating tonight's observations with those of the succeeding weeks and months—what can only be, if he succeeds in detecting it at all, a very tiny movement, or rather, apparent movement—a seeming gradual movement of a relatively near star against the background of the more distant stars, that movement being an

[11] R W Home, "Nollet and Boerhaave: A note on eighteenth-century ideas about electricity and fire," *Annals of Science* 36 (1979), pp. 171-176.

[12] For instance, Stephen Hales' *Vegetable Staticks* of 1727 was subtitled "an account of some statical experiments on the sap in vegetables: being an essay towards a natural history of vegetation."

[13] Maurice Crosland, "Chemistry and the chemical revolution," in Rousseau and Porter (eds.), *Ferment of Knowledge*, pp. 395-6.

[14] One notable attempt to resolve it is John L. Heilbron, "A Mathematicians' mutiny, with morals," in Paul Horwich (ed.), *World Changes: Thomas Kuhn and the Nature of Science*, Pittsburgh: University of Pittsburgh Press, 2010, pp. 81–130, at p. 100.

[15] This account is based on James Bradley, "A Letter to Dr. Edmund Halley… giving an account of a new-discovered Motion of the Fixed Stars," *Philosophical Transactions* 35 (1729), pp. 637-661; Stephen Peter Rigaud (ed.), *Miscellaneous Works and Correspondence of the Rev James Bradley, DD, FRS,* Oxford: Oxford University Press, 1832; John Fisher, "Conjectures and reputations: The composition and reception of James Bradley's paper on the aberration of light with some reference to a third unpublished version," *The British Journal for the History of Science* 43:1 (2010), pp. 19-48; and *ODNB* entries for Bradley, Graham and Molyneux.

illusion created by the Earth's annual motion around the Sun. (At the time, this potential phenomenon had no name; our term "stellar parallax" was coined around 1760, but only took hold in the mid-19th century.) Such a finding would finally clinch the claim that the Earth moves around the Sun. For this purpose, the star to pick is the one called "γ Draconis" in the conventional star catalogue: it's very bright (which was taken to mean that it's relatively near to us) and its position is close to the pole star (so it's almost directly overhead). In all of this, Molyneux and his two collaborators—the astronomer James Bradley and the instrument-maker George Graham—were following the lead of Robert Hooke half a century earlier. Where they had, it seems, improved on Hooke was in the mounting of the telescope, enabling them to move it slightly away from the vertical, to fix it in position, and to register its precise orientation. More fastidiousness—very much in the spirit of Hooke himself.

Their investigation was initially a crashing failure—because while "γ Draconis" did indeed appear to move as the weeks unfolded, it did so not in the expected way but following a quite different path, and one that led to disagreement: Molyneux attributed it to "nutation" (wobbling) of the Earth's axis, and was convinced that this refuted Newton's planetary system, whereas Bradley suspected a flaw in the instrumental set-up. Fate left that tension unresolved, because the Grim Reaper removed Molyneux in early 1728.[16]

Yet that very failure led to resounding success, because subsequently, and chiefly through Bradley's efforts, these unexpected movements were first confirmed in other stars, then imaginatively explained, and finally, in 1729, presented to the Royal Society and published in the *Philosophical Transactions* as a major new discovery: the "aberration of light" (as it came to be called from about 1750). This was a heroic achievement both technically and conceptually, and was rich in implications. The technical challenge involved both refinement of the instrument (in order to widen the view beyond "γ Draconis," Graham had to make another telescope, which was fixed in the house of Bradley's uncle) and considerable observational skills (Bradley could prevent the viewed star from "fluttering"; Edmond Halley, one of several minor collaborators in the project, could not). The conceptual breakthrough was Bradley's realizing that because the light from any star takes time to reach the Earth, the star's apparent position is influenced by the Earth's annual motion around the sun—thereby explaining the seeming motion, that is, the tiny shifts from night to night of

[16] It has been claimed that "Molyneux, after his appointment on 29 July 1727 as one of the lords of the Admiralty, was no longer able to assist [Bradley]" (*ODNB* Molyneux); yet the final observation made with Molyneux's instrument took place on 19 December of that year: Memoirs of Bradley, p. xxviii, in Rigaud (ed.), *Miscellaneous Works and Correspondence of the Rev James Bradley*.

a given near star's position in relation to its more distant neighbours. (Hence that later term "aberration of light.") As for the implications, for one thing, Bradley was able to estimate, with remarkable accuracy, the ratio between the speed of light and the speed of the Earth's orbital motion, and for another, his subsequent pursuit of greater accuracy led him to the further discovery that the earth's axis also showed nutation (as Molyneux had hoped, though not in the way he had thought). And last but not least, the initial failure had been effectively obliterated—since the Earth's motion was indeed confirmed, albeit in a very different way from what had been intended.

The episode is emblematic of the eighteenth-century Great Instauration in many ways; here are six of them, of which the final two will be taken up as themes in this paper's penultimate section.

[1] *Seventeenth-century inheritance*: The entire conception of the investigation stemmed from seventeenth-century achievements (notably the telescope) and projects (that of Hooke to create observational support for the Earth's motion).

[2] *Achievement:* Bradley's discovery of aberration was of the first rank as a technical triumph, as an exemplar of what could be achieved, and for its intrinsic importance. Indeed, it might be seen as a better candidate than Newton's 1687 *Principia* for the honorific role of completing what Copernicus had started.

[3] *Instruments:* The practical, technical basis was the combined use of a modern physical instrument (the telescope and its all-important firm mounting) and a very old procedure that in effect functioned as a kind of instrument (the collation of sequential observations, which of course had characterised astronomy since ancient times).

[4] *Publication*: Bradley had both the empirical law (that all putatively-near stars exhibited an apparent annual elliptical motion) and his theory (that this apparent motion of any given star resulted from that of earth during the passage of its light) by September 1728; it was only four months later that his findings were presented to the Royal Society. Furthermore, after another four months (in May 1729) the discovery was discussed in the Jesuit *Journal de Trévoux*, making it known internationally.[17]

[5] *Personnel*: Bradley was professor of astronomy at Oxford (and also a minister in the church of England); Graham was London's leading instrument-maker; Molyneux, though an accomplished and dedicated astronomical observer, was what would later be called an amateur, in the sense of one who pursued that activity for the love of it. That spread of roles was somewhat fortuitous, in that when the investigation began, around 1722, it had involved not

[17] Fisher, "Conjectures and reputations," pp. 37, 38, 43.

Bradley but James Pound, another amateur; Pound died in 1724 and Bradley, who was his nephew (and effectively his adopted son) took his place. But the mixture—an instrument maker, an amateur, a clergyman-professor—was broadly typical of British investigators of Nature throughout the eighteenth century, in contrast both with the seventeenth century and with the nineteenth. What especially needs to be stressed is the collaboration between the three, even though the actual processes of that collaboration are elusive, as are the motives of the investigators. It has been claimed that the aim was to resolve an implicit tension between Hooke's results with γ Draconis and the arguments of Newton's *Principia*,[18] but it is by no means clear what made that problem, in the early 1720s, sufficiently urgent to warrant the enormous effort and expense that went into the investigation. I suspect that the real instigator was Graham, both because he was the direct successor to Thomas Tompion who had been Hooke's instrument-maker (whereas none of the others had any particular connection with Hooke), and because his commercial interests gave him a powerful motive for making the attempt: proof of Copernicanism by means of one of his instruments would have been the early-modern equivalent of the ad-man's dream.[19]

[6] *Precision*: The detection of the new motion depended on very precise measurements. It has been claimed that the second half of the eighteenth century witnessed an "acceleration in accuracy of instruments," so that what has been dubbed the "quantifying spirit" was particularly characteristic of the final third of the century.[20] Yet as Bradley's activities and achievements illustrate, such a process was under way by the 1720s, and the foundations for that later acceleration were being laid before 1750. Those foundations were threefold, embracing improvements in *instrumentation* (for instance verniers and micrometers); the practical *use* of such aids (both the vernier and the micrometer were seventeenth-century inventions but were much more widely used in the eighteenth); and the development of *mathematical tools* for the elucidation of astronomical phenomena, initiated in the 1740s by Jean le Rond d'Alembert and Leonhard Euler.[21]

[18] *Ibid.*, p. 25.

[19] Richard Sorrenson, *Perfect Mechanics: instrument makers of the Royal Society in the eighteenth century*, Boston, Mass.: Docent Press, 2013, pp. 22-26, gives a beautiful account of the links between Hooke, Tompion, Graham, and the Royal Society; Jim Bennett has also rightly talked up the Hooke-Tompion relationship: "Instruments and Ingenuity," in Michael Hunter and Michael Cooper (eds), *Robert Hooke: Tercentennial Studies* (Routledge, 2005), pp. 65-76.

[20] John L Heilbron, "Introductory Essay," in Tore Frängsmyr, J.L. Heilbron and Robin E. Rider (eds.), *The Quantifying Spirit in the 18th Century*, Berkeley University of California Press, 1990), pp. 1-23, at p. 8.

[21] Curtis Wilson, "Astronomy and Cosmology," in Roy Porter (ed.), *The Cambridge History of Science, Volume 4: Eighteenth-Century Science,* Cambridge: Cambridge University Press, 2003, pp. 328-353, at pp. 338-9.

All that has been possible here is a tiny glimpse of a vast terrain, and one whose very identity may seem artificial or anachronistic. Yet the phrase "physico-mathematical sciences" was used in the eighteenth century, and indeed began at that time (the adjective dates from the 1720s, the noun phrase from the 1760s), while the conjoining of mathematics with physics in the narrower sense that was just beginning to develop was highly characteristic of the period. (And the more familiar phrase "mixed mathematics" shows a similar historical usage-pattern, suddenly soaring in the 1750s.) Of this I shall give two little concluding examples. (i) On 8 January 1698 Johann Bernoulli, professor of mathematics in the University of Groningen, reported to Leibniz that he had been required by the University governors to "amuse our students with mathematic-physical experiments";[22] we shall see later on that this initiative was highly fruitful. (ii) The second example, from 1739, will also serve to illustrate the advances in mathematics which, I would claim, were very much part of the eighteenth-century Great Instauration. This was one of Leonhard Euler's many innovations in mathematics: his liberation of the trigonometric variables (sine, cosine, etc.) from their geometrical roots by redefining them as algebraic "functions," which made it possible to bring them—as neither Newton nor Leibniz had done—within the scope of the differential and integral calculus. As I have just described it, this appears to be a purely mathematical matter; yet as Katz has shown, while the problem could in principle have been motivated mathematically (since its solution closed a gap in the theory and practice of the calculus), Euler's entire attack on it was prompted by a physical problem posed by Daniel Bernoulli (to do with "the vibrations of an elastic band").[23] Similar instances of cross-fertilisation obtained between mathematics and astronomy, and between mathematics and ballistics.[24] In sum, the mathematics of the eighteenth century was profoundly embedded in the physical sciences,[25] and this cluster of eighteenth-century enquiries and investigations already marks out the period as productively creative on a massive scale.

[22] Tammy Nyden, "Experiment in Cartesian Courses: The Case of Professor Buchard de Volder," *Proceedings of the 4th International Conference of the ESHS*, Barcelona, 2010, pp. 384-388, at p. 385.

[23] See Victor J. Katz, "The Calculus of the Trigonometric Functions," *Historia Mathematica* 14 (1987), pp. 311-324, particularly p. 318.

[24] Brett D. Steele, "Muskets and pendulums: Benjamin Robins, Leonhard Euler, and the ballistics revolution," *Technology and Culture* 35 (1994), pp. 348-382; Ken Alder, "French engineers become professionals; or, how meritocracy made knowledge objective," in William Clark, Jan Golinski and Simon Schaffer (eds.), *The Sciences in Enlightened Europe*, Chicago: University of Chicago Press, 1999, pp. 94-125, at pp. 113-116; Catherine France, "Gunnery and the struggle for the new science (1537-1687)," Ph. D. thesis, University of Leeds, 2014.

[25] Curtis Wilson, "Astronomy and Cosmology": H J M Bos, "Mathematics and Rational Mechanics," in Rousseau and Porter (eds.), *Ferment of Knowledge*, pp. 327-356.

Chemistry

Chemistry is both the easiest and the hardest case for the present argument. The easiest, because it's a field amply endowed with important and well-recognized eighteenth-century achievements, notably the discovery of different airs (first fixed, then inflammable, followed by nitrous, dephlogisticated and many more). The hardest, because those achievements are commonly wrapped up in the notion of "the Chemical Revolution," which has long been recognised as deeply problematical, yet seems impossible to shake off. And that notion has had profoundly pernicious effects: as Seymour Mauskopf has remarked, it "threw into obscurity chemical activities during the earlier part of the eighteenth century except for those 'ingredients' that fed into the narrative of the Chemical Revolution."[26] We are forced, therefore, to begin by appraising that concept.

The troubles attending "the Chemical Revolution" begin with its very content, on which there is no consensus: was it all to do with Lavoisier's reinterpretation of combustion—the replacement of "phlogiston" loss by "oxygen" gain—or rather with his account of the "aeriform state" (and the associated role of "caloric"), or with his theory of acidity?[27] Or, yet again and more radically, did "the Chemical Revolution" comprise new "concepts of chemical composition"—the inauguration of the modern element-and-compound framework—in which case Lavoisier merely began that revolution, and Dalton completed it in 1808?[28] These questions, posed in 1982 in a penetrating review by John Christie and Jan Golinski,[29] have never gone away, and have indeed been enlarged: further candidates include nomenclature reform and the concept of chemical affinity,[30] while it has recently been claimed that the "Chemical

[26] Seymour Mauskopf, "Reflections: 'a likely story'," in Lawrence M. Principe (ed.), *New Narratives in Eighteenth-Century Chemistry*, Springer, 2007, pp. 177-193, at p. 179.

[27] The best standard account known to me is Carleton E. Perrin, "The Chemical Revolution," in R.C. Olby et al. (eds.), *Companion to the History of Modern Science*, Routledge, 1990, pp. 264-277; on its limitations, see notes 28 and 50 below. The most insightful account of Lavoisier's approach is Evan Melhado, "Chemistry, Physics, and the Chemical Revolution," *Isis* 76:2 (1985), pp. 195-211.

[28] Robert Siegfried and Betty Jo Dobbs, "Composition, a neglected aspect of the Chemical Revolution," *Annals of Science,* 25 (1968), pp. 275-293. This entire argument was regrettably overlooked by Perrin, "The Chemical Revolution." It has been fruitfully developed by Theodore M. Porter, "The promotion of mining and the advancement of science: the chemical revolution of mineralogy," *Annals of Science* 38:5 (1981), pp. 543-570, and by James W. Llana, "A contribution of natural history to the chemical revolution in France," *Ambix* 32:2 (1985), pp. 71-91. See also Hasok Chang, "Compositionism as a dominant way of knowing in modern chemistry," *History of Science* 49:3 (2011), pp. 247-268.

[29] J. R. R. Christie and J. V. Golinski, "The spreading of the word: new directions in the historiography of chemistry 1600-1800," *History of Science* 20:4 (1982), pp. 235-266.

[30] David Knight, *Voyaging in Strange Seas: The Great Revolution in Science*, New Haven: Yale University Press, 2014, p. 276; Mi Gyung Kim, *Affinity, That Elusive Dream: a genealogy of the Chemical Revolution*, Cambridge, Mass.: MIT Press, 2003.

Revolution" was not in fact a distinct event, but rather was continuous with the seventeenth-century "scientific revolution"—this by way of the distinctly old-fashioned theme of matter theory.[31] Somewhat analogously, but more convincingly, J B Gough argued in 1988 that Lavoisier merely completed a revolution that had been initiated by Stahl.[32] Going much further, German historians of chemistry have repeatedly cast doubt on the very idea of "the Chemical Revolution": Ursula Klein, the most extreme proponent of this view, has called it "a revolution that never happened."[33] Yet strangely, nobody has attempted to rebut that sceptical claim,[34] nor has recent work faced up to the issues that Christie and Golinski raised. Instead, historians of chemistry have bypassed the entire problem—producing in the past twenty years some four separate collections on early-modern chemistry in which "the Chemical Revolution" is almost entirely absent.[35] Meanwhile, in an ironic counterpoint, "the Chemical Revolution" remains the organizing concept in every single textbook account of eighteenth-century chemistry.[36] This contradiction has been captured well, if perhaps inadvertently, by the leading general history-of-science textbook, which asserts that "we have little choice but to reject the chemical revolution," yet frames its account of eighteenth-century chemistry in terms of that very "revolution."[37]

Despite its limitations, to which I shall return in a moment, the new historiography of early-to-mid-eighteenth-century chemistry has vastly expanded

[31] Victor D. Boantza, *Matter and Method in the Long Chemical Revolution*, Routledge, 2013.

[32] J.B. Gough, "Lavoisier and the Fulfilment of the Stahlian Revolution," *Osiris* (2nd Series) 4 (1988), pp. 15-33.

[33] Ursula Klein, "A revolution that never happened," *Studies in History and Philosophy of Science* 49 (2015), pp. 80-90. See also Christof Meinel, "'. . . to make Chemistry more Applicable and Generally Beneficial'—The Transition in Scientific Perspective in Eighteenth Century Chemistry," *Angewandte Chemie Int. Ed. Engl.* 23 (1984), pp. 339-347; Ursula Klein and Wolfgang Lefèvre, *Materials in Eighteenth-Century Science*, Cambridge, Mass.: MIT Press, 2007; and Wolfgang Lefèvre, "Viewing chemistry through its ways of classification," *Foundations of Chemistry* 14 (2012), pp. 25-36.

[34] Though see Maurice Crosland, "Lavoisier's achievement; more than a chemical revolution," *Ambix* 56:2 (2009), pp. 93–114, at p. 107 (responding to Klein and Lefèvre, *Materials in Eighteenth-Century Science*), who however merely cites I.B. Cohen's book of 1985.

[35] Lissa Roberts & Rina Knoeff (eds.), *The Places of Chemistry in Eighteenth-century Great Britain and The Netherlands*, special issue of *Ambix* 53:3 (2006), pp. 197-272; Lawrence M. Principe (ed.), *New Narratives in Eighteenth-Century Chemistry,* Pasadena, Cal.: Springer, 2007; John Perkins, "Sites of Chemistry in the Eighteenth Century," *Ambix* 60:2 (2013), pp. 95-178; Matthew Daniel Eddy, Seymour H. Mauskopf, and William R. Newman (eds.), *Chemical Knowledge in the Early Modern World*, special issue of *Osiris* 29:1 (2014), pp. 1-309.

[36] W.H. Brock, *The Fontana History of Chemistry*, London: Fontana Press, 1992; Porter (ed.), *Cambridge History of Science, Volume 4*; Peter Bowler and Iwan Rhys Morus, *Making Modern Science: A Historical Survey*, Chicago: University of Chicago Press, 2005.

[37] Bowler and Morus, *Making Modern Science*, p. 76.

and enriched our understanding—vindicating and massively extending the picture that Frederic L. Holmes put forward as long ago as 1971, of gradual, cumulative chemical progress in that period.[38] It is now accepted that the transition from alchemy to chemistry in the decades around 1700 involved widespread and diverse institutionalisation in many European polities, including the embedding of the subject in some academies and universities; that chemical techniques, of both analysis and synthesis, developed significantly in the first half of the eighteenth century; that chemistry at this time was also a productive field of conceptual speculation, rivalry and development; and that chemical expertise at that time was already fruitfully connected with fields of practical activity from medicine to mineralogy. Further, the new historiography has picked up and developed an important insight which Maurice Crosland articulated in the 1981 *Ferment* volume, yet which was long overlooked: that chemistry in that period was part of natural history, rather than (as it became around 1800) part of so-called "physical science."[39]

But as has already been implied, these achievements have come at a cost, a cost which has two aspects. The minor aspect is that it is seldom asked how the newly-disclosed developments of the early eighteenth century were connected with those of c. 1770 to 1800 that are still known as "the Chemical Revolution."[40] The major aspect is that the new historiography has left aside not only Lavoisier but also those early- and mid-eighteenth-century practices and theories—such as Stahlian and pneumatic chemistry—which have traditionally been seen as feeding into the "Chemical Revolution." For instance, when Joseph Black is now discussed, the emphasis is entirely on his teaching and industrial activities, ignoring his momentous discoveries of fixed air and of latent heat.[41] It is as if historians, in their determination to move away from the former excessive focus on Lavoisier, have also put aside everything and everyone notionally connected with him—which of course merely re-affirms his notional hegemony, rendering it untouchable. The attempt to bypass the Chemical Revolution has only left it all the more securely in place.

[38] Frederic L. Holmes, "Analysis by fire and solvent extractions: the metamorphosis of a tradition," *Isis* 62:2 (1971), pp. 128-48.

[39] Crosland, "Chemistry and the chemical revolution," pp. 395-396; Anna Marie Roos, *The Salt of the Earth: Natural Philosophy, Medicine, and Chymistry in England, 1650-1750*, Leiden: Brill, 2007; Matthew D. Eddy, *The Language of Mineralogy: John Walker, Chemistry and the Edinburgh Medical School, 1750-1800,* Farnham: Ashgate, 2008; Lefèvre, "Viewing chemistry through its ways of classification."

[40] Exceptions include Roos, *The Salt of the Earth*, Kim, *Affinity*, and Lawrence M. Principe, *The Transmutations of Chymistry: Wilhelm Homberg and the Académie Royale des Sciences*: Chicago, University of Chicago Press, 2020.

[41] Robert G.W. Anderson, "Boerhaave to Black: The evolution of chemistry teaching," *Ambix* 53 (2006), pp. 237-54.

We can start to disentangle this problem if we notice that "the Chemical Revolution" is not an actors' category,[42] for all that the word "revolution" was used at the time, both in private anticipation (Lavoisier in his notebook in 1773) and in public acclamation. The opening definite article (*The* Chemical Revolution), conveying uniqueness, instantly proclaims that this is a retrospective designation; the honorific capital letters underline this; and the disciplinary specificity differentiates the phrase from Lavoisier's initial "révolution en physique et chimie." When, why, and from whose hand, then, did the concept come into being? Strangely enough, these questions are easily answered from secondary sources,[43] even though those questions have never been posed—with the effect that the implications of the answers have gone unnoticed. Here are those answers:

When: in 1890.

Why: as a by-product of the centenary of the French Revolution, and in the context of longstanding Franco-German rivalry, indeed hostility, both national and chemical.

From whose hand: that of Marcellin Berthelot, in the form of a book entitled *La Révolution Chimique: Lavoisier*.

Thus the phrase "the Chemical Revolution," used to depict the activities, achievements and significance of Lavoisier, began as an anachronistic and politically-motivated imposition. It then found its way—through a complex process that urgently requires reconstruction, but is beyond the scope of the present paper—into American, British and French historiography, the leading figure (though not the first) being Henry Guerlac of Cornell University.[44] Furthermore, there is every reason to suspect that Berthelot's hand has remained

[42] A seeming exception is Fourcroy, arguably Lavoisier's most important ally, writing his vast *Système des connaissances chimiques* around 1800 (its eleven volumes appeared in 1801 and 1802). In his historical survey of the subject, Fourcroy depicted the researches of Black, Brownrigg, MacBride, Cavendish and Priestley as the "commencement d'une grande révolution chimique" (p. 27), went on to speak of "une immense révolution" (p. 28), and thereafter, when recounting the achievements of Lavoisier, repeatedly used the phrase "la révolution chimique": Antoine François de Fourcroy, *Système des connaissances chimiques et de leurs applications aux phénomènes de la nature et de l'art*, Paris: Bauduin, IX-X, 1801-1802. But that wording in 1800—that is, before the impact of the atomic hypothesis or electrochemistry, to say nothing of, for instance, the later periodic table—cannot have had the resonance it carried when deployed by mid- and late-twentieth-century historians.

[43] Crosland, "Chemistry and the Chemical Revolution," p. 403; Cohen, *Revolution in Science*, p. 236; Bernadette Bensaude-Vincent, "Between history and memory: centennial and bicentennial images of Lavoisier," *Isis* 87:3 (1996), pp. 481-99; Marco Beretta, "Introduction," in Beretta (ed.), *Lavoisier in Perspective*, Munich: Deutsches Museum, 2005, pp. 11-18, at pp. 13-17.

[44] To be fair to Guerlac, he also stressed Lavoisier's debt to earlier science in France and Germany, notably in his paper "Some French Antecedents of the Chemical Revolution," *Chymia* 5 (1959), pp. 73-112.

invisibly at work behind all subsequent work on Lavoisier. For according to Marco Beretta, writing in 2005, fewer than half of Lavoisier's manuscripts (other than letters) have been mentioned in the secondary literature—because historians "have primarily studied, classified and partially transcribed documents which had already been described by Grimaux and Berthelot."[45]

What is needed, then, is a larger picture which would *include* Lavoisier's achievements, without installing those achievements as the implicit telos of eighteenth-century chemistry as a whole; which would also embrace the wealth of developments disclosed by the new historiography; and which, finally, would bring back into focus the traditionally-recognised advances which that historiography has bypassed. I shall focus here on the third of these components.

The great dual achievement of eighteenth-century chemistry was the productive integration into chemical theory and practice of *fire* and of *air*. The two were linked (for instance, at the end of the period, both in Lavoisier's work and in Dalton's atomic theory), but can conveniently be considered separately, starting with the simpler case of air.

The banal point that needs to be made is that air's invisibility and intangibility posed a real and determinate obstacle to both the understanding and the control of chemical processes, an obstacle whose overcoming was essential to the constitution of chemistry as a science. That overcoming, effectively achieved by about 1770, was as momentous a breakthrough as any in the entire history of science. Air, a supposedly simple substance, became first "airs," that is, multiple substances, and then "gases," that is, matter in the now newly-recognised "vaporous state." Thereafter, *and not before*, chemistry was adequately equipped both practically and conceptually to deal with the tasks it had set itself. This watershed is usually (a) attributed to a small handful of British, chiefly English, dedicated investigators (Black, Priestley, Cavendish); (b) located after mid-century (Black's discovery of fixed air in 1756 being usually seen as the starting-point); and (c) associated with the "pneumatic trough" for collecting airs, which made it possible to study their properties. But each aspect of that broad-brush picture needs to be qualified.

In the first place, while the pneumatic breakthrough was indeed a specifically British achievement (a fact which seems to await explanation),[46] its

[45] Marco Beretta and Andrea Scotti, "Panopticon Lavoisier: A Presentation," in Beretta (ed.), *Lavoisier in Perspective*, pp. 193-207, at p. 199.

[46] And has to be qualified somewhat by the little-known work of Moitrel d'Element, published in Paris in 1719. See Louis-Bernard Guyton de Morveau, "Chymie, pharmacie et métallurgie," *Encyclopédie méthodique, ou par ordre de matières,* Volume 3, Part 2 (1782), pp. 404-7; J R Partington, *A History of Chemistry,* 4 vols., Macmillan, 1961-64, Vol. III, p. 112; and John Parascandola and Aaron J. Ihde, "History of the Pneumatic Trough," *Isis* 60:3 (1969), pp. 351-61, at p. 353.

well-recognized heroes were merely the leaders of a much larger group, amongst whom experimental investigations were commonly anchored in other, more practical interests—men such as William Brownrigg, Timothy Lane, James Lowther, John Maud and John Warltire.[47] Second, the process began long before 1750. Its roots arguably lay in the recognition of "damps," that is, noxious vapours, particularly in coal-mines—a topic discussed in the 1670s and '80s by Francis Jessop and Robert Plot,[48] and investigated in the 1730s and '40s by Lowther, Maud and Brownrigg.[49] Third, the instrumental side of the story was much more complex than the standard view that the pneumatic trough was invented by Stephen Hales (in the 1720s) and subsequently taken up by Cavendish et al, and that its invention was the precondition of pneumatic chemistry.[50] For one thing, Hales did not use the term "pneumatic trough" for any of the devices that he used to collect air; for another, the first people known to collect airs for study (Lowther, Maud, Black) used no such technique;[51] and last but not least, Brownrigg in the 1740s developed a sophisticated understanding of pneumatics without collecting airs at all.[52]

In sum, there is every reason to suppose that much remains to be learnt about the eighteenth-century practical and conceptual disaggregation of air; and that one aspect of this is that the relevant instrumental developments require much more careful attention. As we shall now see, the same applies to the story of fire in the eighteenth century, even though the shape of that story was quite different.

[47] Apart from Maud, all in *ODNB*. For Maud (and also Lowther), see *Phil. Trans.* 38 (1733), pp. 109-113, and 39 (1736), pp. 282-5, and Partington, *History of Chemistry*, pp. 109 and 313. Maud is mentioned also in *Grace's Guide*, entry for "Whiffen and Sons: Fisons Booklet" (https://www.gracesguide.co.uk/Main_Page, accessed 24 July 2022). For Lane, see also Partington, *History of Chemistry*, pp. 266, 320, and Noel G. Coley, "Physicians and the chemical analysis of mineral waters in eighteenth-century England," *Medical History* 26:2 (1982), pp. 123-44.

[48] Martin Lister, "An extract of a letter of July 28, 1675," *Phil. Trans.* 10 [Issue 117] (1675), pp. 391-5; Robert Plot, *The Natural History of Stafford-shire*, Oxford, 1686, pp. 133-44. I thank Anna Marie Roos and Josh Hillman for these.

[49] J. V. Beckett, "Dr William Brownrigg, F.R.S.: Physician, Chemist and Country Gentleman," *Notes and Records of the Royal Society of London* 31:2 (1977), pp. 255-71; Leslie Tomory, "William Brownrigg's Papers on Fire-damps," *Notes Rec. R. Soc.* 64 (2010), pp. 261-70.

[50] Parascandola and Ihde, "History of the Pneumatic Trough." A similar but distinct error is the claim that it was Hales's work which "spawned" or "inspired" British pneumatic chemistry (Perrin, "The Chemical Revolution," p. 267).

[51] For Black, see Maurice Crosland, "'Slippery substances': some practical and conceptual problems in the understanding of gases in the pre-Lavoisier era," in Frederic Lawrence Holmes and Trevor Harvey Levere (eds.), *Instruments and Experimentation in the History of Chemistry*, MIT Press, 2000, pp. 79-103, at p. 82; Douglas McKie, "On Thos. Cochrane's MS. notes of Black's chemical lectures, 1767–8," *Annals of Science* 1 (1936), pp. 101-110; David McBride, *Experimental Essays*, London, 1764, p. 52. For Lowther and Maud see the *Phil. Trans.* essays cited in n. 47 above.

[52] Tomory, "William Brownrigg's Papers on Fire-damps."

Fire entered the conceptual vocabulary of eighteenth-century chemists in two very different ways: in the form of Stahl's "phlogiston" (materia et principium ignis, non ipse ignis) and Boerhaave's "fire." The importance of phlogiston in the history of chemistry has long been recognized; I shall not review that vast topic here, but will focus instead on Boerhaave.

Boerhaave's fire was of course the direct ancestor of Lavoisier's *calorique*. But Boerhaave's fire had another, more permanent legacy, whose descent from Boerhaave has only very recently begun to be noticed: the concept of temperature, or rather, the modern concept thereof. Boerhaave's importance in this regard has been obscured by (i) the misleading continuity of two key words, temperature and thermometer; (ii) the fact that historians of science have shown very little interest in the development of thermometry;[53] and (iii) unwillingness (or inability) to recognise the importance of instrument-makers—for as we shall see, Boerhaave's fire only became real thanks to an instrument-maker, one Daniel Gabriel Fahrenheit.

The key breakthrough in understanding of this topic was made by John P. McCaskey as recently as 2020,[54] though important earlier contributions came from Jan Golinski and John Powers,[55] and independent confirmation can be found in the work of James Sumner.[56] These studies have revealed a momentous two-way transformation, which began around 1710 and was pretty much complete by 1770: chemistry created "temperature," which in turn transformed chemistry. In order to appreciate this, we need to dispel the illusion created by the merely verbal continuity of the word "temperature." Here are the essentials of McCaskey's argument (the separation into numbered propositions is mine):

[1] Temperature initially meant "*mixture*," with strong evaluative connotations—a suitable, healthy or appropriate mixture. It was part of a complex of words that included temper, temperament, and also (though

[53] This neglect on the part of historians of science is very strange, given that the thermometer has been presented as the paradigmatic example of the embodiment of "physical knowledge": Steven Shapin, "Here and everywhere: sociology of scientific knowledge," *Annual Review of Sociology* 21 (1995), pp. 289-321, at p. 308. The two great exceptions confirm the rule: Knowles Middleton was a meteorologist, Hasok Chang is primarily a philosopher. See W.E. Knowles Middleton, *A History of the Thermometer and Its Uses in Meteorology,* Baltimore: Johns Hopkins Press, 1966; Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress,* Oxford: Oxford University Press, 2004.

[54] John P. McCaskey, "History of 'temperature': maturation of a measurement concept," *Annals of Science* 77:4 (2020), pp. 399-444.

[55] Jan Golinski, "'Fit Instruments': Thermometers in Eighteenth-Century Chemistry," in Frederic L. Holmes and Trevor H. Levere (eds.), *Instruments and Experimentation in the History of Chemistry*, Cambridge, Mass.: MIT Press, 2000, pp. 185–210; John C. Powers, "Measuring fire: Herman Boerhaave and the Introduction of Thermometry into Chemistry," *Osiris* 29:1 (2014), pp. 158-177.

[56] James Sumner, *Brewing Science, Technology and Print, 1700–1880*, Routledge, 2013.

McCaskey doesn't include this) temperate. (Consider for instance "losing one's temper," or "the well-tempered clavier.") All such words derived originally from the Latin verb *temperare* which meant (the *OED* explains) "to divide or proportion duly, to mingle in due proportion, to combine properly; to qualify, temper; to arrange or keep in due measure or proportion, to keep within limits, to regulate, rule."

[2] Temperature did not refer solely to hot and cold. McCaskey gives this example: "In a work on measuring humidity, Boyle wrote 'the temperature of the Air is neither considerably moist, nor considerably dry.'"[57]

[3] What were called "degrees"—in Latin, *gradus* (identical in plural and singular)—meant discrete steps; there was no connotation of a continuum.

[4] The mere invention of instruments called "thermometers"—a process which is generally agreed to have been initiated by Galileo in the 1590s, while the word itself was coined in the 1630s—had no effect on the concept of temperature. On the contrary, the conceptual array just summarised remained unchanged throughout the seventeenth century and into the eighteenth. Hence the fact that Hooke, in his "method of making a history of the weather," spoke of "degrees of heat and cold," not temperature, as what the thermometer measured,[58] and that in *Gulliver's Travels* (1726), Swift extolled "the temperature"—meaning the moderateness—"of our climate."[59]

What broke this down and opened the road to the modern concept of temperature was a thermometer of a new kind—Fahrenheit's thermometer.[60] It was new not so much because it used mercury (though that doubtless helped) but above all because it was reliable—meaning that Fahrenheit was able to make multiple thermometers which agreed with each other, an unprecedented achievement whose precise technical basis was probably very complex.[61] But Fahrenheit's thermometer needed Boerhaave, and equally, Boerhaave needed Fahrenheit. The beauty of Fahrenheit's thermometer for Boerhaave was that it made fire *visible*; the attraction of Boerhaave's concept of fire for Fahrenheit,

[57] McCaskey, "History of 'temperature'," p. 413.

[58] Thomas Sprat, *The History of the Royal-Society of London for the Improving of Natural Knowledge,* London, 1667, pp. 173-9. I thank Jeanne Fahnestock for this.

[59] Jan Golinski, *British Weather and the Climate of Enlightenment*, Chicago, 2007, p. 58.

[60] As McCaskey puts it: "people who grew up around Fahrenheit thermometers (or competitors they spawned) conceptualized temperature differently than had their predecessors" ("History of 'temperature'," p. 415).

[61] This makes it intelligible that all the complex processes that have been reconstructed so well (philosophically if not always historically) in Hasok Chang's *Inventing Temperature* took place in the eighteenth century, not the seventeenth, despite the fact that the thermometer was an early-seventeenth-century invention.

who was as much an entrepreneur as an inventor, seems to have offered the prospect of a *market* for his instrument (and indeed, it was specifically Boerhaave's pupils who distributed the Fahrenheit thermometer in the ensuing decades).[62] And one further actor had a key place in the story: George Martine, himself a Boerhaave pupil, who made public the essential techniques that Fahrenheit had—understandably—kept secret.

The Boerhaave-Fahrenheit interaction happens to be partly documented in a series of letters, happily translated into English some forty years ago, whose rich potential has as yet barely been tapped by historians.[63] The collaboration between them nicely illustrates the complexity of the social arrangements associated with the making of natural knowledge in the eighteenth century; and Fahrenheit emerges as a remarkable figure, extraordinarily ingenious, and equipped with a superb ability to tailor his considerable technical skills to the interests of an actual or potential audience. (For Leibniz, he offered a perpetual-motion machine; for those who attended the lectures he gave in Amsterdam in 1718, "how to change base metals into noble ones," amongst many other topics.)

The case of chemistry, like that of the physico-mathematical sciences and (as we shall see) that of electricity could be used to illustrate a great variety of themes; the two that I shall pick out, for discussion in the penultimate section, are the social theme of institutionalisation and the technical one of instruments. The *institutionalisation* of chemistry between about 1660 and 1740 was nothing short of remarkable; the subject became embedded in many universities (for example, Leyden and Glasgow), in the Paris Académie, in the Swedish *Bergskollegium* (Board of Mines),[64] and in many German mining schools. And typically, even in didactic contexts, chemistry came with a strong investigative component, whence the many discoveries that ensued, from new metallic elements to better analytic techniques. As for the two *instruments* discussed here—the pneumatic trough and the thermometer—while I have been stressing the complexity and theory-embeddedness of their origins, we should not lose sight of their consequences. The pneumatic trough realised—made real—the pneumatic chemistry which for Brownrigg had been merely conceptual; the thermometer made possible first Joseph Black's twofold discovery of latent heat and specific heat, then the calorimeter which played an essential part in Lavoisier's chemistry, and alongside these, the concept of temperature that was to become one of the founding ontological concepts of modern science.

[62] Ibid, p. 60 note 6, citing the 1824 *Encyclopaedia Britannica* supplement to the 3rd, 4th and 5th editions, 5:331.

[63] Pieter van der Star (ed.), *Fahrenheit's Letters to Leibniz and Boerhaaave*, Amsterdam: Rodopi, 1983, quoted below from p. 9.

[64] Tore Frängsmyr, "Swedish Science in the Eighteenth Century," *History of Science* 12 (1974), pp. 29-42, at pp. 31-2.

Electricity

The phenomenon of "electricity" referred initially, in the sixteenth century, to the property of amber (whose Greek name was ἤλεκτρον, *elektron*) that when rubbed, it attracts light objects such as feathers or scraps of paper. Although the seventeenth century saw minor advances in the study of this phenomenon—for instance, the discovery that some other substances shared this property with amber—it had received very little attention before 1700, but in the course of the eighteenth century it acquired a hitherto unexpected importance as a growing field of experimental practice and of theorization. This history has been approached in several ways, variously emphasizing theories, concepts, experiments, entertainment, instruments.[65] But the dominant account, published over forty years ago, remains that of John L Heilbron, whose emphasis was different again: on Method with a capital M, meaning what might be called overall conceptual strategy, for the telos of that account was the eventual triumph—or supposed triumph—of instrumentalism over theory.[66] An adequate appraisal of Heilbron's *magnum opus* would take a paper in itself; I limit myself to observing that with respect to the "eighteenth-century problem," it is both part of the solution, and part of the problem. It is part of the solution insofar as it not only reconstructs in immense detail, and with exemplary and heroic scholarship, the main lines (and many of the branch lines) of eighteenth-century electrical research, but also brings to light many of the social and institutional settings within which that research was carried out and published. But it is also part of the problem, because it subtly, and sometimes not so subtly, assimilates eighteenth-century activities to their supposed telos of nineteenth-century science.

What has been lost sight of, not by Heilbron but by subsequent historiography, is that the eventual outcome of eighteenth-century electrical research was a massive advance in the *practical* conquest of natural phenomena: the creation in 1800 of an instrument, namely Alessandro Volta's "pile," which generated continuous electric current. If ever a discovery, or invention, made

[65] See respectively Thomas S. Kuhn, *The Structure of Scientific Revolutions,* Chicago: University of Chicago press, 1962; R.W. Home, *Aepinus's Essay on the Theory of Electricity and Magnetism*, Princeton: Princeton University Press, 1979; I. Bernard Cohen, *Franklin and Newton: An inquiry into speculative Newtonian experimental science and Franklin's work in electricity as an example thereof,* Philadelphia: American Philosophical Society, 1956; Patricia Fara, *Entertainment for Angels : Electricity in the Enlightenment*, Cambridge: Icon Books, 2003; W. D. Hackmann, *Electricity from Glass: The history of the frictional electrical machine, 1600-1850*, Alphen aan den Rijn: Sijthoff & Noordhoff, 1978.

[66] John L Heilbron, *Electricity in the Seventeenth and Eighteenth Centuries: A study of early modern physics*, Berkeley: University of California Press, 1979.

a difference, this one did; in Heilbron's words, it "opened up a limitless field."[67] Yet as a human achievement it has become historiographically invisible, as a glance at the textbooks makes all too clear.[68] Here we have a stark instance of the "eighteenth-century problem." No-one could deny (i) that current electricity was foundational for vast areas of nineteenth-century science; (ii) that it was brought into being by Volta's invention of the "pile"; or (iii) that that invention would not have been possible without the prior tradition of eighteenth-century electrical investigations. Yet those investigations have failed to weigh on the historiographic scales—which creates at least a prima facie case that the "eighteenth-century problem" has got worse since Cantor pointed it out in 1982.

We need to ask, then: how did Volta's pile come about?

This is a story that might be said—though somewhat inaccurately, as we shall see—to begin with a barometer in 1676 and to finish with a frog, a little over a century later. Those two moments were connected by a sometimes tortuous, yet always intelligible, process of theorization, experimentation, publication and controversy. In order to reduce this to a manageable compass, I shall concentrate on the barometer and the frog, merely glancing at the developments in between.

We begin, then, in 1676 with the barometer, an instrument of recent vintage (the word dates from the 1660s, the founding discoveries by Torricelli and Pascal from the 1640s).[69] It consists of a column of mercury in a narrow, vertical tube about three feet long, closed at the top, open at the bottom into a mercury reservoir; the mercury in the tube rises and falls with altitude, for it

[67] More fully: "The pile was the last great discovery made with the instruments, concepts and methods of the eighteenth-century electricians. It opened up a limitless field. It was immediately applied to chemistry, notably to electrolysis, and soon brought forth the shy elements sodium and potassium from fused soda and potash. [Subsequently,] its steady current provided the long-sought means for establishing a relationship between electricity and magnetism. The consequent study of electromagnetism transformed our civilization" (Heilbron, *Electricity*, p. 494).

[68] Volta's pile is barely mentioned in the main history of science textbook (where it appears *after* Oersted's subsequent discovery of electromagnetism). Similarly, the *Cambridge History of Eighteenth-Century Science* devotes less than three pages to the entire history of electricity, mentions the pile only incidentally, and in its sole allusion to Galvani merely describes him as an experimenter, not mentioning the topics of his research, let alone his finding of "animal electricity." See Bowler and Morus, *Making Modern Science*, 84; R. W. Home, "Mechanics and Experimental Physics," in Porter (ed.), *Cambridge History of Science, Volume 4,* pp. 354-74, at pp. 368-71, 372; Thomas H. Broman, "The Medical Sciences," in Porter (ed.), *Cambridge History of Science, Volume 4,* pp. 463-84, at p. 475.

[69] David Corson, "Pierre Polinière, Francis Hauksbee, and electroluminescence: a case of simultaneous discovery," *Isis* 59:4 (1968), pp. 402–13; Gad Freudenthal, "Early electricity between chemistry and physics," *Historical Studies in the Physical Sciences* 11 (1981), pp. 203-29; and *DSB* entries for Torricelli, Pascal and Picard.

is supported by the weight (Greek βάρος, *baros*) of the air—a notion as new, and initially as scandalous, as Torricelli's contention that the space at the top of the tube was not only empty of mercury but empty of matter, a vacuum. This particular barometer is in the hands of the astronomer Jean Picard, who notices a bluish glow in it when carrying it one night out of his dwelling in the recently-created Paris observatory.[70] The phenomenon is published, but attracts little attention,[71] perhaps because it's difficult to reproduce, more probably because nobody knows what to make of it. But that suddenly changes in the late 1690s, when this "mercurial phosphor" is picked up by Johann Bernoulli, professor of mathematics at the University of Groningen—it would seem in order to satisfy the new requirement (which I mentioned earlier) that he should "amuse our students with mathematic-physical experiments." Bernoulli pursues the mercurial phosphor with assiduous interest, apparently seeing it as offering a potential vindication of Descartes' theories of light and of matter. He investigates it systematically, painstakingly defines the conditions required to produce it, theorizes a Cartesian explanation—and in 1700 conveys his findings in three letters to the French Académie, which publishes them.

The impact of Bernoulli's published letters is dramatic, in two respects. For one thing, the "mercurial phosphor" is immediately taken up by at least three individuals, all independently—Pierre Polinière in Paris, Francis Hauksbee and Samuel Wall in London. For another, all three of them are led by their investigations to connect the "mercurial phosphor," wholly unexpectedly, with the well-known but seldom-studied phenomenon called "electricity." That remarkable connection has arisen through a combination of chemical theory (amber was "oily" or "sulphureous," and so was glass) and the notion that the emission of light was due to the motion of particles.[72] This theoretical conjunction leads each of Polinière, Hauksbee and Wall to explore the effects of friction between different substances, and thus gradually to change focus from luminescence to electricity. But although Nature and theory have conspired to bring about a striking convergence of their activities and findings, power and patronage now lead to a wild divergence in personal outcomes. Both Polinière and Wall

[70] Walking out at night with a barometer is not as strange as it may seem. Picard put much work into estimating the size of the earth, which required travel to different localities; a barometer would enable him to measure altitudes; he was probably embarking on such a journey and making an early start.

[71] Surprisingly, given that there was widespread interest at the time in "phosphors," that is, sources of light without heat. See for instance Jan V Golinski, "A noble spectacle: phosphorus and the public cultures of science in the early Royal Society," *Isis* 80:1 (1989), pp. 11-39.

[72] This from Freudenthal's brilliant article of 1981, to which no summary—certainly not this one—can do justice. The term "sulphureous" was almost as new as the barometer: the *OED* reports a usage in the 1620s, but such instances were patchy until the 1660s.

are disgracefully sidelined (Polinière by Bernoulli and his Paris allies, Wall by Isaac Newton); but Hauksbee, newly appointed as curator of experiments to the Royal Society, is encouraged to produce, for the eager audience of the Fellows, new demonstrations of electrical effects. This leads Hauksbee—who has already shown his mettle as an ingenious inventor of greatly improved air-pumps[73]—to invent two devices for the more effective demonstration of electrical effects: a glass rod or cylinder, and a rotating globe. And Hauksbee's experiments, carried out between 1705 and 1707, many of which are soon published, initiate the serious study of electricity.[74]

It can be seen why it is both true and false to say that our story begins with a barometer: false, in that its effective beginning was Hauksbee's experiments; true, in that those experiments owed their very existence both to Picard's fortuitous discovery and to Bernoulli's development of that discovery. That double discontinuity has gone unremarked, not least in Heilbron's story, based as it was on the implicit continuity of electrical enquiry. And further discontinuities may well be suspected amidst subsequent eighteenth-century electrical episodes. Nevertheless there is no doubt that electrical knowledge and mastery advanced on several fronts: greater power (as shown by sparks and shocks), new phenomena, new theories, more control, the creation of measuring instruments, and the connection of man-made "electricity" with natural phenomena, notably lightning.

But I shall leap to 1786 and to the frog—which itself can no longer leap, as it has been dissected by Lucia Galvani and her husband Luigi, who are using a preparation of the unfortunate creature's legs and spinal cord to investigate the susceptibility of animals to electrical stimuli. They have been pursuing such experiments for several years at their house in Bologna, trying to elucidate the relationship between the nervous fluid (which mediates the functions of the nerves) and the electrical fluid (thought to be involved in the action of the muscles).[75] But now, with the remnants of this particular frog strung up by a brass hook dangling from an iron wire, Lucia and Luigi stumble upon an entirely unexpected finding: the muscles do not just respond to an electrical stimulus; they seem to *generate* electricity. There follow five years of

[73] Terje Brundtland, "Francis Hauksbee and his air pump," *Notes and Records of the Royal Society* 66:3 (2012), pp. 253-72. This paper argues, incidentally, that Hauksbee's connection with the Royal Society was probably independent of Newton's presidency, despite the coincidence of dates.

[74] On Hauksbee's researches see R. W. Home, "Francis Hauksbee's theory of electricity," *Archive for History of Exact Sciences* 4 (1967), pp. 203-17 (still much the best exposition, and insufficiently appreciated).

[75] J. L. Heilbron, "The contributions of Bologna to Galvanism," *Historical Studies in the Physical and Biological Sciences* 22:1 (1991), pp. 57-85, at p. 68.

further research on many more frogs, to confirm and clarify the phenomenon, after which the newly-widowed Luigi (for Lucia died in 1790) announces in print his discovery of what he calls "animal electricity." He must surely have believed that he had hit upon the explanation for the remarkable powers of what the English called the "cramp-fish," or in Latin the torpedo—a flatfish which had for centuries been known to "benumb the fisher's hand," a capacity which had recently been attributed to electricity in the pages of the *Philosophical Transactions*—though he seems not to have mentioned this publicly, choosing instead to investigate torpedo fish privately, perhaps in the hope of some future breakthrough. [76] At all events, many throughout the international scholarly community are persuaded that Galvani's "animal electricity" is a real phenomenon.

But there is a weakness in Galvani's case: in order to elicit the phenomenon, it's necessary for the frog preparation to be in contact with two different metals (iron and brass initially, later other pairs such as iron and silver)—and Galvani's theory cannot account for this. As a result, a few people are not persuaded, and one of them is a formidable opponent with well-placed allies: Alessandro Volta, the professor of experimental physics at the University of Pavia, whose very different theory meets with favour in London's Royal Society. Volta regards the muscle of Galvani's frog as a mere *detector* of electricity; the electricity itself he sees as arising from the two metals, so he calls it "metallic electricity." Within a year of Galvani's publication, that is, in 1792, Volta issues a counterblast, and there ensues a protracted argument carried out on Galvani's behalf by his nephew Giovanni Aldini. The controversy remains unsettled, as well it might—for Volta's position has a strategic weakness that precisely matches that of Galvani's: just as Galvani cannot demonstrate "animal electricity" without the use of two metals, so Volta is unable to demonstrate "metallic electricity" without frog muscle.[77]

At some point, apparently in 1799, Volta—who is a skilled and seasoned experimenter—hits on a way around the problem: amplify the "metallic electricity" by making a battery, that is, an assemblage, of the two-metal junctions which in his view generate "metallic electricity." He does this in several ways, of which the most effective and important takes the form of a *pile* of discs—and this frees him at last from the frog, for the electric discharge from the pile, he writes, not only "excites contractions and spasms in the muscles" but also

[76] On the torpedo, see Marco Piccolino, *The Taming of the Ray: electric fish research in the Enlightenment, from John Walsh to Alessandro Volta*, Firenze: Olschki, 2003.

[77] Indeed, it has perhaps never been settled: see Hasok Chang, "Practicing eighteenth-century science today," in Mario Biagioli and Jessica Riskin (eds.), *Nature Engaged: science in practice from the Renaissance to the Present*, Basingstoke: Palgrave Macmillan, 2012, pp. 41-48, at pp. 47-50.

"irritates… the organs of taste, sight, hearing, and feeling, properly so called, and produces in them sensations peculiar to each."[78] And he adds that the pile is an "artificial electric organ" which precisely imitates the powers of the torpedo fish. All this he communicates to Joseph Banks, President of the Royal Society, who sees to it that Volta's letter is immediately published in the *Philosophical Transactions*. And thus did the pile come into the world.

The frog, then, is to the ending of our story much as the barometer was to its beginning. On the one hand, the final act is Volta's, not Galvani's, and the frog is no longer important—just as the opening move was Hauksbee's, not Bernoulli's (or Picard's), and the barometer had by then dropped out. But on the other hand, just as it was the "mercurial phosphor" which led to Hauksbee's innovations, so it was "animal electricity" which led to Volta's seminal invention.

Volta's pile was attended with two ironies. In the first place, his theory of "metallic electricity" had a strategic weakness that he failed to notice: he could not account for the fact that in addition to the two metals, his pile required an intervening moist conductor of electricity. As a result he was immediately outflanked theoretically by others (Nicholson and Carlisle in London, Johann Wilhelm Ritter in Germany) who offered the very different theory that the pile worked through chemical action—a claim that they were able to support, albeit indirectly, by showing that the electricity emanating from the pile had chemical effects, thereby opening up a vast new space of investigation. All of this seems to have been entirely lost on Volta. Second, and relatedly, Volta never grasped the fact that what the pile produced was something new: not a static electrical discharge, but a continuous current.

The social theme that I want to draw from this story is its *international* aspect. At its ending, this is apparent in the fact that the pile was invented in Italy, published in England, and rapidly exploited in Germany as well as England. So too its beginning, it will be recalled, involved events in Paris, in Groningen and in London. And were we to fill in the key details in between we should find ourselves again moving from place to place, sometimes rapidly: for instance, what really got electrical experimentation going was the work of an English researcher (Stephen Gray), but this only became influential because it was immediately taken up by a Paris academician (Charles Dufay)—their respective publications appearing in 1730 and 1733—while the key developments of the 1740s took place in Wittenburg (the invention of more powerful generating machines), in Leyden (the discovery of what quickly became known

[78] Quoted in Alexander Mauro, "The role of the Voltaic pile in the Galvani-Volta controversy concerning animal vs. metallic electricity," *Journal of the History of Medicine and Allied Sciences* 24:2 (1969), pp. 140-150, at p. 148.

as the Leyden phial, usually referred to by historians as the Leyden jar), and in Philadelphia (the wide-ranging researches of Benjamin Franklin).

The technical theme on which I shall focus in the next section is the remarkable fact that every single important discovery in this story was *accidental*. It is possible that in some cases (such as that of Dufay) this was a matter of presentation, a rhetorical trick; but most of the key breakthroughs really were fortuitous. That was certainly true, for instance, of Picard's discovery of the "mercurial phosphor," of the Leyden jar, of Galvani's "animal electricity," and of the fact that Volta's pile delivered a continuous current. We need to ask—whether or not we can answer the question—how a series of accidents should somehow produce cumulative progress.

Themes

Social themes

To recapitulate, these three themes were (i) "personnel," that is, the diversity of social roles of those engaged in natural knowledge; (ii) institutionalisation; and (iii) the fact that the pursuit of natural knowledge involved several different European polities. But it's convenient to consider these in a different order.

Institutionalisation: This was exemplified above in the case of chemistry, but it also took place across the board (we might instance Euler's salaried posts at the academies of first St Petersburg and then Berlin, or Volta's chair of experimental physics at Pavia—jobs that had not existed in the seventeenth century). The process was haphazard and its outcomes were patchy, but the overall effect was huge. Its two most important aspects, which were closely connected, were the creation of academies and of periodicals. And I suggest that it was these, and especially the periodicals, which brought about the Great Instauration of the eighteenth century.

The academies have been chronicled by McClellan,[79] but the periodicals await a comparable reconstruction. As Dawson and Topham have rightly said of the early nineteenth century, periodicals create communities;[80] once we apply that notion to our period, it becomes apparent that the coming into

[79] James E. McClellan, *Science Reorganized: scientific societies in the eighteenth century,* N.Y.: Columbia University Press, 1985. The title is ill-chosen, both because this was not yet science, and because what took place was not reorganization but organization (if indeed "organization" is apt at all in this context) for the first time.

[80] See Gowan Dawson and Jonathan R Topham, "Introduction: constructing scientific communities," in Gowan Dawson, Bernard Lightman, Sally Shuttleworth, and Jonathan R. Topham (eds.), *Science Periodicals in Nineteenth-Century Britain: Constructing Scientific Communities*, Chicago: University of Chicago Press, 2020, pp. 1-34.

being of the *Journal des Scavans* and the *Philosophical Transactions of the Royal Society*, both in 1665, was an event of the highest importance in the history of natural knowledge. Those two journals began to make a new social world, and this process was subsequently enhanced by additional periodicals such as the *Acta Eruditorum* founded in 1682 by Leibniz and Mencke, and a variety of others that emerged in the eighteenth century in association with some of the new academies. Yet it is remarkable how little attention this dramatic innovation has received from historians of science. Brendan Dooley's remarks in the context of Italy, penned in 2001, apply right across Europe and remain true two decades later:[81]

> Italian science between the age of Galileo and the age of Galvani and Volta underwent two revolutions, not one. The first concerned methods of investigation, and it has received a considerable amount of scholarly attention. The second revolution concerned methods of diffusion, and this has hardly been studied at all.

This gains added point from the fact—pointed out by Jim Secord almost twenty years ago—that the relevance of communication to science goes far beyond "diffusion," for communication is constitutive of science.[82] In the case of the early nineteenth century, this insight has subsequently been put to brilliant effect, not least by Secord himself;[83] but its potential for the early-modern period has yet to be exploited, and this despite interest in practices of writing (rhetoric), publishing and reading.[84]

I claim, then, that the new periodicals devoted chiefly, and in some cases solely, to natural knowledge brought into being a community of investigators into Nature—which we can call, for convenience, a natural-philosophical community. That community, however, was very different from the scientific community which would come to succeed it in the nineteenth century. Thus David Cahan is both right and wrong to assert that "there was no identifiable

[81] Brendan Dooley, *Science and the Marketplace in Early Modern Italy*, Lanham, Maryland: Lexington Books, 2001, p. xiii.

[82] James A. Secord, "Knowledge in transit," *Isis* 95:4 (2004), pp. 654-72. See also Jonathan R. Topham, "Scientific readers: a view from the industrial age," *Isis,* 2004, 95:3, pp. 431-42.

[83] James A. Secord, *Victorian Sensation: The extraordinary publication, reception, and secret authorship of Vestiges of the natural history of creation*, Chicago: University of Chicago Press, 2000; Ruth Richardson, *The Making of Mr Gray's Anatomy: Bodies, Books, Fortune, Fame,* Oxford: Oxford University Press, 2008.

[84] Alan G. Gross, Joseph E. Harmon and Michael S. Reidy, *Communicating Science: The scientific article from the 17th century to the present,* Oxford: Oxford University Press, 2002; Adrian Johns, *The Nature of the Book: Print and Knowledge in the Making*, Chicago: University of Chicago Press, 1998; Ann Blair, "An early modernist's perspective," *Isis* 95:3 (2004), pp. 420-30.

scientific community before the early nineteenth century."[85] By the same token, it would be a mistake to equate the eighteenth-century natural-philosophical community with the so-called "republic of letters."[86] Indeed the nature of that community is precisely what needs to be explored.

Internationalism: This, the second of our social themes, hardly needs elaboration here, in view of what has just been said about the academies and periodicals. It emerged in the section on electricity, but could equally have been illustrated from chemistry or from the physico-mathematical sciences, or indeed from any field of eighteenth-century natural enquiry. Perhaps the most spectacular example was botany, which, though increasingly dominated by Linnaeus, was probably pursued in every European country. In other fields the picture was more patchy; I know of no attempt to map activities against polities, and would recommend this as a potentially fruitful line of enquiry.

Social roles: The third theme under consideration here is the *diversity of social roles* of those engaged in natural knowledge, which I illustrated with the team that carried out the investigations that led to the discovery of the aberration of light—Molyneux the wealthy amateur, Graham the instrument-maker, Bradley the clergyman-professor. Such diversity is well known and needs no stressing here: even mathematics, the most technical field and for that reason dominated by such salaried academicians as d'Alembert and Euler, had room for a major development in probability theory to be produced by a Presbyterian minister, Thomas Bayes of Tunbridge Wells. But while the phenomenon is familiar, its significance seems to await interrogation. An important aspect of that significance, I suggest, is that there was no clear boundary between those who produced natural knowledge and those for whom they produced it. It is hard to capture the social processes at work, because the terminology that we are apt to use obscures the very phenomena that need elucidation: this is true not just of "science" and "scientist" but also of "audience," which subtly inserts a barrier and insinuates an inappropriate passivity. In fact, I suggest that the wider public played far more of a constitutive role in the eighteenth-century generation of natural knowledge than has yet been appreciated. Certainly what currently serves as the standard essay on "the forms, sites, and social meanings of natural knowledge in the eighteenth century" has doubly blocked off

[85] David Cahan, "Looking at nineteenth-century science: an introduction," in Cahan (ed.), *From Natural Philosophy to the Sciences: Writing the history of nineteenth-century science*, Chicago: University of Chicago Press, 2003, pp. 3-15, at p. 11.

[86] As was assumed by Lorraine Daston, "The ideal and reality of the Republic of Letters," *Science in Context* 2 (1991), pp. 367–86. That tricky term seems to have meant different things in different polities (see Kasper Risbjerg Eskildsen, "How Germany left the republic of letters," *Journal of the History of Ideas* 65:3 [2004], pp. 421-32), and its history was quite different in France, Britain and Germany, as can readily be established using Google Ngram Viewer.

this theme—by saying nothing about either the *content* or the *genesis* of such knowledge, instead focusing solely upon its popularisation.[87]

One way to bring out the importance of this theme is to consider the activities of the instrument-makers. Recent historiography has rightly drawn attention to the skills and significance of these men (I know of no female examples),[88] but there is surely much more to be discovered about their role, or roles, in the remarkable advances that took place.[89] The exchanges between Fahrenheit and Boerhaave are, by great good fortune, partly documented, and that documentation is highly revealing; those between first Graham and Molyneux, then Graham and Bradley, seem to have left no known trace. What is clear is that in London, Paris and Leyden, and surely elsewhere as well, there had already developed by 1700 a flourishing market for instruments—or rather, at least two markets, one amongst the virtuosi, another for a wider public, requiring very different marketing strategies.[90] Further, as is well known, there was a substantial market for both lectures and books on experimental philosophy: emblematic examples are Harris's *Lexicon Technicum* of 1704, Chambers's *Cyclopaedia* of 1728, and the fact that after his expulsion from Cambridge in 1710, William Whiston was able to earn a living by lecturing in London on natural philosophical topics. What is more, both Larry Stewart and Michael Hunter have given us reason to suspect that the wider audience may well have played a constitutive role in some early eighteenth-century natural-philosophical developments.[91] In sum, the social matrix within which

[87] Mary Fissell and Roger Cooter, "Exploring natural knowledge: science and the popular," in Roy Porter (ed.), *Cambridge History of Science, Volume 4*, pp. 129-58, at p. 131. For instance, so-called "Newtonianism" is wholly black-boxed (pp. 134-9), as is the botanical system of Linnaeus (pp. 152-3).

[88] Lissa Roberts, Simon Schaffer, and Peter Dear (eds.), *The Mindful Hand: Inquiry and Invention from the Late Renaissance to Early Industrialization*, Amsterdam: Koninklijke Nederlandse Akademie van Wetenschappen, 2007.

[89] Of the most eminent Paris instrument-maker of the early eighteenth century, Nicholas Bion, it has been said that "Despite the relatively high number of instruments carrying Bion's name to have survived, and their wide range, we know astonishingly little about the operation of his trade." Anthony Turner, "Nicolas Bion, globe-maker, instrument-maker, author and businessman," *Globe Studies* 59/60 (2014), pp. 198-218, at p. 209.

[90] Jeffrey R. Wigelsworth, "Bipartisan politics and practical knowledge: advertising of public science in two London newspapers, 1695–1720," *The British Journal for the History of Science* 41:4 (2008), pp. 517-40. Neither Tompion nor Graham is mentioned as advertising; Hauksbee is, but only for cupping-glasses, not for philosophical instruments. On the other hand, in 1770 Edward Nairne, very much of the elite caste, had a trade card: see Paola Bertucci, "A philosophical business: Edward Nairne and the patent medical electrical machine (1782)," *History of Technology* 23 (2001), pp. 41–58, at p. 48.

[91] Larry Stewart, "Other centres of calculation, or, where the Royal Society didn't count: commerce, coffee-houses and natural philosophy in early modern London," *The British Journal for the History of Science* 32:2 (1999) [*Did the Royal Society Matter in the Eighteenth Century?*],

natural investigations took place was exceedingly complex and diverse, and this very complexity was itself probably fruitful, despite its seemingly chaotic character.

Technical themes

These themes appeared in the sequence (i) precision, (ii) instruments and (iii) accidental discovery; again, I shall discuss them in a different order.

Instruments: It is well known that the eighteenth century was the era of the instrument, and this has been exemplified here not only in chemistry (the specific field from which I drew this theme) but also in the physico-mathematical sciences and in the study of electricity. In all those domains, and in many others as well, instruments opened up a new world. The simple device that came to be called the pneumatic trough transformed chemistry; Graham's telescope, along with Bradley's skill both in using it and in interpreting the results, brought about the first decisive proof of the Earth's motion; Hauksbee's glass rod amplified electrical effects, so that eventually, in Gray's hands, they began to become amenable to study. Notice that in every case the story was not *only* about instruments, but also, and decisively, about the ways in which those instruments were used, and thus (to spell out the obvious) about the aims, interests and presuppositions of the individuals who were using them. Instruments alone, then, did not bring about progress; but they were essential to such progress. And as Richard Sorrenson's paper in the present special issue shows, many of the instruments themselves progressed – that is, were improved – in the period.

Further, I suggest that instruments were but one particular form (albeit a very important one) of a wider pattern that prevailed throughout eighteenth-century natural investigations: that is, the use of what Bacon called "helps" for the more effective apprehension of Nature. Here are three further examples—the first, which has already been mentioned, an ancient one, the other two new in the eighteenth century. (i) As we saw in connection with Bradley's discovery of the aberration of light, the collation of sequential observations had been the practical basis of astronomy since its beginnings in the ancient world. Such collation makes apparent, as direct observation cannot, the movements of stars and planets. (ii) A classification table, such as Linnaeus's *Systema Naturae* of 1735, created a new object, one that was not and never could be perceptible in Nature: the assembly of kindred (or notionally kindred) species. Such an assembly could also be realised physically both

pp. 133-53; Michael Hunter, "The Royal Society and the decline of magic," *Notes and Records of the Royal Society* 65:2 (2011), pp. 103-19.

by the herbarium and by the suitably-ordered botanical garden, but its most potent form—for reasons of both comprehensiveness and portability—was on the printed page. (iii) Anatomy was transformed by the use of permanent preparations—an invention of the late seventeenth century, but very much advanced, indeed in a sense perfected, in the eighteenth.[92] Such preparations played an essential part in William Hunter's two great discoveries, the function of the lymphatic system and the nature of the placenta; and they were even more important as the material basis from which Hunter's nephew Matthew Baillie constructed his *Morbid Anatomy* of 1793, the work which initiated a new, empirical discipline of pathology.[93] By turning the ephemeral (the rapidly-decomposing cadaver), or rather part of it, into something that could be studied at leisure (a specimen preserved in diluted "spirit of wine," that is, alcohol), preparations made diseased appearances available for study in a way that was simply impossible for, say, Valsalva or Morgagni earlier in the century.

Each of these transformations—from the individual species to the assembly of kindred species, from the ephemeral cadaver to its preserved parts—was precisely equivalent to that wrought by Hauksbee's glass rod or Fahrenheit's thermometer. Such transformations often stemmed from seventeenth-century practices (as for instance with thermometers and anatomical preparations), but typically achieved effective practical realisation only in the eighteenth century. The uniformity of this development across a vast number of disparate fields is concealed by the diversity of its forms: at first glance, an anatomical preparation, a glass rod and a botanical table have nothing in common. But that, of course, was the very strength of this process, for each transformation was fitted to its object. And taken together, the vast suite of such transformations amounted to a change of just the kind that historians like to call a "revolution."

Precision: Bradley's heroic discovery shows that in telescopic astronomy, precision was already sought and achieved by the 1720s; as is well known, both electricity and chemistry became fields of precise measurement in the late eighteenth century. Such precision, though in many cases modest by the higher standards that developed in the nineteenth century, was still a heroic achievement, always involving both skill and ingenuity, and often (as we have seen both with Bradley / Graham and with Fahrenheit / Boerhaave) the complex collaboration of natural philosopher and instrument-maker. It may be suspected that

[92] Andrew Cunningham, *The Anatomist Anatomis'd: An Experimental Discipline in Enlightenment Europe*, Farnham: Ashgate, 2010, pp. 231-5.

[93] Richard T. Bellis, "'The object of sense and experiment': the ontology of sensation in William Hunter's investigation of the human gravid uterus," *The British Journal for the History of Science* 55 (2022), pp. 227–246, at pp. 241-4; idem, "Making anatomical knowledge about disease in late Georgian Britain, from dissection table to the printed work and beyond: Matthew Baillie's *Morbid Anatomy* and its accompanying engravings," PhD thesis, University of Leeds, 2019.

such precision had its analogues in domains not involving measurement: for instance in chemistry, purity of ingredients and control of processes may well have increased in our period, but this is a possibility I have not been able to test.[94] But what can be said is that the increase of precision is intelligible as an outcome of widespread and repeated observational and experimental investigations, for the simple reason that Nature sometimes—often enough to serve as an inducement to the ambitious—rewards such precision. (It is suggestive that the use-frequency both of "precision" and of "accuracy" increased tenfold between the 1740s and the 1780s, according to Google Ngram Viewer.)

Accidental discovery: Although accidental discoveries are most easily evidenced from electricity, they were characteristic of experimental investigations in general. Yet this feature of eighteenth-century investigations has received remarkably little attention, and even when it is noticed, historians have a tendency to shy away from it. Here is a striking example. In Giuliano Pancaldi's highly-regarded 2003 book on Volta, the conclusion is entitled "Science, Technology and Contingency," and Pancaldi summarises its argument thus: "Diversity and contingency were just as important as the Enlightenment ideal of 'useful knowledge' and the 'quantifying spirit' in bringing about the battery."[95] Yet of the nine reviews of the book that I have found, four did not mention contingency at all, four mentioned it briefly, and just one review foregrounded it.[96] A rigorous theorisation of accidental discovery is long overdue;[97] I shall make just two observations. First, there is every reason to expect accidental discoveries in the process of experimental investigation, for the simple reason that whatever is being investigated cannot—by definition!—yet be fully understood. Thus the mere fact of widespread experimentation, which certainly took place in the eighteenth century on an unprecedented scale, more or less guaranteed frequent *opportunities for* accidental discovery. Second, the *exploitation* of such opportunities required something more, namely appropriate alertness on the part of the investigator. Roderick Home has kindly supplied for me an excellent example: Gray's discovery of electrical conduction—a giant moment in the history of electricity—began from his noticing the anomalous

[94] Some hints to this effect can be discerned in Ursula Klein, "Objects of inquiry in classical chemistry: material substances," *Foundations of Chemistry* 14:1 (2011), pp. 7-23, at p. 17.

[95] Giuliano Pancaldi, *Volta: Science and Culture in the Age of Enlightenment,* Princeton: Princeton University Press, 2003, p. 285.

[96] The heroic exception, namely Massimo Mazzotti (in *Technology and Culture* 45:2 [2004], pp. 420-1), perhaps confirms the rule, in that he has also (though several years later) collaborated with Pancaldi (as co-editor of *Impure Cultures: Interfacing Science, Technologies, and Humanities*, Bologna: Università di Bologna, 2010).

[97] Much the most sensitive discussion I have found is Nahum Kipnis, "Chance in science: the discovery of electromagnetism by H.C. Oersted," *Science & Education* 14:1 (2005), pp. 1-28.

fact that an electrified glass tube had passed its electrification to its cork stopper, contradicting existing theory. Here, I suggest, we have in microcosm what was in fact a vast and widespread process: not "testing" a theory by rational "deduction," but on the contrary bumping up against the surprises—in electricity, the shocks—that Nature offered. And we can sum up these two points, the opportunities and their exploitation, under the working slogan that accidents are (or were) no accident.

Conclusion

I conclude by indicating two of the limitations of what has been proposed here.

First, there is at least one respect in which the present argument leaves what Cantor called the "eighteenth-century problem" wholly untouched. I refer to the dual question as to the identity and trajectory of natural philosophy—a topic which was a major theme of Cantor's essay, which had already in the early 1980s long haunted the historiography and has done so ever since, and which remains entirely unresolved. As to natural philosophy's *identity*, it has proved easier to say what it was not—that is, it was not science—than to specify what it was. Most historians would probably agree that amongst its defining characteristics was a lack of clear boundaries between what we are apt to see as distinct fields (for instance chemistry and geology), and a corresponding lack of specialisation on the part of its leading practitioners; many would probably accept that natural philosophy was the study of the *created* world, as distinct from an impersonal Nature, and perhaps too that in this respect, an atheist like d'Holbach or Laplace was the exception confirming the rule; but such scattered points as these do not begin to give us the kind of conceptual map that the topic surely requires. The most interesting suggestion I know of, which was articulated by Simon Schaffer both in the *Ferment* volume and in a subsequent essay, is that "wonder" was constitutive of eighteenth-century natural philosophy;[98] this claim, long neglected and sometimes entirely overlooked,[99] has more recently been handsomely vindicated and enriched by Bycroft's work on Dufay.[100]

[98] Simon Schaffer, "Natural philosophy," in Rousseau and Porter (eds.), *Ferment of Knowledge*, pp. 55-91; idem, "Natural philosophy and public spectacle in the eighteenth century," *History of Science* 21 (1983), pp. 1-43.

[99] By Lorraine Daston and Katharine Park, *Wonders and the Order of Nature*, New York: Zone Books, 1998, which did not cite either of those Schaffer papers, and downplayed the role of wonder in eighteenth-century natural investigations.

[100] Michael Bycroft, "Wonders in the Academy: The value of strange facts in the experimental research of Charles Dufay," *Historical Studies in the Natural Sciences* 43:3 (2013), pp. 334-70.

Further difficulties arise in respect of natural philosophy's historical *trajectory*, a topic which has proved highly elusive. The term of course was of medieval origin (the *OED* records it from 1393), but came to have a new, more empirically-informed inflection in the seventeenth century; yet its real heyday was the eighteenth century, when the usage of the phrase had leapt tenfold (judging by Google Ngram Viewer) by 1750. We need to ask, therefore, whether eighteenth-century natural philosophy differed from its seventeenth-century predecessor, and if so, in what ways; and there is an even more pressing question as to how—and indeed precisely when—it was subsequently transmuted into what came to be called "science."[101] An indication of the inherent difficulty of this narrative question is the fact that it has been elided by most of the writing on natural philosophy's *identity*.[102] Indeed I know of only two serious attempts to tackle that narrative question: by Heilbron in the *Ferment* volume, and by Schuster and Watchirs in 1990.[103] Both of these essays focus specifically on experimental natural philosophy (thereby bracketing off natural history); both take electricity as their exemplar (thereby omitting, for instance, optics and chemistry); and both take their cue from Kuhn, but in very different ways. Heilbron's story was based on, and provided a very helpful summary of, his then-recent *Electricity* book, whose governing frame had been taken from Kuhn's classic essay "Mathematical vs. experimental traditions";[104] that story was one of experimental physics happily freeing itself from the distorting trappings of such characteristically natural-philosophical themes as the theory of matter.[105] Here a

[101] For a different suggestion—that natural philosophy never died, but rather remains part of what we call science—see Peter Dear, "What is the history of science the history *of*? Early modern roots of the ideology of modern science," *Isis* 96:3 (2005), pp. 390-406.

[102] Schaffer, "Natural philosophy"; Andrew Cunningham, "How the *Principia* got its name; or, taking natural philosophy seriously," *History of Science* 29:4 (1991), pp. 377-92; Cantor, "The eighteenth-century problem." Nevertheless some hints are to be found in Geoffrey Cantor, *Optics after Newton: Theories of Light in Britain and Ireland, 1704-1840*, Manchester: Manchester University Press, 1983, and in Schaffer's papers "Natural philosophy and public spectacle in the eighteenth century" and "Scientific discoveries and the end of natural philosophy," *Social Studies of Science* 16:3 (1986), pp. 387-420.

[103] John L Heilbron, "Experimental Natural Philosophy," in Rousseau and Porter (eds.), *Ferment of Knowledge*, pp. 357-87; John Schuster and Graeme Watchirs, "Natural philosophy, experiment and discourse in the eighteenth century: beyond the Kuhn/Bachelard problematic," in H. E. LeGrand (ed.), *Experimental Inquiries: Historical, philosophical and social studies of experiment*, Dordrecht: Reidel, 1990, pp. 1-48, at p. 30. True, the question is also tackled by Stephen Gaukroger's *The Collapse of Mechanism and the Rise of Sensibility: Science and the Shaping of Modernity, 1680-1760*, Oxford: Oxford University Press, 2010, but from within the frame of an enquiry into the origins of scientific authority.

[104] Thomas S. Kuhn, "Mathematical vs. experimental traditions in the development of physical science," *The Journal of Interdisciplinary History* 7:1 (1976), pp. 1-31.

[105] As Cantor's review of *Ferment* pointed out: Cantor, "The eighteenth-century problem," pp. 52-3 and especially 58-9.

splendid narrative coherence was achieved, but at the very high price of effectively assimilating natural philosophy to science. But Schuster and Watchirs took a very different approach, distancing themselves from Kuhn's two-traditions paper both by showing that it was in tension with his *Structure of Scientific Revolutions* and by creatively counterposing the work of Gaston Bachelard. Further, they anchored eighteenth-century natural philosophy in the immediately-preceding developments, drawing on Schuster's earlier account of the "Scientific Revolution" as a process,[106] and extending that process into the eighteenth century. This enabled them to put forward a "model" of what they called the "dynamics of experimental natural philosophy," a model which claimed to account for the fragmentation of that field into separate fields of enquiry. It is much to be regretted that few scholars have noticed that outstanding paper; any future enquiry into either the trajectory or the identity of eighteenth-century natural philosophy will have to take this as its starting-point.[107]

Second, the question needs to be asked: how literally does this paper deploy Bacon's phrase "The Great Instauration"? It is convenient to distinguish between what I shall call capital-B Baconianism and small-b baconianism. Capital-B Baconianism means a conscious implementation of Bacon's programme; baconianism with a small "b" signifies a vindication of Bacon's vision. Although I suspect that further enquiry may well reveal a good deal more eighteenth-century capital-B Baconianism than is commonly acknowledged, the argument advanced here has been limited to small-b baconianism. Of this I shall give just three examples. (i) It was fundamental to Bacon's vision that future knowledge-making would be not individual but collective; and this is precisely what characterised not only the fields of enquiry discussed here but also all the others. (Even such a seeming individualist as William Herschel turns out to have been anchored in the Bath Philosophical Society,[108] and of course to have depended on his sister Caroline; at the opposite end of the spectrum one might instance Linnaean botany, which derived its efficacy precisely from its collective character.[109]) (ii) The same applies to the means by which this was achieved. The institutional settings of eighteenth-century investigations of Nature—a messy and internationally-diverse congeries of societies, academies,

[106] John A. Schuster, "The Scientific Revolution," in R. C. Olby, G. N. Cantor, J.R.R. Christie, and M.J.S. Hodge (eds.), *Companion to the History of Modem Science*, Routledge, 1990, pp. 217-42.

[107] As can easily be achieved, as it's on Academia.edu.

[108] Simon Schaffer, "Herschel in Bedlam: Natural History and Stellar Astronomy," *The British Journal for the History of Science* 13:3 (1980), pp. 211-39.

[109] Lisbet Koerner, *Linnaeus: Nature and Nation*, Cambridge, Mass.: Harvard University Press, 2001; Bettina Dietz, "Aufklärung als Praxis. Naturgeschichte im 18. Jahrhundert," *Zeitschrift für Historische Forschung* 36:2 (2009), pp. 235-57.

universities, didactic settings, private businesses and State institutions—were a far cry from the carefully-regimented structures of "Solomon's House." And yet their effect was just what Bacon had intended "Solomon's House" to achieve: the transformation of knowledge-making from an individual process to a collective one, with massive consequent gains in every field. (iii) So too the investigations themselves seem not to have been governed by the precise procedures set out in *The Advancement of Learning* (tables of instances, of absences, and so forth), yet the reason they were so widely successful was precisely in line with Bacon's core methodological insight, namely the need to adapt one's questions in the light of Nature's responses—what Sophie Weeks has aptly termed the "cybernetic" aspect of Bacon's epistemological strategy.[110] This fits both the widespread phenomenon of "accidental" discoveries and, at the opposite extreme, the gradual refinement of technique by those investigators (such as Bradley, Cavendish, Lavoisier and Herschel) who appreciated that Nature rewarded precision, who acted accordingly, and who reaped appropriately rich philosophical rewards.

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[110] Sophie Weeks, "The role of mechanics in Francis Bacon's great instauration," in Claus Zittel (ed.), *Philosophies of Technology: Francis Bacon and his Contemporaries,* 2 vols., Leiden: Brill, 2008, pp. 131-95.

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