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RESEARCH ARTICLE

# A neo-positivist theory of scientific change

Michael Bycroft

History Department, University of Warwick, UK
Email: m.bycroft@warwick.ac.uk

## Abstract

Historians of science appear to agree on two things. There is a shortage of large-scale histories of science, and positivism is best avoided. In fact, we have many big-picture histories of science. The problem is not the lack of such histories but the lack of agreement between them. They differ with respect to chronology, geography, narrative structure, favoured disciplines, recent revisionism and epistemology. To make the most of these differences, I resurrect an idea from nineteenth-century positivism, namely that science evolves by the migration of methods from one matter to another. This is an old form of materialism that complements more recent materialisms. The neo-positivist approach may be illustrated by matters as varied as stars, crystals and the Pacific Ocean. If we revive positivism as an intellectual project, we might also revive the social goal of positivism, which was to use the history of science to make the world more rational. A present-day version of this project is to use the history of science to defend the humanities as a rational enterprise.

Where have all the big pictures gone? Historians of science have been asking this question for three decades, usually in an elegiac tone.[1] But we might also ask: where will we find the time to read all the big pictures we now have?

The 2020s have barely begun, yet they have already produced at least three ambitious accounts of past science by professional historians.[2] For the 2010s, a list of big-picture histories of science would include works by Floris Cohen, Chunglin Kwa, David Knight, David Wootton, Stephen Gaukroger, Frans van Lunteren and Lisa Jardine, among others.[3] To

1 James A. Secord, 'Introduction', *BJHS* (1993) 26(4), pp. 387–9. Robert Kohler, 'A generalist's vision', *Isis* (2005) 96(2), pp. 224–9; and the articles by Steven Shapin, Paula Findlen and David Kaiser in the same issue; Lorraine Daston, 'Science studies and the history of science', *Critical Inquiry* (2009) 35(4), pp. 798–813, 809; Nicholas Jardine and Emma Spary, 'Worlds of history', in Helen A. Curry, Nicholas Jardine, James A. Secord and Emma Spary, *Worlds of Natural History*, Cambridge: Cambridge University Press, 2018, pp. 3–14, 11–12.

2 Ofer Gal, *The Origins of Modern Science: From Antiquity to the Scientific Revolution*, Cambridge: Cambridge University Press, 2021; Sebastian Falk, *The Light Ages: A Medieval Journey of Discovery*, London: Penguin, 2021; James Poskett, *Horizons: A Global History of Science*, London: Penguin, 2022.

3 Floris H. Cohen, *How Modern Science Came into the World: Four Civilizations, One 17th-Century Breakthrough*, Amsterdam: Amsterdam University Press, 2010; Chunglin Kwa, *Styles of Knowing: A New History of Science from Ancient Times to the Present*, Pittsburgh: University of Pittsburgh Press, 2011; David Knight, *Voyaging in Strange Seas: The Great Revolution in Science*, London: Yale University Press, 2014; David Wootton, *The Invention of Science: A New History of the Scientific Revolution*, London: Penguin, 2015; Stephen Gaukroger, *The Natural and the Human: Science and the Shaping of Modernity, 1739–1841*, Oxford: Oxford University Press, 2016; Frans van

these we can add a steady stream of guides, textbooks, companions and dictionaries, not to mention *longue durée* histories of individual disciplines, from chemistry to crystallography. Even in the 1990s, when we began to worry about our overcommitment to microhistories, there were several new macrohistories on the table.[4] And this is before we consider anything published in languages other than English.

What is going on? Why do we worry so much about our big-picture deficit and so little about our big-picture excess? The short answer is 'the scientific revolution'. The problem is not that the scientific revolution is a myth. The theory that modern science was invented in seventeenth-century Europe is as good as any other theory we have about the origins of science. The problem is that this theory has become a fixation for both critics and advocates. The critics have offered a long line of alternatives to the scientific revolution: the second scientific revolution, the modern origins of science, mechanical objectivity, Old Regime science, the enlightened Middle Ages, global cultural exchange and so on. Meanwhile, advocates of the scientific revolution have tried to resuscitate the original idea. Yet the two sides disagree as much amongst themselves as they do with their putative opponents. David Wootton's defence of the scientific revolution is very different from Floris Cohen's defence. Indeed, in some respects, Wootton's defence has more in common with John Pickstone's critique than it does with Cohen's defence. Similarly, James Poskett's critique of the scientific revolution may be read as a critique of the Eurocentrism of the other critiques.[5] What I am suggesting is that this proliferation of big pictures has gone unnoticed because the idea of the scientific revolution still looms so large in our collective unconscious, whether as a tradition we need to defend or as a myth we need to debunk. We mentally parse our big pictures into 'debunkers' and 'defenders'. In doing so, we overlook the similarities between the two camps and the differences within each one.

The first step out of this impasse is to be frank about the differences between existing accounts. There are at least six bones of contention. One is geographical. Has Europe played a distinctive role in modern science? If so, what was that role? If not, what was the geographical pattern of past science? These questions are especially charged at the moment, but they should not obscure other points of disagreement. It has been argued that science was invented around 1200, around 1650, around 1800, around 1850, in some combination of these periods, and in no period in particular. More confusingly still, some scholars choose one discipline and use it as model for the periodization of other disciplines. Physics, medicine and technology have all been proposed, with divergent results. Next, there is the subtle but important question of how to organize a narrative about the history of science – as a sequence of epistemes, a convergence of traditions, a series of world-historical events, or in some other way?

These questions are hard enough to answer on their own. They get harder when we see that they are aligned with disagreements about recent changes in our field. James A. Secord gave one perspective in his introduction to a special issue of this journal published in 1993. Secord gave a glowing account of what he called 'the revisionism of the

Lunteren, 'Clocks to computers: a machine-based "Big Picture" of the history of modern science', *Isis* (2016) 107 (4), pp. 762–76. Note also Lisa Jardine's *Seven Ages of Science*, a BBC Radio 4 series that aired in 2013.

4 David Lindberg, *The Beginnings of Western Science: The European Scientific Tradition in Philosophical, Religious, and Institutional Context, 600 B.C. to A.D. 1450*, Chicago: The University of Chicago Press, 1992; Roy Porter and Mikuláš Teich, *The Scientific Revolution in National Context*, Cambridge: Cambridge University Press, 1992; Alistair Crombie, *Styles of Scientific Thinking in the European Tradition: The History of Argument and Explanation Especially in the Mathematical and Biomedical Sciences and Arts*, 3 vols., London: Duckworth, 1994; Floris H. Cohen, *The Scientific Revolution: A Historiographical Inquiry*, Chicago: The University of Chicago Press, 1994.

5 The works I have in mind are Wotton, op. cit. (3); Poskett, op. cit. (2); Cohen, op. cit. (3); John Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine*, Manchester: Manchester University Press, 2000.

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new history of science': 'After years of expert demolition by specialists, the established stories in the field – from the origins of science in ancient Greece to the Darwinian and Einsteinian "revolutions" – are in ruins.'[6] Compare John Heilbron's pointed remarks, also in this journal, about a recent textbook:

> The fundamental problem with [the book under review] is that its authors are caught up in the tight, re-entrant, Anglo-American whirlpool of 'modern historians' of science. These anonymous wizards have exposed as naive or disingenuous scientists' core belief that science pursues true factual knowledge about the world.[7]

These quotes may seem to refer to different things: the first one is about the origins of science, the second about the claims of science to truly represent the world. But these two topics – call them 'history' and 'epistemology' – are hard to separate. They are both related to the historiographical question of how we should evaluate recent revisionism in the history of science, whether positively (Secord) or negatively (Heilbron). One name for the revisionism in question is 'constructivism'. Turning to the standard work on constructivism in the history of science, by Jan Golinski, we find that history and epistemology are mixed up there as well. The premise of the book is that we no longer believe in narratives of scientific progress (history) because we no longer believe that science is a reflection or revelation of the truth about the natural world (epistemology).[8] This premise lies behind at least one argument against the traditional story of the scientific revolution; the rejection of the premise lies behind at least one defence of that story.[9] We can add epistemology to the list of things we disagree about, alongside geography, chronology, disciplines, narrative technique and recent revisionism.

These differences may seem insurmountable. In a sense, they are: historians of science will never agree on everything, and nor should they. But if we cannot eliminate disagreement, we can at least make it productive. We can ask whether there are any approaches to the history of science that give us some hope of synthesizing or reconciling the existing big pictures. I shall argue that there is such an approach, one that comes from a surprising source: the positive philosophy of the nineteenth-century French thinker Auguste Comte. The basic idea is that science evolves through the migration of methods from one matter to another. This may be thought of as a variety of materialism. Specifically, it is a counterpart in the history of science to recent histories of natural resources written by economic and political historians. The Comtean approach may be illustrated by the study of crystals, stars and the Pacific Ocean, all of which were studied in new ways by European savants in the eighteenth century. In each case, the new methods were variants of methods that had already been applied to other matters, such as plants, planets and the Atlantic Ocean. After giving these examples, I return to the disagreements discussed above and sketch out a plan for resolving them based on the neo-positivist theory of science. Because positivism was a social programme as much as an intellectual one, I end by making the case for an updated version of Comte's programme, one that would use the history of science to defend the humanities as a rational enterprise.

6 Secord, op. cit. (1), 388.

7 John Heilbron, review of Peter Bowler and Iwan Rhys Morus, *Making Modern Science: A Historical Survey*, *BJHS* (2007) 40(1), pp. 118–19, 119.

8 Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science*, 2nd edn, Chicago: The University of Chicago Press, 2005, pp. 3–5, 187–8.

9 Respectively Andrew Cunningham and Perry Williams, 'De-centring the "big picture": *The Origins of Modern Science* and the modern origins of science', *BJHS* (1993) 26(4), pp. 407–32, 411, 413–14, 415; and Wootton, op. cit. (3), *passim*.

## Comte's materialism

Matter is back. After years of learning that 'there is nothing outside the text', we are now learning that 'nature is agentic'.[10] The 'New Materialism' has reminded us that the material world makes a difference to human life and that it therefore needs to be included in accounts of the human past. This is a general movement in the humanities that has obvious parallels in the history of science. We study the hardware of science, we link science to money and power, we replicate past experiments, we collaborate with museum curators, we talk to environmental scientists, and so on. One might wonder how new all this is: Ernst Mach called for the study of the material heritage of science way back in 1883, in his *Science of Mechanics*; Francis Bacon had much to say about instruments and collections in his *Great Instauration*, first published in 1620. Still, there is no escaping from academic fashions. Those of us who are interested in big-picture histories of science need to engage with the New Materialism, even if it is not as new as it appears.

Historians of natural resources have shown how this can be done. They have written ambitious and accessible histories based on the idea that different resources lend themselves to different kinds of political organization. 'Because it is bulky and requires many men to move it around, coal was a catalyst for democracy and progress', to quote a high-profile review of Timothy Mitchell's *Carbon Democracy*, a pioneering work in the genre.[11] The history-of-science equivalent is to replace 'natural resources' with 'the subject matter of science' and 'political organization' with 'the methods of science'. In other words, the methods of science change when they are transferred from one matter to another. They change, at least in part, because of the materiality of the matter. The methods of astronomy changed when they were transferred from planets to the stars, because stars are different from planets; the methods of classification changed when they migrated from plants to animals, because plants are different from animals; and so on. Ways of knowing depend on the things known. New things lead to new ways of knowing.

This is not a new way of thinking about science.[12] The key modern proponent of the idea was Auguste Comte, the inventor of what we now call 'positivism' and what he called 'la philosophie positive'. Of course, positivism has a dismal reputation among historians of science today. Although no one agrees on what the big picture is, nearly everyone seems to agree on what it must not be – it must not be positivist.[13] New Materialists, too, see positivism as something to dodge rather than something to embrace.[14] But there are signs that historians and sociologists of science are getting over their knee-jerk opposition

10 'Il n'y a pas de hors-texte' (Jacques Derrida). Diana Coole and Samantha Frost, 'Introducing the new materialism', in Diana Coole, Samantha Frost, Jane Bennett, Pheng Cheah, Melissa A. Orlie and Elizabeth Grosz (eds.), *New Materialisms: Ontology, Agency, and Politics*, Durham, NC: Duke University Press, 2010, pp. 1–44, 5.

11 Susanna Rustin, '*Carbon Democracy: Political Power in the Age of Oil* by Timothy Mitchell', *The Guardian*, 29 December 2015, at www.theguardian.com/books/booksblog/2015/dec/29/carbon-democracy-political-power-in-the-age-of-oil-by-timothy-mitchell (accessed 23 March 2023); Timothy Mitchell, *Carbon Democracy: Political Power in the Age of Oil*, London: Verso, 2011. See also Alexander Etkind, *Nature's Evil: A Cultural History of Natural Resources*, Cambridge: Polity Press, 2021.

12 For other statements of the idea see Crombie, op. cit. (4), vol. 1, pp. 46–7, 59, 83–8, 93, 229–38. The latter passage traces the idea to Aristotle.

13 Jardine and Spary, op. cit. (1), 11, though this is nuanced at 8–9; Paula Findlen, 'The two cultures of scholarship?', *Isis* (2005) 96(2), pp. 230–7, 236; David Kaiser, 'Training and the generalist's vision in the history of science', *Isis* (2005) 96(2), pp. 244–51, 244, 245; Daston, op. cit. (1), 812.

14 Coole and Frost, op. cit. (10), 7; Susan Yi Sencindiver, 'New materialism' (2017), in *Oxford Bibliographies* (Oxford: Oxford University Press), doi: 10.1093/obo/9780190221911-0016; John H. Zammito, 'Concluding (irenic) postscript', in Sarah Ellenzweig and John Zammito (eds.), *The New Politics of Materialism: History, Philosophy, Science*, New York: Routledge, 2017, pp. 300–21, 303.

to Comte's philosophy.[15] Meanwhile, historians of philosophy have written sympathetic reappraisals of Comte and of his nineteenth- and twentieth-century successors.[16] The time is ripe to revive Comte's idea that methods change in response to matters.

Comte gave a general statement of the idea in the first volume of the *Course of Positive Philosophy*, published in Paris in 1830. The idea flowed from the fundamental principle of the positive philosophy, which was that science ought to describe the phenomena rather than discover the causes of the phenomena. It followed that the sciences should be classified according to the phenomena they describe. The phenomena ranged from the most simple, general and abstract phenomena (the motions of the planets) to the most complex, specific and concrete phenomena (the social behaviour of humans). This principle yielded five fundamental sciences: astronomy, physics, chemistry, biology and sociology. Crucially, Comte insisted that each science had distinctive methods that depended on its distinctive subject matter. Astronomers made observations but did not do experiments, physicists did experiments but did little in the way of classification, sociologists did history but not mathematics, and so on.[17] This was a historical point as well as a philosophical one, since Comte believed that the sciences had evolved in order of the complexity of their phenomena. Astronomy was the first science to become positive; the method was then extended to physics, chemistry, physiology and sociology, in that order. As the method moved from one discipline to another, it changed or 'adapted' (Comte's word) to meet the demands of the phenomena of each new discipline.[18] There was a similar pattern within individual sciences: the physics of weight became positive before the physics of electricity, for example.[19] For Comte, the history of science was the history of the adaptation of the positive method to new matters.

As it stands, this theory will not do. It is just too positivistic – too reliant on a rigid hierarchy of the disciplines, too committed to a narrow view of scientific method, too abstract in its characterization of 'simple' or 'complex' phenomena. But this is no reason to reject the core idea that methods change when they are transferred to new matters. In fact, Comte gives us a good reason to retain that idea: he rarely put it into practice as a historian. Although he implied, for example, that physics emerged from the adaptation of the methods of astronomy to terrestrial phenomena, he did not describe in any detail how this happened. The *Course of Positive Philosophy* was primarily a work of philosophy, not of history, as Comte himself admitted.[20] This rather static picture of scientific method persists in the work of Ian Hacking, a recent champion of Comte, and in the work of Alistair

15 Jardine and Spary, op. cit. (1), 8–9; Harold Cook, 'Problems with the word made flesh: the great tradition of the scientific revolution in Europe', *Journal of Early Modern History* (2017) 21(5), pp. 394–406, 396, 401; Ian Hacking, *Historical Ontology*, Cambridge, MA: Harvard University Press, 2002, pp. 4, 164–5, 190; John Tresch, *The Romantic Machine: Utopian Science and Technology after Napoleon,* Chicago: The University of Chicago Press, 2012, Chapter 9; Steve Fuller, *The Philosophy of Science and Technology Studies*, New York: Routledge, 2006, Chapter 4.

16 Examples are Michael Friedman, *Reconsidering Logical Positivism*, Cambridge: Cambridge University Press, 1999; Anastasios Brenner, *Les origines françaises de la philosophie des sciences*, Paris: Presses universitaires de France, 2003; Erik C. Banks, *The Realistic Empiricism of Mach, James and Russell: Neutral Monism Reconceived*, Cambridge: Cambridge University Press, 2014; Annie Petit, *Le système d'Auguste Comte: De la science à la religion par la philosophie*, Paris: Vrin, 2016; Jordi Cat and Adam Tamas Tuboly (eds.), *Neurath Reconsidered: New Sources and Perspectives*, Cham: Springer, 2019; Evaldas Nekrašas, *The Positive Mind: Its Development and Impact on Modernity and Postmodernity*, Budapest: Central European University Press, 2022.

17 Auguste Comte, *Cours de philosophie positive*, 2 vols., Paris: Rouen frères, 1830–5, vol. 1, pp. 14–15 (positive method defined), 60–1, 86–98 (classification by phenomena), 98–101, 107–9 (link between phenomena and methods); vol. 2, pp. 18–19, 29–30; and Johan Heilbron, 'Auguste Comte and the second scientific revolution', in Andrew Wernick (ed.), *The Anthem Companion to Auguste Comte*, London: Anthem Press, 2017, pp. 26–8.

18 Comte, op. cit. (17), vol. 1, pp. 17–22, 100–1, 109 (*s'adapter*).

19 Comte, op. cit. (17), vol. 1, pp. 98, 115; vol. 2, pp. 457–9.

20 Comte, op. cit. (17), vol. 1, pp. 77–85.

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Crombie, who shared Comte's vision of 'diversification of scientific methods [by] the problems imposed by different subject-matters'.[21] The upshot is that the most interesting part of Comte's big picture – the moment when an old method changes under the pressure of new matter – is the part that has been overlooked by historians. It is time to realize Comte's vision, though without Comte's vices. The best way to do this is by example. As Comte wrote about positivism, 'the method cannot be studied independently of the uses to which it is put'.[22]

## Crystals

Classification is a good place to start, for several reasons. It is often seen as characteristic of eighteenth-century science, and even of the Enlightenment more generally. The binomial nomenclature, introduced by the Swedish botanist Carl Linnaeus in 1753, is a staple of surveys of eighteenth-century science. So is Linnaeus's sexual system of plant classification and his quarrel with Georges Buffon, his French counterpart, over the relative merits of natural and artificial systems of classification.[23] The quarrel featured in an especially important survey, Michael Foucault's *The Order of Things*, first published in 1966. Foucault used natural history to illustrate what he called 'the classical episteme', a general approach to knowledge that he found in law, linguistics and other human sciences, as well as in natural history.[24] This book set an influential precedent, not only by making classification characteristic of eighteenth-century science but also by showing how to write the history of knowledge as a series of epistemes that each emerged simultaneously across many subject matters, from plants to animals to languages.[25] Epistemes are very much alive today, whether in the form of 'ways of knowing', of 'mechanical objectivity' or of 'the invention of science'.[26] Eighteenth-century natural history is therefore an important test case for big-picture histories more broadly.

The basic problem with the received view is that plants, animals and minerals have different histories. This has been pointed out recently by historians of all three kingdoms.[27] The difference between the kingdoms was not just a matter of timing, with zoology and mineralogy lagging behind botany. It was also a matter of method. Crudely put, the classification of animals and minerals was experimental in a way that the classification of plants was not. It is true that there was an experimental dimension to Linnaeus's work, as recent research has shown. But the fact remains that he classified

21 Crombie, op. cit. (4), vol. 1, p. 93; Hacking, *Historical Ontology*, pp. 178–99, in an essay entitled '"Style" for historians and philosophers'.

22 Comte, op. cit. (17), vol. 1, p. 39.

23 Thomas L. Hankins, *Science and the Enlightenment*, Cambridge: Cambridge University Press, 1999 (first published 1985), pp. 145–52; Peter Dear, *The Intelligibility of Nature: How Science Makes Sense of the World*, Chicago: The University of Chicago Press, 2006, Chapter 2; Knight, op. cit. (3), Chapter 10.

24 Michel Foucault, *Les mots et les choses: Une archéologie des sciences humaines*, Paris: Gallimard, 1966, pp. 229–33, 238–45.

25 Foucault's influence on historians of science is noted at Daston, op. cit. (1), pp. 809–10.

26 Pickstone, op. cit. (5); Wootton, op. cit. (3); Lorraine Daston and Peter Galison, *Objectivity*, Princeton, NJ: Zone Books, 2007.

27 See also Knight, op. cit. (3), 226; Dániel Margócsy, *Commercial Visions: Science, Trade, and Visual Culture in the Dutch Golden Age*, Chicago: The University of Chicago Press, 2014, Chapter 2, esp. pp. 33–8, 65–70; Staffan Müller-Wille, 'Eighteenth-century classifications of non-living nature', in Ursula Klein (ed.), *Spaces of Classification*, Berlin: Max Planck Institute for the History of Science, 2003, pp. 115–30; Stéphane Schmitt, *Aux origines de la biologie moderne: Histoire de l'anatomie comparée, d'Aristote à la théorie de l'évolution*, Paris: Belin, 2006, pp. 13, 21. See also the classic studies by Henri Daudin, *De Linné à Jussieu: Méthodes de la classification et idée de série en botanique et zoologie (1740–1790)*, Paris: Alcan, 1926, pp. 22, 48, 67–8, 70, 118, 156–9; Daudin, *Les classes zoologiques et l'idée de série animale en France à l'époque de Lamarck et de Cuvier (1790–1830)*, vol. 1, Paris: Alcan, 1926, pp. ii–iii.

plants without taking them apart, as did his eighteenth-century followers.[28] By contrast, naturalists who extended Linnaeus's methods to the other kingdoms certainly did take minerals and animals apart. The science of crystals illustrates the point.[29] Linnaeus himself classified minerals by their crystal form, first in his *Systema naturae* of 1735 and later in a stand-alone dissertation published in 1747. This resembled his work on plants insofar as it was based on a simple, visible variable. The number of crystal faces did for minerals what the number of pistils and stamens had done for plants. This scheme was slow to catch on, however. It took half a century for Linnaeus's idea to be developed into a tool that could plausibly be used to classify the whole of the mineral kingdom. The people who fashioned this tool – mainly Torbern Bergman, Jean-Baptiste Louis Romé de l'Isle and René-Just Haüy – all took Linnaeus as a model. But they also found themselves doing things to minerals that Linnaeus had never done to plants. Romé de l'Isle imagined geometrical operations that could generate many crystal forms from a single, simple form. Starting with a cube, for example, he mentally sliced off the corners of the cube to produce an eight-sided shape; slicing off the edges of a cube gave a different shape; and so on. Haüy's innovation was to replace these mental operations with a physical one, dividing his crystals along their cleavage planes until their shape was unchanged by further divisions. The unchanging core helped to classify the original crystal; all crystals with the same core belonged to the same species. All this introduced new practices into crystallography, such as tapping minerals with a sharp tool to divide them, and consulting the gem cutters who performed this operation on a daily basis. The extent of the change is shown by contemporary resistance to it, as when Romé de l'Isle accused Haüy of 'mutilating' crystals. The effort to transfer Linnaean methods from plants to minerals resulted in methods that were not quite Linnaean. Old methods plus new matter gave new methods.

Some clarifications are in order. First, minerals were not new in any absolute sense, and neither were crystals. Both had been studied for centuries by naturalists in Europe and elsewhere. But they had not been studied using methods of the kind that Linnaeus had applied to plants. They were a new matter for those methods. Second, there was more to crystallography than the appropriation of methods from botany. Experimental physics was another source of inspiration, especially for Haüy, who used electrical and optical properties to classify minerals, alongside crystal form; chemistry was important as well.[30] Third, the traffic between botany and mineralogy went in both directions. One of Haüy's followers was Augustin-Pyramus de Candolle, a Swiss botanist who borrowed the idea of symmetry from crystallography when he developed a new classification of plants early in the nineteenth century.[31] Finally, the material properties of plants and minerals did not act on their own. They acted in tandem with human factors. The fact that diamonds have cleavage planes, whereas daffodils do not, is a material property that helps to explain why diamonds were classified by taking them apart whereas daffodils were not. But other factors help to explain the same thing. The growing consensus among

28 Note Daudin's remarks on the non-use of anatomical methods for plant classification in the eighteenth century: Daudin, *De Linné à Jussieu*, op. cit. (27), pp. 24–5, 52–3. Linnaean experimentation in Staffan Müller-Wille, 'Collection and collation: theory and practice of Linnaean botany', *Studies in History and Philosophy of Biological and Biomedical Sciences* (2007) 38(3), pp. 541–62, 544–5.

29 The following summary is based on John G. Burke, *Origins of the Science of Crystals*, Berkeley: University of California Press, 1966, esp. pp. 62–106. The adaptation of Linnaean methods to animals is a major theme of Daudin, *De Linné à Jussieu*, op. cit. (27), esp. pp. 48–65, 65–70, 73–6, 124–5, 144–59, 217–27, 233–4; and of Daudin, *Classes zoologiques*, op. cit. (27), *passim*.

30 See, for example, Christine Blondel, 'Haüy et l'électricité: De la démonstration-spectacle à la diffusion d'une science newtonienne', *Revue d'histoire des sciences* (1997) 50(3), pp. 265–82; and Stephen T. Irish, 'The corundum stone and crystallographic chemistry', *Ambix* (2017) 64(4), pp. 301–25.

31 Peter F. Stevens, 'Haüy and A.-P. Candolle: crystallography, botanical systematics, and comparative morphology, 1780–1840', *Journal of the History of Biology* (1984) 17(1), pp. 49–82.

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eighteenth-century naturalists that minerals are fundamentally different from plants and animals was an important factor; the discovery of diamonds in Brazil in the 1720s was another. The point is that the history of minerals is different from the history of plants, partly because minerals are different from plants. All these differences, material and human, meant that methods changed when they migrated from plants to minerals.

This analysis makes better sense of crystallography than existing big pictures do. Crystallography is normally treated in one of two ways in such surveys. Either it is yet another instance of the eighteenth-century mania for classification, or it is a harbinger of whatever epoch or episteme is thought to have succeeded the eighteenth-century one.[32] The former treatment ignores the fact that minerals were a distinctive subject matter that posed peculiar problems for naturalists bent on classifying them. The latter treatment ignores the fact that crystallographers in the last quarter of the century were self-consciously following the example set by Linnaeus several decades earlier. A Comtean analysis can account for both the differences between botany and mineralogy and the continuity between them. They were different because plants are not minerals; they were continuous because naturalists hoped, reasonably enough, that a method that worked for plants would also work for minerals.

## Stars

The stars are a different problem for historians than are crystals. There is no equivalent in the historiography of astronomy to the tendency to conflate plants, animals and minerals in the historiography of natural history. In particular, there has been no failure to distinguish between the astronomy of the solar system and the astronomy of the rest of the universe. It is well known that most early modern astronomy was about the Sun, the Moon and the planets. The stars were treated as a fixed background against which to track the motions of objects in the solar system. Heliocentrism did little to change this. In fact, the 'fixed stars' were even more fixed in the Sun-centred cosmos than they had been in the Earth-centred cosmos, because motions once attributed to the stars were now attributed to the Earth. Even Isaac Newton had little to say about the stars. His 'universal' theory of gravity effectively stopped at Saturn.

This changed in the eighteenth century, largely due to the British German astronomer William Herschel and his sister, Caroline Herschel. The Herschels became famous for discovering a new planet in the solar system, Uranus, but the universe beyond the solar system was their main subject of study. Methodologically, they broke new ground by doing what William Herschel called 'the natural history of the heavens'. They collected and classified thousands of celestial objects, especially double stars, star clusters and nebulae. They then arranged these objects in temporal sequence, just as naturalists arranged specimens of a plant species in order to show the life cycle of an individual plant. The result has been called a 'biological', as opposed to a 'clockwork', vision of the universe.

This account of eighteenth-century stellar astronomy has been circulating for at least forty years, largely due to the work of Michael Hoskin and Simon Schaffer.[33] It seems like

32 An example of the former is Dear, op. cit. (23), p. 53. Examples of the latter are Knight, op. cit. (3), p. 284; and Pickstone, op. cit. (5), p. 123.

33 Michael Hoskin, *Stellar Astronomy: Historical Studies*, Chalfont St Giles: Science History Publications, 1982; Hoskin, *The Cambridge Concise History of Astronomy*, Cambridge: Cambridge University Press, 1999, Chapter 7; Hoskin, *The Construction of the Heavens: The Cosmology of William Herschel*, Cambridge: Cambridge University Press, 2012; Hoskin, *Discoverers of the Universe: William and Caroline Herschel*, Princeton, NJ: Princeton University Press, 2011; Simon Schaffer, 'Herschel in Bedlam: natural history and stellar astronomy', *BJHS* (1980) 13(3), pp. 211–39; Schaffer, '"The great laboratories of the universe": William Herschel on matter theory and planetary life', *Journal of the History of Astronomy* (1980) 11, pp. 81–111.

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a ready-made example of a new method emerging from new matter. But there are problems with the received account, the most obvious of which is that William's pioneering telescopes owed far more to astronomy than they did to natural history. The natural history of plants and animals was one model for William, but the more important model was the astronomy of the solar system. William took astronomical methods that had been applied to the Sun, Moon, planets and comets; he applied these methods to stars and nebulae; and he changed the methods in the process. This view is present in the work of Hoskin and Schaffer, but it is in need of explication and elaboration.[34]

William's theory of gravitational attraction illustrates the point. The astronomer tried to do for the stars what Newton had done for bodies in the solar system – to give a precise description of their motions and to explain these motions in terms of radially symmetric forces of attraction. This comes through clearly in a 1785 paper in which William explained how gravity pulls stars into clusters of various shapes and sizes, including the very large cluster that we know as the Milky Way.[35] Gravity was less prominent in William's earlier publications on the stars, but it was present nevertheless;[36] it went on to become the driving force of his mature cosmological theory.[37] William frequently reminded the reader of the analogy between gravity in the solar system and gravity beyond it. In a typical passage, he recalled one of the standard Copernican arguments for the daily rotation of the Earth on its axis, namely that this rotation is much simpler than the daily rotation of the stars around the Earth that Ptolemy's theory supposed. By the same token, William reasoned, it is simpler to suppose that the solar system is moving towards a point in the constellation Hercules than to suppose that all stars have the contrary motion.[38] Elsewhere, William likened nebulae in the heavens to the aurora borealis on Earth. Examples can be multiplied. William understood the stars by analogy to the solar system.

But the world beyond the solar system was not quite the same as the world within it. The study of the stars called for new methods – or, rather, adjustments to old methods. It called for new telescopes, ones that collected enough light to observe distant stars and nebulae. For the Herschels, this meant using reflecting telescopes rather than refracting ones, and large mirrors rather than small ones.[39] Granted, such telescopes also enabled

34 It is present, for example, in Hoskin, *Stellar Astronomy*, op. cit. (33); and in Schaffer, 'Herschel in Bedlam', op. cit. (33), esp. p. 87 ('tried to extend gravity').

35 William Herschel, 'On the construction of the heavens', *Philosophical Transactions of the Royal Society of London* (1785) 75, pp. 213–66, esp. 214–16.

36 Pre-1785 examples are William Herschel, 'On the parallax of the fixed stars', *Philosophical Transactions of the Royal Society of London* (1782) 72, pp. 82–110, 103; Herschel, 'Account of some observations tending to investigate the construction of the heavens', *Philosophical Transactions of the Royal Society of London* (1784) 74, pp. 437–51, 448; Herschel, 'On the proper motion of the sun and solar system', *Philosophical Transactions of the Royal Society of London* (1783) 73, pp. 247–83, 248. In the latter paper, Herschel invoked his three reviews of the stars (pp. 249–58) to suggest that stars move according to the 'theory of attraction' (248). These reviews began in 1778: Hoskin, *The Construction of the Heavens*, op. cit. (33), pp. 16–17.

37 William Herschel, 'Astronomical observations relating to the construction of the heavens', *Philosophical Transactions of the Royal Society of London* (1811) 101, pp. 269–336, esp. 284, 331; Herschel, 'Astronomical observations relating to the sidereal part of the heavens', *Philosophical Transactions of the Royal Society of London* (1814) 104, pp. 248–84, esp. 253, 257, 269, 271.

38 Herschel, 'Proper motion of the sun', op. cit. (36), pp. 266–7. Similar examples are at William Herschel, 'On nebulous stars, properly so called', *Philosophical Transactions of the Royal Society of London* (1791) 81, pp. 71–88, 83–4; Herschel, 'Catalogue of a second thousand of new nebulae … Remarks on the construction of the heavens', *Philosophical Transactions of the Royal Society of London* (1789) 79, pp. 212–55, 219; Herschel, 'Catalogue of 500 new nebulae … Remarks on the construction of the heavens', *Philosophical Transactions of the Royal Society of London* (1802) 92, pp. 477–528, 481–2; Herschel, 'On the nature and construction of the sun and fixed stars', *Philosophical Transactions of the Royal Society of London* (1795) 85, pp. 46–72, 46, 78–81.

39 Michael Hoskin, 'Herschel, William', *Oxford Dictionary of National Biography*, at https://doi.org/10.1093/ref:odnb/13102 (accessed 2 February 2023); Hoskin, *The Construction of the Heavens*, op. cit. (33), pp. 38–43.

observations of small objects in the solar system, such as Uranus. But William built them for remote objects rather than small ones, as suggested by his usual term for the performance of his telescopes, 'space-penetrating power'.[40] Needless to say, he did not invent the telescope from scratch. Rather, he learnt the basics of telescope construction from a book filled with solar system astronomy, James Ferguson's *Astronomy Explained upon Sir Isaac Newton's Principles* (1756). He then adapted what he learnt to the study of the stars. In his search for nebulae, he began with small and mobile telescopes that were designed for bright, fast-moving comets. He soon replaced these with large and cumbersome instruments that were perfect for faint, slow-moving nebulae.[41] He also replaced his usual method of recording observations – by himself, using artificial light – with the oral method of calling his observations out to his sister. Caroline tweaked her own method, rearranging a star catalogue so that she could read off the positions of stars in the order in which they passed through the field of William's telescope.[42] Studying the stars called for new social and literary arrangements as well as new instruments.

The study of the stars also involved statistical reasoning, or what William called 'the doctrine of chances'. William's map of the Milky Way is the standard example. In a 1785 paper, he mapped the boundaries of the galaxy by counting the number of stars he saw in 690 patches of the night sky. The more stars in the patch, he reasoned, the deeper the Milky Way in the direction of the patch. The argument assumed that stars are distributed evenly throughout the galaxy. This assumption was literally false but statistically plausible. As William put it, 'when we take all the stars collectively there will be a mean distance which may be assumed as the general one'.[43] This example has been widely cited by historians, but William invoked the 'doctrine of chances' in many other places. His usual approach was to use the sheer quantity observable stars to argue that such-and-such an appearance could not be a coincidence. For example, there were many more double stars (stars that appear very close together) than there would be if all stars were distributed at random through the sky. For William, this was one step towards showing that each double star is a physical system governed by the laws of gravity.[44] William used similar arguments for the physical reality of star clusters and for the spherical shape of clusters that appear circular.[45] He could argue thus because he could see thousands of stars but only a handful of planets and comets, and because stars were much further away than solar objects and therefore beyond the reach of 'strict demonstration', in his phrase.[46] Stars gave William the means and the motive to be statistical.

All this puts a new complexion on William's engagement with natural history. To begin with, William's 'natural history of the heavens' was not as coherent as it appears at first sight. He used that phrase for the first time in 1802, two decades after he became a professional astronomer.[47] He did invoke natural history before this, but in a piecemeal and opportunistic way. In 1782 he used it to avoid giving an explanation, in 1783 to give an explanation along geological lines,[48] in 1789 to show the continuity between different

40 Hence the title of James A. Bennett's detailed account of Herschel's telescopes: '"On the power of penetrating into space": the telescopes of William Herschel', *Journal of the History of Astronomy* (1976) 7, pp. 75–108.

41 Hoskin, *Discoverers of the Universe*, op. cit. (33), pp. 82–6, 91–2.

42 Hoskin, *Discoverers of the Universe*, op. cit. (33), pp. 93–4, 96–7.

43 Herschel, op. cit. (35), p. 245; Hoskin, *The Construction of the Heavens*, op. cit. (33), pp. 58–9.

44 Herschel, op. cit. (35), p. 485. Cf. Hoskin, *The Construction of the Heavens*, op. cit. (33), pp. 17–20.

45 Herschel, op. cit. (35), p. 255 n. f. Herschel, 'Construction of the heavens' (1789), op. cit. (38), pp. 215, 217–18.

46 Herschel, 'On the parallax of fixed stars', op. cit. (36), p. 104.

47 Herschel, 'Construction of the heavens' (1802), op. cit. (38), p. 157.

48 Herschel, 'Construction of the heavens' (1784), op. cit. (36), p. 438, cf. 451.

celestial objects,[49] and in 1791 to show the lack of continuity between such objects.[50] William's catalogues of double stars and nebulae may look like natural history, but they were an extension of earlier star catalogues, with their long columns of numbers and their classification of stars by brightness.[51] The idea that star clusters evolve into galaxies may sound biological, but William came to the idea through the theory of gravity and not through the life histories of plants and animals.[52] William's main astronomical goal was not the natural history of the heavens but 'knowledge of the construction of the heavens'. This was shorthand for a knowledge of the three-dimensional structure of the universe beyond the solar system.[53] This programme was far more explicit and sustained in William's career than was the natural history of the heavens. And it was modelled on the astronomy of the solar system, with its three-dimensional arrangement of bodies driven by gravity and observable with telescopes.

When Herschel did invoke natural history, he did so with a view to 'constructing the heavens'. In 1783, his reference to geological strata helped to introduce the idea that the stars should be viewed in three dimensions rather than two. In 1789, the life cycle of plants was a metaphor for the formation of star clusters under the action of central powers such as gravity. In 1802, the 'proper classes' of celestial objects meant the classes that made sense in terms of the 'known laws of gravitation'.[54] The finely graded series of celestial objects that we find in William's mature cosmological papers, in 1811 and 1814, fit the same pattern. These papers extended the gravitational theory of star clusters to the phenomenon of 'true nebulosity' that William had announced in 1791.[55] In sum, the natural history of the heavens was a by-product of stellar astronomy, just as stellar statistics and twenty-foot reflectors and oral reporting had been. The effort to extend astronomy to the stars led to a new kind of astronomy.

## The Pacific Ocean

Meanwhile, the effort to extend navigation to the Pacific led to new kinds of navigation. Navigation illustrates a third historiographical challenge, different from the two discussed above. For natural history, the challenge was to distinguish between two matters usually lumped together – plants and minerals. For astronomy, the challenge was to correctly identify the old matter – the solar system, not plants and animals. For navigation, the challenge is to see that old matter of some kind really did make a difference. The study of Pacific exploration is so focused on the Pacific that other oceans have been overlooked as a source of methods that were then adapted to the Pacific.

The Pacific Ocean, like stars and crystals, was an enigma in 1700. Ferdinand Magellan had crossed it in 1521, and Spanish galleons had crossed it many times since then, but crossing an ocean is not the same thing as criss-crossing it. Magellan crossed the ocean in a narrow band of sea just below the equator, making the most of the easterly trade winds there; the explorers who followed him into the Pacific did the same. Meanwhile, the galleons went back and forth in an equally narrow band of sea just north of the equator. The galleons missed Hawaii and the explorers missed most of the

49 Herschel, 'Construction of the heavens' (1789), op. cit. (38), p. 137, cf. 145. See also Herschel, 'On nebulous stars', op. cit. (38), p. 147.

50 Herschel, 'On nebulous stars', op. cit. (38), p. 148.

51 Herschel's analogy for these catalogues was bibliographical, not natural-historical: 'Construction of the heavens' (1802), op. cit. (38), p. 157.

52 Herschel, op. cit. (35), pp. 214-16.

53 Herschel, 'Construction of the heavens' (1811), op. cit. (37), p. 169.

54 Herschel, 'Catalogue of 500 new nebulae' (1802), op. cit. (38), p. 478.

55 Herschel, 'Construction of the heavens' (1811), op. cit. (37); Herschel, 'Sidereal part of the heavens,' op. cit. (37).

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Polynesian islands. Dutch explorers made it to Australia and New Zealand in the 1640s, but they mapped only a small fraction of those two land masses. This changed little in the first two-thirds of the eighteenth century. It then changed dramatically. By 1803, the coasts of New Zealand and Australia had been circumnavigated; Tahiti and Hawaii had been encountered; numerous Polynesian islands had been precisely located; the northern coasts of the Pacific, both Asian and American, had been outlined; the Antarctic Circle had been crossed; and the vast empty ocean in the south Pacific had been shown to be just that. The modern map of the Pacific was born.[56]

How did this happen? The Comtean answer is easy to state. Europeans took the methods of mapping and seafaring that had worked in the Atlantic and Indian Oceans and applied them to the Pacific; the methods changed in the process because the Pacific was not the Atlantic or the Indian Ocean. Half of this answer is common knowledge among historians of Pacific exploration. The classic work on the topic, written by John Beaglehole and published in 1934, started from the assumption that the Pacific was distinctive and therefore called for distinctive methods of exploration. The 'immense size of the ocean' and the 'minute size of the islands that sprinkled its surface' were the main difficulties; a means of determining longitude at sea was the main solution.[57] The distinctiveness of the Pacific meant that Beaglehole treated the exploration of the Pacific in the late eighteenth century as a continuation of earlier explorations of the Pacific. This approach persists in the most recent surveys of the topic.[58] It is reinforced by histories that look at Pacific exploration from the Polynesian point of view. The standard work in this genre begins by noting that the 'habitat' of Polynesian and Micronesian peoples is made up mainly of water, with two units of dry land for every thousand of water.[59] Many of the methods described in the book are specific to the Pacific, with its large stretches of water and its archipelagos made up of small, low atolls.[60] 'A Polynesian approach to navigation was perfectly adapted to the Pacific', as James Poskett puts it.[61]

This is only half the story. The Pacific was certainly distinctive. But other seas were distinctive too, and sailors have a habit of moving from one sea to another, changing their methods as they go. This is true even within the Pacific, which is not one sea but many seas. New Zealand is larger than all the other Polynesian islands combined.[62] The Marshall Islands resemble other archipelagos in the region but are situated in the band of light winds known as 'the doldrums'.[63] Rapa Nui (Easter Island), at the eastern extremity of Polynesia, is an isolated island rather than a member of an archipelago.[64] The point

56 John C. Beaglehole, *The Exploration of the Pacific*, London: Adam and Charles Black, 1966 (first published 1934); Glyndwr Williams, 'Pacific voyages', in Mathew H. Edney and Mary Sponberg Pedley (eds.), *The History of Cartography*, vol. 4: *Cartography in the Enlightenment*, Chicago: The University of Chicago Press, 2019, part 2, pp. 1083–90; John Gascoigne, *Encountering the Pacific in the Age of the Enlightenment*, Cambridge: Cambridge University Press, 2014, maps 3–12 and *passim*.

57 Beaglehole, op. cit. (56), pp. 10–12, 309–15; John C. Beaglehole, *The Life of Captain James Cook*, Stanford, CA: Stanford University Press, 1974, pp. 109–11.

58 Williams, op. cit. (56), p. 1083; Gascoigne, op. cit. (56), p. 33; Ben Finney, 'Nautical cartography and traditional navigation in Oceania', in David Woodward and G. Malcolm Lewis (eds.), *The History of Cartography*, vol. 2, book 3: *Cartography in the Traditional African, American, Arctic, Australian, and Pacific Societies*, Chicago: The University of Chicago Press, 1998, pp. 443–92, 445.

59 David Lewis, *We, the Navigators: The Ancient Art of Landfinding in the Pacific,* Honolulu: University of Hawaii Press, 2022 (first published 1972), p. 15.

60 Lewis, op. cit. (59), esp. Chapter 4.

61 Poskett, op. cit. (2), pp. 123–4.

62 James Belich, *Making Peoples: A History of the New Zealanders, from Polynesian Settlement to the End of the Nineteenth Century*, Auckland: Allen Lane, 1996, p. 41.

63 Finney, op. cit. (58), p. 476.

64 Finney, op. cit. (58), p. 461.

is not just that the Pacific was plural but also that it housed a corresponding plurality of seafaring cultures. The light winds in the Marshall Islands mean that ocean swells are unusually visible there, which helps to explain why Marshall Islanders have sophisticated methods of swell analysis that are not found anywhere else in the Pacific.[65] In New Zealand, Polynesians learnt to navigate on the turbulent rivers in the southeastern corner of the country.[66] More generally, the people who first settled in the islands of the Pacific changed their methods of seafaring in response to the new seascapes they discovered.[67] Something similar happened in early modern Europe, when sailors trained in the Mediterranean Sea began to travel regularly to other seas, such as the English Channel and the southern Atlantic. They picked up new methods as they went: quadrants and cross-staffs to find their latitude down the coast of West Africa, sounding gear to navigate the shoals and tides of the Channel.[68] The history of navigation is quite literally a history of the migration of methods.

The exploration of the Pacific in the late eighteenth century is no exception to this rule. Consider the career of Captain James Cook, the most celebrated European sea explorer of the period. Cook's three voyages to the Pacific spanned the last decade of his life, from 1768 to 1779. But he had already been sailing in the northern Atlantic for two decades before this – one decade in the North Sea and the English Channel and another decade in the seas around Newfoundland. These were ideal training grounds for a seafarer. The east coast of England was a notoriously difficult stretch of water, with its shoals, sandbars, storms and strong tides and currents.[69] This called for skill in the 'three L's' of early English navigation: the lead to sound the sea floor, the log to measure the ship's speed and the look-out to give early warning of dangers.[70] All this served Cook well when, in 1755, he left the coal industry and joined the navy. In 1759 he helped a British fleet to navigate the sand-banks and narrow straights of the St Lawrence river, an echo of his earlier experience on the Thames estuary.[71] Cook continued to explore the Newfoundland area until 1767, refining his methods as he went. He learnt the latest techniques of land surveying, including plane tables, theodolites, triangulation and draughtsmanship. He adapted these methods to the intricate coastline of Newfoundland.[72] He refloated a grounded ship, grappled with icebergs and dense fog, brewed spruce beer to ward off scurvy, and used a telescopic quadrant to observe a solar eclipse and thereby determine his longitude.[73] Cook was already an accomplished Atlantic sailor before he became a Pacific one.

Moreover, the Pacific was not as strange as it might seem. To be sure, it presented its own challenges to any European sailor, the most obvious being the need to regularly determine the ship's longitude while at sea. Cook took his first longitude at sea at the

65 Finney, op. cit. (58), pp. 475–85, esp. 485.

66 Belich, op. cit. (62), p. 51.

67 Finney, op. cit. (58), pp. 486–9; Phillip Lionel Barton, 'Māori cartography and the European encounter', in Woodward and Lewis, op. cit. (58), pp. 491–532, 493.

68 E.G.R. Taylor, *The Haven-Finding Art: A History of Navigation from Odysseus to Captain Cook*, London: Hollis and Carter, 1958 (first published 1956), pp. 28–32, 131–43, 158–67; Peter Ifland, *Taking the Stars: Celestial Navigation from Argonauts to Astronauts*, Florida, CA: Krieger Publishing Company, 1998, pp. 5–6.

69 Beaglehole, op. cit. (57), 8; Victor Suthren, *To Go upon Discovery: James Cook and Canada, from 1757 to 1779*, Toronto: Dundurn Press, 2000, 16; Jerry Lockett, *Captain James Cook in Atlantic Canada: The Adventurer and Map Maker's Formative Years*, Halifax: Formac Publishing, 2011, p. 18.

70 Beaglehole, op. cit. (57), pp. 12–14. Lockett, op. cit. (69), pp. 75–8.

71 Beaglehole, op. cit. (57), pp. 43–5. Lockett, op. cit. (69), pp. 64–5.

72 Beaglehole, op. cit. (57), Chapters 3, 4.

73 Ice at Beaglehole, op. cit. (57), pp. 35, 68, 311; Lockett, op. cit. (69), p. 62. Fog at Lockett, op. cit. (69), p. 130; Taylor, op. cit. (68), pp. 25–6. Rocks at Beaglehole, op. cit. (57), pp. 93–4; Lockett, op. cit. (69), pp. 132, 138–9. Spruce beer at Lockett, op. cit. (69), pp. 44, 36–7. Telescopic quadrant and eclipse at Beaglehole, op. cit. (57), pp. 87–90; Lockett, op. cit. (69), pp. 122, 134.

beginning of his first Pacific voyage; famously, he made successful use of marine chronometers for determining longitudes on his second and third voyages.[74] But there were also striking analogies between Cook's Pacific experience and his Atlantic education. Dead reckoning was still needed to verify the longitudes given by astronomical methods.[75] Hill climbing was part of Cook's surveying technique, in New Zealand as in Newfoundland.[76] The sounding line was indispensable in the Great Barrier Reef, as it was in the North Sea.[77] The sandbanks of Kuskokwim Bay, in the north-eastern corner of the Pacific, were just as treacherous as those in the English Channel, and for similar reasons.[78] The gales around New Zealand may have reminded Cook of the equally windy stretch of coast between the Tyne and the Thames.[79] While sheltering from these gales in a sound, he used the leaves of the rimu tree to imitate the spruce beer of Newfoundland.[80] Last but not least, Cook consulted local experts in the Pacific just as he had in the Atlantic. The pilots of the Thames and the fisherman of Newfoundland had their counterparts in the Tahitian, Māori, Russian and Native American cartographers who guided Cook around the Pacific.[81] The Pacific was both strange and familiar to the Yorkshireman – strange enough to require new techniques, familiar enough to accommodate old ones. Yes, Pacific exploration was adapted *to* the Pacific. But it was also adapted *from* the Atlantic. Cook changed his methods in response to new matter, just as Haüy had done for crystals and the Herschels for the stars.

## Depolarizing the history of science

These three examples show how neo-positivism can work in practice. It is not a rigid hierarchy with a preordained slot for every scientific discipline. It is a simple recipe for making sense of episodes of scientific change. The recipe is: look for a change in method, look for a difference in matter, try to explain the former in terms of the latter. For example: note the change from classification by observation to classification by experiment in the latter part of the eighteenth century, note that classification was done on minerals as well as plants, explain the change in method in terms of the effort to extend classification by observation from plants to minerals. The recipe varies depending on the state of the literature on the topic under study. It varies because historians have usually already identified one or more of the ingredients. The job of the neo-positivist is to complete the recipe. This can be done by distinguishing matters that are usually conflated (as for natural history), by emphasizing a matter that is usually treated as secondary (as for stellar astronomy), or by bringing old matters into a picture that is dominated by a new matter (as for Pacific navigation). Neo-positivism is a mountain that can be climbed from different sides.[82]

74 Beaglehole, op. cit. (57), p. 154 (first longitude), *passim* (chronometers). Other Pacific novelties include those noted at Beaglehole, op. cit. (57), pp. 311 (birds), 245 (reefs), 315–16 and 622–3 (drifting ice), 323 n. 1 (122 days at sea).

75 Beaglehole, op. cit. (57), p. 166 n. 1.

76 Beaglehole, op. cit. (57), pp. 213, 221, 239, 615; R.A. Skelton, 'Captain James Cook as a hydrographer', *The Mariner's Mirror* (1954) 42(2), pp. 91–119, 114.

77 Beaglehole, op. cit. (57), p. 244.

78 Beaglehole, op. cit. (57), p. 612.

79 Beaglehole, op. cit. (57), pp. 208–10.

80 Beaglehole, op. cit. (57), p. 324.

81 Fisherman in Suthren, op. cit. (69), p. 150; Thames pilot in Beaglehole, op. cit. (57), p. 93; Tahitian pilot in Poskett, op. cit. (2), pp. 60–54; Māori, Russian and Native American guides in Beaglehole, op. cit. (57), pp. 215–16, 631–3, 598.

82 The metaphor is from Derek Parfit, *On What Matters*, Oxford: Oxford University Press, 2011, p. 26.

There are other ways of reaching the peak that may be illustrated by other eighteenth-century matters. One might think about the way the methods of mathematical physics were extended from celestial bodies to terrestrial phenomena such as electricity, or the way quantitative chemistry was extended from solids and liquids to gases. But there is no room to develop these examples here. In any case, a big picture is more than a list of examples. The question is, how does the neo-positivist theory of scientific change help to resolve the disagreements discussed earlier in this article? Since my examples have been about the eighteenth century, my answer will be based on the same century. This is a good century to focus on because it appears to be strung between two revolutions, the 'first scientific revolution' centred on about 1650 and the 'second scientific revolution' centred on about 1800.[83] We do not have a good account of how these two events were connected. Neo-positivism suggests such an account. Simply put, the second revolution was the result of the effort to extend the methods of the first revolution to new matters. This is a bold claim, but it emerges readily from the examples discussed so far. The novelties I have been discussing – crystallography, stellar astronomy, and Pacific exploration – are all associated with the second scientific revolution. I have argued that these novelties were continuous with earlier developments, such as systematic botany, planetary astronomy and Atlantic exploration. Now, these earlier developments have all been associated with the first scientific revolution. We do not have to choose between the first and second revolutions; we can see the latter as an elaboration of the former.

So much for the chronological disagreement. The other bones of contention were geography, disciplines, narrative structure, revisionism and epistemology. The disciplinary question can be answered simply by noting the range of examples discussed so far. Neo-positivism works for sciences near the top of Comte's hierarchy (like astronomy) and sciences near the bottom of that hierarchy (such as botany). It works for traditions that Comte treated as arts rather than sciences, such as navigation. Indeed, it works for any tradition that uses some distinctive method or methods to investigate some distinctive matter or matters. Although I have not given any examples from the human sciences, it is easy to see how neo-positivism might work for such sciences. It has been argued, for example, that a new approach to human difference emerged in Europe in the late eighteenth century, and that this was an extension of new approaches to differences among other animals. The notion of 'race' was transferred from dogs and horses to humans.[84] Were the methods of animal classification altered by this transfer, given that humans are not the same as dogs and horses? We may not yet have an answer to this question, but the question certainly makes sense.

The geographical question is not so easily answered. I do not know of a simple schema that would reconcile the Eurocentric and global approaches to eighteenth-century science. Still, the examples given so far at least show that these two approaches are both consistent with a Comtean view of scientific change. Herchel's stellar astronomy is a canonical episode in European science, one that is usually explained in terms of familiar themes of European history in the period, such as the growth of the public sphere and royal patronage for science. By contrast, European exploration of the Pacific is impossible to understand without considering people and places beyond Europe. And yet stellar astronomy and Pacific exploration can both be usefully viewed through a Comtean lens,

83 For discussion of these and other narratives of eighteenth-century science see Michael Bycroft, 'Science beyond enlightenment', *Journal of Early Modern Studies* (2023) 12(1), pp. 9–31. See also the other articles in the same special issue, edited by Adrian Wilson and Michael Bycroft and entitled *The Eighteenth-Century Problem, Forty Years On*.

84 Justin E.H. Smith, *Nature, Human Nature, and Human Difference: Race in Early Modern Philosophy*, Princeton, NJ: Princeton University Press, 2015.

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as I argued above. In the case of Pacific exploration, empire helps to explain the migration of navigational expertise to the Pacific in the 1770s and 1780s. With the end of the Seven Years War in 1763, and with a workable settlement between France and Britain in the Americas, the two powers turned to the Pacific as the next theatre of imperial competition.[85] This is a European view of things, of course. But we have seen that Polynesian navigation, no less than the European kind, can be understood in terms of the adaptation of methods to new matters.

It might be objected that this focus on the matters of science pushes people into the background. If the stars themselves account for stellar astronomy, what becomes of human explanations of stellar astronomy? What becomes of the effort to explain science in terms of wider social, economic and political phenomena? The answer is that those explanations are enriched by new questions. Why did the Herschels take an interest in the stars? Whence their knowledge of the astronomy of the planets? Whence the new practices that bridged the gap between planets and stars? These questions are to be answered by reference to people, not to the stars, but the answers take on new significance when we see that the stars were different from the planets. Historians who see the history of science as a special case of general history have nothing to fear from neo-positivism.

The same goes for historians who follow Michel Foucault in seeing history as a succession of epistemes, and for historians who follow Thomas Kuhn in seeing history as a convergence of intellectual traditions. In fact, neo-positivism helps to explain why these apparently opposed approaches make sense. Epistemes make sense because a method can migrate quickly across several different subject matters. This explains such things as the 'analytical way of knowing', to use John Pickstone's term for the fashion for classifying things in terms of their physical constituents that swept across chemistry, anatomy and crystallography in the late eighteenth century.[86] The convergence of traditions makes sense as well, because methods sometimes move slowly. This is why Kuhn could write about the convergence of the 'experimental' and 'mathematical' traditions in the physical sciences.[87] What he meant was that some matters (such as planetary motion) were given a mathematical treatment in the seventeenth century whereas other matters (such as electricity) received this treatment about a century later. Sweeping epistemes and converging traditions are limiting cases of the same phenomenon, the migration of methods across matters.

The penultimate sticking point is recent revisionism. There is disagreement about the value of what Secord called the 'new history of science', as shown by the quotes from Secord and Heilbron. My experience in writing this article has been that there is merit in both sides of this argument. Older histories of science tended to be organized by discipline and to give plenty of technical detail about the sciences they described. The authors had few qualms about invoking present-day knowledge about plants, nebulae, the Pacific Ocean or whatever the subject of their chosen discipline happened to be. For all these reasons, such histories are full of insights about the way in which methods change in response to new matters. Newer histories are also full of insights, but of a different kind. They have a broader view of what I have been calling the 'methods' of science. They cover the full range of social, material and literary technologies that scientists bring to bear on their chosen matters. They are alive to unexpected connections between different scientific disciplines. They also have much to say about such things as trade, empire, state formation and gender relations, all of which help to explain why new matters and methods become salient in

85 Beaglehole, op. cit. (56), pp. 179, 194; Gascoigne, op. cit. (56), Chapter 9.

86 Pickstone, op. cit. (5), pp. 8, 11–12, Chapters 4, 5.

87 Kuhn, 'Mathematical versus experimental traditions in the development of physical science,' *Journal of Interdisciplinary History* (1976) 7(1), 1–31.

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particular times and places. These are coarse distinctions, of course, and any given book or article contains elements of the 'old' and the 'new'. But the basic point stands: neo-positivism works best when positivists and post-positivists work together.

Finally, what about epistemology? Is science a true description of a mind-independent reality? Or is this a naive and outdated view that historians of science must abandon? The answer is that it doesn't matter. We do not need to settle this question in order to do our jobs as historians. This may come as a surprise, given what I have written so far. Comte certainly did want to settle this question; my unqualified references to the cleavage planes of diamonds, the speed of nebulae and so on might suggest that I am defending some kind of scientific realism. I suspect that I am some kind of realist about science. But I am not an expert on the realism debate, so I don't expect anyone to take my opinion on the matter very seriously.[88] Fortunately, there is nothing in neo-positivism that requires anyone to be a realist about science. If any Berkeleyan idealists are reading this, for example, they are welcome to interpret phrases such as 'faint, slow-moving nebulae' as references to a pattern of ideas in the mind of God. All I ask is that Berkeleyan idealists, when they write the history of astronomy, pay attention to this particular pattern of ideas in the mind of God. Also, that they ask how this pattern relates to other patterns of ideas in the mind of God, such as the ones corresponding to the phrase 'Caroline Herschel's revised star catalogue'. In this way, the Berkeleyan idealist will be able to make sense of my claim that Caroline revised her star catalogue because (at least in part) nebulae are faint and slow-moving. If even the Berkeleyan idealist can do this, other kinds of anti-realist can surely do the same.

Some might object that this is beside the point. Historians of science are not genuine anti-realists, only methodological anti-realists. We refuse to talk about the speed or faintness of nebulae, not because we deny the existence of nebulae, but because we deny that the discussion of nebulae is part of the job description of historians of science. We study images of nebulae, instruments for detecting nebulae, social conflicts about nebulae and so on; we leave the study of the nebulae themselves to astronomers and physicists. To do otherwise (the argument runs) is to fall foul of the symmetry principle, to become an environmental determinist, to confuse history with other disciplines or to commit some other methodological error. This is why it sounds fishy to say, for example, that the Herschels studied the stars in a certain way because the stars are a certain way. The way to remove the fishy odour is to consider more down-to-earth examples. No one accused Daniel Margócsy of violating the symmetry principle when he pointed out that eighteenth-century naturalists studied plants in a certain way because plants were, as a rule, lighter and more portable than animals.[89] No one accused Sujit Sivasundarum of being an environmental determinist when he wrote that the Pacific Ocean 'had a role to play' in the study of the Pacific Ocean.[90] It should be no more objectionable to say that nebulae had a role to play in the study of nebulae, crystals in the study of crystals, dinosaurs in the study of dinosaurs, quarks in the study of quarks and so on. Neo-positivism does not ask for a more ambitious kind of realism than the realism we already have. At most, it asks for the same kind of realism, but about other things – about nebulae as well as the Pacific Ocean, for example. Most importantly, it asks us to consider what happens to methods when they migrate from one thing to another. This should be acceptable to anti-realists of all kinds, methodological or otherwise.

88 For an expert treatment see Saatsi Juha (ed.), *The Routledge Handbook of Scientific Realism*, Boca Raton, FL: Routledge, 2017.

89 Margócsy, op. cit. (27).

90 Sujit Sivasundaram, 'Science', in David Armitage and Alison Bashford (eds.), *Pacific Histories: Ocean, Land, People*, London: Bloomsbury, 2014, pp. 237–60, 238.

## Positivism for the twenty-first century

Why does this matter? What are the wider implications of the neo-positivist theory of science? The question is worth asking because Comte asked it. The point of the positive philosophy was to understand the methods of natural science with a view to extending those methods to the study of human life. The science of 'social physics' or 'sociology' was designed to complete the great revolution of modernity that, in Comte's view, had begun in the Middle Ages and come to a head in the French Revolution. Comte outlined this plan in 1822.[91] A century later, the US historian James Harvey Robinson made a similar case for the history of science as a programme of social and intellectual reform, as James Secord has recently shown. Robinson wanted the history of science to 'play a pivotal role in educating a critical citizenry, the vital keystone in the survival of democracy'. Secord endorses the goal, writing that it has 'too often been abandoned' by historians of science.[92] Does neo-positivism help to revive it?

Yes, but not quite in the way that Robinson had in mind. The ideas in this article were not designed to support any of the causes that Secord finds in Progressive Era histories of science. This article was not designed to contribute to 'women's rights, racial equality, secular humanism, and global peace'.[93] Nor is this article a contribution to progressive causes in the present, such as the anti-capitalism and anti-colonialism that Secord hints at in the article in question.[94] I did not write this article because I 'want the nature of who has power in the present to be more equitably distributed than it has historically been', to quote an influential recent summary of the historian's mission.[95] I did not do this research to make the world more inclusive, sustainable or entrepreneurial, the official functions of the British history profession in 2023.[96] Like the authors of *Leviathan and the Air-Pump*, I was not concerned to 'defend or criticize any version of the Good Society'.[97] The intended function of this research was to understand past science. It was driven by what Michael Polanyi once called 'intellectual passion' and what Paula Findlen has called 'love of some arcane subject'.[98] I do not say that these motives are especially virtuous or veridical, just that they are no less so than any other motives. Ambitious intellectual projects do not need to be driven by ambitious social ones. We do not need to choose between antiquarianism and activism; we can choose neither.

Some would say that this is itself a political statement. Quite so. It is an instance of a broader point: if the humanities are going to survive in their current form, they need to be more than a vehicle for progressive politics. This is not an attack on progressive politics. Nor is it call for the excision of politics from scholarship. It is a call for a bipartisan defence of the humanities – a call to scholars of all political persuasions, including those of no settled persuasion, to explain why the organized study of history, philosophy, literature and the like is worth preserving. To be effective, the explanation cannot be a

91 See especially Auguste Comte, 'Plan des travaux scientifiques nécessaire pour réorganiser la société' (May 1822), in *Auguste Comte: Philosophie des sciences*, Paris: Gallimard, 1996, pp. 227–347.

92 James A. Secord, 'Inventing the scientific revolution', *Isis* (2023) 114(1), pp. 50–76, 76.

93 Secord, op. cit. (92), abstract.

94 Secord, op. cit. (92), abstract, pp. 75, 76.

95 Helen Carr and Suzannah Lipscomb, *What Is History, Now? How the Past and Present Speak to Each Other*, London: Weidenfeld and Nicolson, 2022, 10.

96 Subject Benchmark Statement: History, Quality Assurance Agency for Higher Education, 2022, pp. 5–7, at www.qaa.ac.uk/the-quality-code/subject-benchmark-statements/history# (accessed 20 March 2023).

97 Simon Schaffer and Steven Shapin, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton, NJ: Princeton University Press, 2011 (first published 1985), p. xvii.

98 Michael Polanyi, *Personal Knowledge: Towards a Post-critical Philosophy*, Routledge and Kegan Paul, 1958, pp. 133–5; Paula Findlen, 'Why go to grad school?', *Chronicle of Higher Education*, 17 November 2014, at www.chronicle.com/article/why-go-to-grad-school (accessed 20 March 2023).

restatement of the values of one band of the ideological spectrum. An argument for the humanities that takes progressive values for granted is only going to persuade progressives. It may not persuade even them, because there are many other institutions that are dedicated to progressive causes, from think tanks to political parties to publishing houses. Why spend three years and thousands of pounds on a degree in the history of science, when one can join an activist organization and make an immediate difference at a fraction of the cost? What can historians of science add to such causes? For that matter, what can the humanities as a whole add to them?

One answer is that we can make the conversation more rational. Crudely put, they make true beliefs more common than false ones. It is easy to scoff at this answer, but it is a good answer for anyone interested in the history of science as a public project in the middle decades of the twenty-first century. There are many reasons why 'post-truth' became the *Oxford English Dictionary*'s word of the year in 2016, but the underlying reason is that people care about the truth value of their beliefs.[99] Many of us, much of the time, prefer to have true beliefs than false ones. Hence the demand for advice on how to separate truths from falsehoods. Hence also the commercial success of that perennial genre, the self-help book for rationalists. Comte's *Course of Positive Philosophy*, and the popular lectures that brought Comte's ideas to the Parisian public, were arguably part of this tradition. James Harvey Robinson's *The Mind in the Making: The Relation of Intelligence to Social Reform*, first published in 1921, was a very successful work in the same tradition.[100] Exactly a century later, Steven Pinker published his *Rationality: What It Is, Why It Seems Scarce, Why It Matters*, another bestseller.[101]

Pinker is a scientist, a university professor, a classical liberal and a white North American male.[102] But it is a mistake to think that these are the only social groups with a stake in redefining rationality in the present. Eric Hayot is a professor of comparative literature, Emmanuel Eze a postcolonial philosopher of Nigerian heritage, Ben Burgis a socialist with a PhD in logic, Jonathan Marks a conservative political scientist, Anne Salmond an anthropologist and environmental activist, and Justin Smith-Ruiu a historian of philosophy who describes himself as a 'Christian anarchist'.[103] Yet they have all written books on rationality for a general audience in the last two decades. These books are not knee-jerk reactions to one-off events. They are serious reflections on rationality in an age of social media, political polarization, economic crisis, environmental decay and – not coincidentally – an age of much soul-searching about the value of the humanities.[104] Of

99 'Word of the Year 2016', *Oxford Languages*, https://languages.oup.com/word-of-the-year/2016 (accessed 21 March 2023).

100 James Harvey Robinson, *The Mind in the Making: The Relation of Intelligence to Social Reform*, New York: Harper, 1921; Secord, op. cit. (92).

101 Steven Pinker, *Rationality: What It Is, Why It Seems Scarce, Why It Matters*, London: Viking, 2021. New York Times Best-Sellers, 17 October 2021, at www.nytimes.com/books/best-sellers/2021/10/17/combined-print-and-e-book-nonfiction (accessed 21 March 2023).

102 Pinker identifies himself as a 'classical liberal' in his *Enlightenment Now: The Case for Reason, Science, Humanism and Progress*, London: Penguin, 2018, p. 15.

103 Ben Burgis, *Give Them an Argument: Logic for the Left*, Winchester: Zero Books, 2019; Emmanuel Chukwudi Eze, *On Reason: Rationality in a World of Cultural Conflict and Racism*, Durham, NC: Duke University Press, 2008; Anne Salmond, *Tears of Rangi: Experiments across Worlds*, Auckland: University of Auckland Press, 2020; Justin Smith-Ruiu (writing as Justin E.H. Smith), *Irrationality: A History of the Dark Side of Reason*, Princeton, NJ: Princeton University Press, 2020; Jonathan Marks, *Let's Be Reasonable: A Conservative Case for Liberal Education*, Princeton, NJ: Princeton University Press, 2021; Eric Hayot, *Humanist Reason: A History, an Argument, a Plan*, New York: Columbia University Press, 2021 In the same genre, see Michael W. Clune, *A Defense of Judgment*, Chicago: The University of Chicago Press, 2021.

104 Academic soul-searching is a vast genre, but here are some works that (along with those in the previous note) have informed the present section. Justin Smith-Ruiu (writing as Justin E.H. Smith), 'The moral contortions

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course, we are not going to solve all the world's problems by writing down rules of method. But we can at least try to defend the island of sanity that is the humanities. One way to do this is to show that the humanities are a rational enterprise. In Hayot's words, we can 'reclaim and redescribe the work of humanist thought and humanist scholarship as a form of reason, as a form of truth-seeking'.[105]

Historians of science ought to have something to give to this project. It must be said, however, that the field has an awkward relationship with rationality. The main thrust of the 'new history of science' that Secord identified in 1993 has been to show that intellectual life depends on social life.[106] This is a major achievement, but it has often come at the expense of *reducing* intellectual life to social life. It is hard to argue that truth is one product of the humanities when we are told that truth is not even a product of the sciences.[107] It is hard to defend the history profession on epistemic grounds when prominent historians treat epistemology as something that is best avoided.[108] We may need to look beyond the new history of science for guidance on how to be a rational humanist in a divided world.

Where might we look? Well, we are looking for an intellectual tradition that has robust notions of truth, evidence and rationality. The tradition needs to be politically engaged but politically versatile, with the potential to appeal to the hard left, the hard right and everything in between. It needs to follow science closely but not slavishly. It must have some affinity with the language of 'facts', 'big data' and 'lived experience' that shapes so much folk epistemology today. Since the aim is to defend the humanities, the tradition should have some affinity with history, philosophy and literary scholarship, and with the distinctiveness of the methods and subject matters of these disciplines. Since the aim is to say something new and provocative, a certain amount of iconoclasm is desirable. In fact, a tradition that we have been debunking since the 1960s would be an ideal candidate.

Positivism, anyone?

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of the New University', *Chronicle of Higher Education*, 1 December 2022, at www.chronicle.com/article/the-moral-contortions-of-the-new-university (accessed 22 March 2023); Niall Ferguson, 'I'm helping to start a new college because higher ed is broken', *Bloomberg Opinion*, 8 November 2021, at www.bloomberg.com/opinion/articles/2021-11-08/niall-ferguson-america-s-woke-universities-need-to-be-replaced (accessed 23 March 2023); 'University and Arts Council in drive to re-brand "soft" academic subjects', *The Observer*, 21 June 2020, at www.theguardian.com/education/2020/jun/21/university-and-arts-council-in-drive-to-re-brand-soft-academic-subjects (accessed 23 March 2023); Helen Small, *The Value of the Humanities*, Oxford: Oxford University Press, 2013; Boaventura de Sousa Santos, *Decolonising the University: The Challenge of Deep Cognitive Justice*, Newcastle upon Tyne: Cambridge Scholars Publishing, 2017.

105 Hayot, op. cit. (103), p. 9.

106 Secord, op. cit. (1), p. 388.

107 Steven Shapin, 'Lowering the tone in the history of science: a noble calling', in Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority*, Baltimore, MD: Johns Hopkins University Press, 2010, pp. 1–14, 5–6.

108 For some examples see Michael Bycroft, 'Good as gold', in Dániel Margócsy and Richard Staley, *The Mantis Shrimp: A Simon Schaffer Festschrift*, Cambridge: The HPS Collective, 2022, pp. 231–4.

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