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RETROSPECT

SCIENCE IN HISTORY*

Charlie Dunbar Broad, 'The new philosophy: Bruno to Descartes', [*Cambridge Historical Journal*], 8 (1944), pp. 36–54.

Henry Dale, 'Experiment in medicine', [*Cambridge Historical Journal*], 8 (1946), pp. 166–78.

Quentin Skinner, 'Thomas Hobbes and the nature of the early Royal Society', 12 (1969), pp. 217–39.

Roy MacLeod, 'The Royal Society and the government grant: notes on the administration of scientific research, 1849–1914', 14 (1971), pp. 323–58.

Roy Porter, 'Gentlemen and geology: the emergence of a scientific career, 1660–1920', 21 (1978), pp. 809–36.

John Morgan, 'Puritanism and science: a reinterpretation', 22 (1979), pp. 535–60.

Lawrence Goldman, 'The origins of British 'social science': political economy, natural science and statistics, 1830–1835', 26 (1983), pp. 587–616.

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ABSTRACT. *What is the history of science? How has it changed over the course of the twentieth century? And what does the future hold for the discipline? This 'Retrospect' provides an introduction to the historiography of science as it developed in the Anglophone world. It begins with the foundation of the Cambridge History of Science Committee in the 1940s and ends with the growth of cultural history in the 2000s. At the broadest level, it emphasizes the need to consider the close relationship between history and the history of science. All too often the historiography of science is treated separately from history at large. But as this essay shows, these seemingly distinct fields often developed in relation to one another. This essay also reveals the ways in which Cold War politics shaped the history of science as a discipline. It then concludes by considering the future, suggesting that the history of science and the history of political thought would benefit from greater engagement with one another.*

Science has a history. There was once a time when such a statement might have proved controversial. Science was thought of as a collection of timeless truths produced by great men united by their genius: Aristotle, Galileo, Newton, and Darwin. However, with the formation of the history of science as a professional discipline in the early twentieth century, this picture began to change.[1] Science started to be thought of as a human activity, with a history just like any other. Still, what exactly the history of science should consist of was open to debate. The collection of articles featured in this 'Retrospect' provides an opportunity to reflect on the most significant changes in the historiography of science which have taken place over the past seventy years.

Drawing on the archive of the *Historical Journal*, it is largely an Anglophone historiography, although connections to other European traditions are highlighted throughout. It begins in the 1940s, with the formation of the Cambridge History of Science Committee, moves through the intellectual history of the 1960s, the social history of the 1970s, and the cultural history of the 1990s and 2000s. In the process, I make four core arguments. First, historians of science need to address the relationship between their discipline and history. They have spent a lot of time reflecting on the connection between the history of science and other disciplines, particularly philosophy, anthropology, and sociology.[2] This is important, as each of these disciplines played a significant role in the development of the historiography of science, as we will see. But historians of science have spent much less time considering how their

[1] For eighteenth and nineteenth-century traditions in the history of science, see Hendrik Floris Cohen, *The scientific revolution: a historiographical inquiry* (Chicago, IL, 1994), pp. 24–52.

[2] Steven Shapin, 'History of science and its sociological reconstructions', *History of Science*, 20 (1982), pp. 157–211; Emily Martin, 'Anthropology and the cultural study of science', *Science, Technology and Human Values*, 23 (1998), pp. 24–44; and Michael Friedman, 'History and philosophy of science in a new key', *Isis*, 99 (2008), pp. 125–34.

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discipline relates to the wider historical profession.[3] What exactly is the *history* in the history of science? How did changes in intellectual history, social history, and cultural history affect what historians of science were doing? Second, this essay argues that the reverse is also true. General historians need to consider the role that the history of science has played in the development of their own discipline. Many of the major developments in the practices of writing history in the twentieth century had their origins in debates concerning the history of science. Herbert Butterfield's Whig interpretation of history, Quentin Skinner's political thought, Asa Briggs's social history, and Roger Darnton's cultural history were all products of engagement with the history of science. Third, this essay reveals a broad shift in the disciplinary background of practitioners. In the 1940s, the history of science was written by scientists. Today, it is written by professional historians. Charting that change is key to understanding how the discipline of the history of science emerged. Finally, this essay reveals the ways in which twentieth-century politics, particularly relating to the Cold War, transformed the historiography of science. Much of the debate over the nature of science and its past, as well as how it should be studied, reflected divergent attitudes towards capitalism and socialism. This raises important questions about the nature of the history of science today. Now the Cold War is over, what is the history of science for and how should it be written? This is a question I return to in the conclusion.

I

Charlie Dunbar Broad was neither a scientist nor a historian. Why then, in March 1944, was he invited to give a lecture at the University of Cambridge as part of a series on 'Science in the Sixteenth and Seventeenth Centuries'? With no formal historical training, Broad was closer to what we would now call a philosopher of science. Although he entered Trinity College to study Natural Sciences in 1906, he quickly switched to the Moral Sciences Tripos, specializing in epistemology and the philosophy of the mind. Later, as Knightsbridge Professor of Moral Philosophy at Cambridge, he published books with titles such as *The mind and its place in nature* (1925).[4] He also published a widely read introduction to the philosophy of science entitled *Scientific thought* (1923). This book – a product of Broad's time lecturing undergraduate scientists whilst at the University of Bristol – set out the philosophical consequences of recent developments in physics, including Einstein's theory of

[3] For earlier discussions of this relationship, see Arnold Thackray, 'Science, technology, and medicine', *Journal of Interdisciplinary History*, 12 (1981), pp. 299–314, at pp. 305–6; and Alfred Rupert Hall, 'Can the history of science be history?', *British Journal for the History of Science*, 4 (1969), pp. 207–20.

[4] Robert Yost, 'The philosophy of C. D. Broad', *Philosophical Review*, 77 (1968), pp. 474–90; and Thomas Redpath, 'Cambridge philosophers VIII: C. D. Broad', *Philosophy*, 72 (1997), pp. 571–94.

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relativity.[5] Intriguingly, alongside his appreciation of Einstein, Broad maintained a life-long interest in psychical research, taking seriously reports of 'paranormal cognition' and 'telepathy'.[6] Today, Broad is largely forgotten. He is not someone historians or scientists tend to read. His eclectic mix of science, philosophy, and the paranormal makes him an uneasy fit for the disciplinary and professional identities which developed over the course of the twentieth century.[7] Yet this is precisely why Broad is such a good starting point. He is representative of the contested nature of the history of science as a discipline in the 1940s. What was the history of science for? And who should write it? In the early twentieth century, the answers to these questions were far from obvious. Historians, philosophers, and scientists, amongst others, all tried to shape the meaning of science, both past and present.

Broad delivered his lecture, entitled 'The new philosophy: Bruno to Descartes', at the invitation of the History of Science Committee at Cambridge. The lecture then formed the basis for an article published later that year in the *Cambridge Historical Journal*. In the article, Broad described a major transition in how the world was understood in the sixteenth and seventeenth centuries. He characterized this as a shift from the 'old philosophy' to the 'new philosophy'. The 'old philosophy', according to Broad, was grounded in the work of Aristotle and other ancient Greek thinkers. Within this worldview, there was no need for experiments. Ancient and medieval scholars instead made deductive inferences based on the properties associated with the four elements: fire, earth, wind, and water. In contrast, the 'new philosophy' rejected this way of doing things. Instead, men like Galileo and Francis Bacon turned to experiment in order to understand the natural world. And, according to Broad, this 'combination of reasoning and experiment' was 'characteristic of modern science'.[8]

Broad was not alone in his understanding of the sixteenth and seventeenth centuries as a period of monumental change. Often referred to as the 'scientific revolution', many in the early twentieth century saw this as a period in which both modern science and modern society emerged. Writing in *The making of the modern mind* (1926), the philosopher John Herman Randall Jr argued that 'it was not humanism, and it was not the Reformation, that was destined to

[5] Charlie Dunbar Broad, *Scientific thought* (London, 1923), pp. 3–6.

[6] Charlie Dunbar Broad, *Lectures on psychical research* (London, 1962). On the history of the Society for Psychical Research, of which Broad acted as president between 1934–5 and 1958–60, see Andreas Sommer, 'Psychical research in the history and philosophy of science', *Studies in the History and Philosophy of Science*, 48 (2014), pp. 38–45; and John Forrester and Laura Cameron, *Freud in Cambridge* (Cambridge, 2017), p. 362.

[7] Broad does feature in Forrester and Cameron, *Freud in Cambridge*, pp. 109, 348, 362, 487, as part of the history of psychoanalysis and psychology. He also appears briefly in Ray Monk, *Bertrand Russell: the ghost of madness, 1921–1970* (London, 2000), pp. 201 and 207, as part of the history of analytic philosophy.

[8] Charlie Dunbar Broad, 'The new philosophy: Bruno to Descartes', *Cambridge Historical Journal*, 8 (1944), pp. 36–54, at pp. 36–44.

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work the greatest revolution in the beliefs of men…it was science'.[9] Similar views were expressed by Preserved Smith in the 1930s and Alexandre Koyré in the 1940s.[10] Indeed, in many ways the history of science as a discipline formed around the concept of the 'scientific revolution'.[11] Yet, beneath this apparent consensus, there was much disagreement over how seventeenth-century science should be interpreted. What exactly was the source of change? And how did it manifest itself? These questions would continue to occupy historians of science right throughout the twentieth century.[12]

Broad was writing at a time when the history of science as a discipline, particularly in Cambridge, was undergoing a period of major transition. The original History of Science Committee had been established in 1936 by a group of scientists led by the biochemist Joseph Needham. Alongside Needham, the Committee included the Marxist crystallographer John Desmond Bernal as well as the pathologist Walter Pagel, a Jewish refugee who had fled Nazi Germany in 1933.[13] Although they differed in their politics, the early members of the Committee were united in seeing the history of science as a way of addressing contemporary problems. Needham worried about overspecialization in science, and the lack of moral awareness amongst scientists in an age of industrial war.[14] Bernal had similar concerns, later writing that 'the troubles of the times…have focused attention on the historical aspect of science'.[15] Pagel, although more conservative than Needham and Bernal, also saw the history of science as part of a political project, one to rescue civilization in an age of uncertainty.[16] Together, they represented what the Harvard historian of science George Sarton called the 'new humanism'. They were committed to scientific progress, but demanded that science be examined as part of society. Only by educating scientists in the humanities, and humanists in the sciences, could civilization survive in wake of the First World War. With this aim in mind, the Committee established a series of annual lectures, open to all members of the university. The dream, for this

[9] Ierome Bernard Cohen, *Revolution in science* (Harvard, MA, 1985), pp. 389–95.

[10] Alexandre Koyré, 'Galileo and Plato', *Journal of the History of Ideas*, 4 (1943), pp. 400–28; Alfred Rupert Hall, 'Alexandre Koyré and the scientific revolution', *History and Technology*, 4 (1987), pp. 485–96; Floris Cohen, *Scientific revolution*, pp. 73–87, Cohen, *Revolution in science*, p. 395.

[11] Floris Cohen, *Scientific revolution*, p. 2

[12] On debates surrounding the causes and consequences of the scientific revolution, see ibid., pp. 239–377.

[13] Anna-K. Mayer, 'Setting up a discipline: conflicting agendas of the Cambridge History of Science Committee, 1936–1950', *Studies in the History and Philosophy of Science*, 31 (2000), pp. 665–89.

[14] Anna-K. Mayer, 'Setting up a discipline II: British history of science and "the end of ideology", 1931–1948', *Studies in the History and Philosophy of Science*, 35 (2004), pp. 41–72, at pp. 46–8.

[15] John Desmond Bernal, *Science in history* (London, 1954), p. vii.

[16] Mayer, 'Setting up a discipline II', p. 48.

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generation, was that the 'unity of knowledge' would produce the 'unity of mankind'.[17]

Not everyone, however, was convinced that the history of science should be left to scientists. Butterfield, who at the time was editor of the *Cambridge Historical Journal*, attended some of the early lectures organized by the History of Science Committee in the 1930s. He was not impressed.[18] Writing in *The Whig interpretation of history* (1931), Butterfield was famous for his attack on present-centred political and constitutional histories.[19] For Butterfield, historians of science were guilty of the same mistakes. The early lectures, particularly those given by leading scientists such as Ernest Rutherford and William Henry Bragg, placed science 'in too modern a context'.[20] Echoing *The Whig interpretation of history*, Butterfield wrote that 'the present-day scientist may only be interested in the past in so far as it provides these likenesses to the present; but the historian can almost be defined by the fact that he is interested in the unlikenesses'. As it stood, Butterfield saw the history of science as 'a kind of defective antiquarianism' charting 'the discoveries of one great genius to those of another'.[21] Something had to be done. Sure enough, Butterfield's opportunity came in 1942 when Needham left Cambridge as part of a scientific mission to China. With the chair of the History of Science Committee vacant, Butterfield stepped in, determined to 'set the subject on its feet'. On taking the chair, Butterfield radically changed the make-up of the Committee. Rather than being dominated by scientists, Butterfield shifted the emphasis towards the humanities, and particularly history. The new Committee included the economic historian Michael Postan and the historian of Renaissance literature Basil Willey.[22]

How then did Broad fit into this? For a start, it is clear that he was invited to give the lecture in 1944 by Butterfield rather than Needham. And it was Butterfield who arranged for the publication of the subsequent article in the *Cambridge Historical Journal*. This reflected Butterfield's commitment to treat the history of science as part of 'the general history of thought' rather than a separate discipline run by scientists.[23] Significantly, this was the first time Butterfield had published one of the History of Science Committee lectures, despite the series having been in operation for nearly a decade at this point. With all this in mind, we can read Broad's article as part of a struggle

[17] George Sarton, 'The new humanism', 6 (1924), pp. 9–42, at p. 9.

[18] Michael Bentley, *The life and thought of Herbert Butterfield: history, science and God* (Cambridge, 2011), p. 187.

[19] Herbert Butterfield, *The Whig interpretation of history* (London, 1931), p. vi.

[20] Bentley, *Life and thought*, p. 187; and Joseph Needham and Walter Pagel, eds., *Background to modern science* (Cambridge, 1938).

[21] Hertbert Butterfield, 'The historian and the history of science', *Bulletin of the British Society for the History of Science*, 1 (1950), pp. 49–58, at pp. 51–4. See Butterfield, *The Whig interpretation*, p. 12, for comparable use of the terms 'likeness' and 'unlikeness'.

[22] Bentley, *Life and thought*, pp. 188–9.

[23] Butterfield, 'The historian and the history of science', p. 57.

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between scientists like Needham and historians like Butterfield over who should write the history of science and how. Unsurprisingly, Broad's argument in many ways mirrored Butterfield's approach to the history of science. The article opens by cautioning against seeing seventeenth-century science in terms of the present alone. Broad argues that 'it is necessary to devote what may seem to be a disproportionate part of to-day's lecture to the old philosophy, simply because the victory of the new was in the end so complete and has for so long been unchallenged'. He went on to warn that the old philosophy 'taken out of its context, seems to be a mere childish fairy-tale too ridiculous to have ever been sincerely believed'. At the same time, the new philosophy 'when explicitly stated, seems so trite and trivial that we cannot understand why the innovators made such a fuss'. Treating both the old and new philosophy on their own terms, according to Broad, was the key to understanding seventeenth-century science.[24]

Reading Broad's article today it is easy to miss the influence of Butterfield's approach to history. Particularly as, despite all the appeals to historicism, Broad in fact comes across as incredibly Whiggish. Throughout the article, Broad repeatedly celebrates the achievements of modern science. He identifies Galileo as a 'genius' and Descartes as being 'the first person to have stated this law [of inertia] fully and correctly'. Much of the article is spent trying to 'sum up the strong and weak points' of various philosophies, effectively marking the homework of past thinkers.[25] Additionally, Broad makes absolutely no reference to social or political contexts, something both scientists such as Needham and historians such as Butterfield saw as fundamental to the history of science. Marxist historians of science in particular, like Bernal, would also have been shocked by the lack of attention to technology and manual work, something they saw as part and parcel of the history of ideas. Finally, Broad's article relies on a fundamental break between the 'old' and 'new' philosophies of the seventeenth century. By the end of the twentieth century, the revolutionary nature of the scientific revolution was increasingly drawn into question.[26] But even in the 1940s, there were many historians and historians of science who did not see the Renaissance or the Reformation, along with the accompanying scientific movements of the time, in such progressive and revolutionary terms.[27]

Broad's article therefore presents a paradox: published by Butterfield and rejecting presentism, yet seemingly Whiggish nonetheless. With this in mind, Broad's article is best read as an uneasy compromise between a number of

[24] Broad, 'The new philosophy', p. 36.

[25] Ibid., pp. 45–52.

[26] For a classic statement, see the first line of Steven Shapin, *The scientific revolution* (Chicago, IL, 1996), p. 1.

[27] A more gradualist understanding to scientific change characterized the work of a number of early twentieth-century thinkers including the philosopher Pierre Duhem, the historian of science George Sarton, and the physicist Ernest Rutherford; Floris Cohen, *Scientific revolution*, pp. 45–52; and Cohen, *Revolution in science*, p. 389.

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competing approaches. There is clearly an element of Butterfield's philosophy of history in there. However, it is important to remember that what counted as Whiggish in the 1940s was not the same as today. Butterfield did not reject progress or revolutionary change outright, he simply worried that Whig history, like Marxist history, made progress seem inevitable. After all, Butterfield himself famously declared in his *The origins of modern science* (1949) that the scientific revolution 'outshines everything since the rise of Christianity'. What Butterfield wanted was a history in which the causes of progress were properly examined.[28] This meant attending to 'the misfires and mistaken hypotheses of early scientists' as well as their successes.[29] Broad's apparently progressive narrative was therefore not necessarily at odds with Butterfield's approach. But beyond this, it is important to recognize that Broad, by his own admission, was 'a professional philosopher, and not a historian'. Trained in the British Idealist tradition by John McTaggart at Trinity, Broad was fundamentally interested in how the history of ideas could shed light on philosophical problems. He was part of a generation of philosophers, including Bertrand Russell and G. E. Moore, who combined an Idealist training with a move into the developing field of analytic philosophy.[30] In many ways, Broad's article, as well as his monographs on Newton and Bacon, read more like Russell's *A history of Western philosophy* (1945) than anything Butterfield wrote.[31] The article is a genealogy of ideas, aimed at illuminating contemporary issues surrounding the nature of science and the physical world. Broad's preoccupation with the detail of ideas and how they connected to one another therefore makes perfect sense within the tradition of analytic philosophy he practised. As Broad himself explained in *Scientific thought*, the aim of philosophy was to help 'clear up some of the concepts used in the natural sciences'. Broad was in fact much more interested in collaborating with scientists than historians. Reading Broad in light of the analytic philosophy of the time helps make sense of what he was trying to achieve in 1944. Yes, he adopted Butterfield's approach to the study of history, particularly the emphasis on studying ideas which were subsequently rejected. But Broad also explicitly advanced a series of methodological and philosophical claims concerning the sciences in the present. As he concluded in *Scientific thought*, 'the co-operation of philosophers and scientists is of the utmost benefit to the studies of both'.[32]

[28] Nick Jardine, 'Whigs and stories: Herbert Butterfield and the historiography of science', *History of Science*, 41 (2003), pp. 125–40, at pp. 125–31.

[29] Herbert Butterfield, *The origins of modern science* (London, 1949), p. ix

[30] Nicholas Griffin, 'Russell and Moore's revolt against British Idealism', in Michael Beaney, ed., *The Oxford handbook of the history of analytic philosophy* (Oxford, 2013).

[31] Charlie Dunbar Broad, *The philosophy of Francis Bacon* (Cambridge, 1926); and Charlie Dunbar Broad, *Sir Isaac Newton* (London, 1926). On the relationship between Broad and Russell, see Ray Monk, *Ludwig Wittgenstein: the duty of genius* (London, 1990), p. 39; and Monk, *Bertrand Russell*, pp. 201 and 207.

[32] Broad, *Scientific thought*, pp. 12–25.

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II

When Needham returned to Cambridge in 1948, he found the History of Science Committee had been transformed. Writing to a friend, he complained that 'the committee seems to have become dominated by professional historians'.[33] Needham was right. By the end of the Second World War, the History of Science Committee under Butterfield was composed largely of trained historians. This was in stark contrast to the 1930s when the Committee had been founded largely as a venue for scientists.[34] But the difference between Butterfield's and Needham's vision for the history of science was not just a question of disciplinary training. Butterfield and Needham clashed politically, and as a consequence, promoted very different intellectual traditions. Needham, like his colleague Bernal, was a Marxist. Butterfield was not. As a conservative Christian, Butterfield rejected Marxist history as dangerous and unrealistic. The essential fallibility of human nature, Butterfield argued, precluded utopian schemes for the improvement of society.[35] In terms of the sciences, this was part of a broader disagreement between conservative and socialist thinkers on the relationship between science and society. Socialist scientists like Needham and Bernal, often looking to the Soviet Union, promoted the idea that Britain should adopt a system of scientific and technical planning.[36] Writing in *The social function of science* (1939), Bernal attacked the notion of 'pure science' as 'a form of snobbery'. The ideal of the disinterested observer had turned science into nothing more than 'an amusing pastime'. And without sufficient planning, science was failing to address the 'problems of production or of welfare'. Whilst Bernal admitted that 'science can never be administered as part of the civil service', he nonetheless looked to the Soviet Union as an example of how state-led planning could transform academic research for the betterment of society.[37]

Bernal had his supporters, but there was also plenty of opposition, both within and beyond the scientific profession. Liberals and conservatives worried that a planned society would stifle scientific creativity. In response, a group of scientists led by the chemist Michael Polanyi and the zoologist John Baker founded the Society for Freedom in Science. Throughout the 1940s,

[33] Bentley, *Life and thought*, p. 189.

[34] Mayer, 'Setting up a discipline', p. 676.

[35] Reba Soffer, *History, historians and conservatism in Britain and America: from the Great War to Thatcher and Reagan* (Oxford, 2008), pp. 180–6.

[36] Mary Jo Nye, *Michael Polanyi and his generation: origins of the social construction of science* (Chicago, IL, 2011), pp. 184–222; Mayer, 'Setting up a discipline', pp. 676–84; and Mayer, 'Setting up a discipline II', pp. 41–60.

[37] John Desmond Bernal, *The social function of science* (London, 1939), pp. xiii and 96–7. Other leading advocates of scientific planning in this period include the journalist James Crowther and the Marxist mathematician and founding member of the British Society for the History of Science Samuel Lilley; Steven Shapin, 'Discipline and bounding: the history and sociology of science as seen through the externalism–internalism debate', *History of Science*, 30 (1992), pp. 333–69, at p. 339.

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they defended the value of 'pure science', arguing that 'science can only flourish and therefore can only confer the maximum cultural and practical benefits on society when research is conducted in an atmosphere of freedom'.[38] Crucially, many of the key figures on the History of Science Committee under Butterfield were also members of the Society for Freedom in Science. These included historians such as Michael Postan as well as the few remaining scientists such as the physicists William Dampier and Herbert Dingle.[39] The History of Science Committee, therefore, was dominated by a very specific politics as well as intellectual approach. It was a venue for conservative and liberal historians and scientists to promote a history of science which ran counter to socialists such as Needham and Bernal. This was also reflected in the pages of the *Cambridge Historical Journal*.

Amongst the members of the Society for the Freedom of Science was the leading biochemist Henry Hallett Dale. Dale had won the Nobel Prize in Physiology or Medicine in 1936 for his work on neurotransmission and spent most of his working life as chairman of the Wellcome Trust, overseeing the funding of medical research. He was intimately involved in the politics of science, serving as both president of the Royal Society and on the government's Scientific Advisory Panel during the Second World War.[40] And, whilst Dale recognized the importance of scientific collaboration with the Soviet Union during the war, he nonetheless vigorously defended the value of pure science against the kind of planned scientific enterprise promoted by socialists like Bernal.[41] Dale was therefore someone that could combine genuine scientific expertise along with a commitment to the values that Butterfield and the Society for Freedom in Science shared. With this in mind, Butterfield invited Dale to deliver a lecture as part of the 1946 programme organized by the History of Science Committee. And once again, Butterfield arranged for the subsequent publication in the *Cambridge Historical Journal*.

Dale's lecture was on the subject of 'Experiment in medicine', a largely celebratory account of the rise of physiology and biochemistry. Dale dated the rise of what he called 'experimental medicine' to the late nineteenth century, with the work of the French bacteriologist Louis Pasteur and the German physiologist Carl Ludwig. He then explained that modern experimental techniques, alongside the development of laboratories, had 'opened the new era of medical progress in which we are living to-day'. The public had experimental physiologists to thank for antibiotics, vitamins, and hormone treatments. With

[38] William McGucken, 'On freedom and planning in science: the Society for Freedom in Science, 1940–1946', *Minerva*, 16 (1978), p. 48.

[39] Mayer, 'Setting up a discipline II', p. 58.

[40] Elizabeth Tansey, 'Charles Sherrington, E. D. Adrian, and Henry Dale: the Cambridge Physiological Laboratory and the physiology of the nervous system', in Peter Harman and Simon Mitton, eds., *Cambridge scientific minds* (Cambridge, 2002), pp. 193–6.

[41] Henry Dale, 'International collaboration and freedom of science', *Nature*, 148 (1941), pp. 678–80.

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a patriotic flourish, Dale also highlighted 'our own British Schools of Experimental Physiology'. It was not just the Germans or the French, but 'the inspired enterprise of our own countrymen' that was responsible for the most recent advances in experimental medicine such as penicillin. Setting out the genealogy of British experimentalists, Dale identified a group of researchers at Cambridge including Michael Foster, Charles Sherrington, and John Langley who had spearheaded 'the revival and initiation of the experimental method'. 'Their pupils played an important role in putting British physiology high in the world's esteem', explained Dale.[42] (As his audience would have well known, Dale *was* one of those students, having studied under Langley and Foster.)[43] Dale also emphasized the political importance of experimental medicine in the present. He asked his audience to compare the casualties lost to 'enteric fever in the South African war, with the experience in the war just ended, in which inoculation had eliminated these scourges as significant military problems'. And it was the development of new drugs, such as mepacrine, 'which made it possible for the armies to fight in the malarial swamps of New Guinea, when the Japanese seized the world's major sources of quinine'.[44]

So far, so celebratory. Dale's was a history of experimental medicine in which 'individual workers of genius' – implicitly men like him – had produced unprecedented intellectual progress, and effectively won the Second World War in the process. But Dale also asked a more searching question, one that directly related to the politics of the Society for Freedom in Science. Like Butterfield, Dale was not just interested in cataloguing progress, but understanding its causes. For this reason, Dale opened his lecture, not with the nineteenth century, but with the sixteenth. He began by examining a much earlier history of experimental medicine with the work of the English physicians William Gilbert and William Harvey. Dale identified Gilbert and Harvey as 'the leaders in this country of the scientific revolution'. Together, they pioneered 'the method of direct, experimental enquiry', with Dale crediting Harvey for discovering 'the double circulation of the blood'. Why then, given these examples, did Dale argue that the rise of experimental medicine should be dated to the nineteenth century and not the sixteenth? In his view, despite the work of Gilbert and Harvey, progress in science and medicine had been 'slow and capricious' since the sixteenth century. The experimental method had not permeated the Royal College of Physicians, or the world of medicine at large. Rather, experimental medicine after Harvey had fallen 'so largely into abeyance during more than two centuries'. Explaining this was the core motivation behind Dale's lecture. He contrasted early modern and modern experimental medicine in order to interrogate the causes of scientific progress. Like Broad, this provided a means to reconcile Butterfield's philosophy of

[42] Henry Dale, 'Experiment in medicine', *Cambridge Historical Journal*, 8 (1946), pp. 166–78.
[43] Tansey, 'Charles Sherrington, E. D. Adrian, and Henry Dale', pp. 193–6.
[44] Dale, 'Experiment in medicine', p. 177.

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history with a celebratory account of scientific advance. There had been a 'lapse of progress' before the 'outburst of experimental activity…in the middle of the nineteenth century'. The question was: what had changed?[45]

In answering this question, Dale implicitly attacked the Marxist histories promoted by Needham and Bernal. Dale, as a member of the Society for Freedom in Science, wanted to ensure society valued pure science. Indeed, Dale had promoted this ideal at the Wellcome Physiological Laboratories, encouraging scientists to produce experimental results and collect data apparently without particular commercial outcomes in mind.[46] For Dale then, the problem with early modern science was not a lack of 'genius', but the lack of freedom. Harvey lived in a world dominated by 'orthodoxy and ancient tradition'.[47] The same was true of the eighteenth century. Dale singled out the 'plausible but fallacious phlogiston hypothesis'. This theory, which explained combustion through the existence of a fire-like particular rather than oxidation, had 'seized the minds of [a] generation with the force of a dogma'. Without free thinking, Dale argued, eighteenth-century chemistry had become dominated by a theory which 'distorted the interpretation of all chemical observations'.[48]

For Dale, the factors which had held back early modern science – dogma and distortion – were also on the rise in the present. Dale used his 1941 Presidential Address to the Royal Society to caution against allowing science to 'become entangled with controversial politics'. This would lead to a world in which 'the rigid standards of true science would be relaxed…allowing the convenience of results for policy or for propaganda to enter into the assessment of their validity as evidence'. At the same time, 'fundamental researches, having no immediately practical appeal, would be allowed to fall into arrears through relative neglect'.[49] In Dale's view, this was exactly what was happening in the Soviet Union. Throughout the 1930s and 1940s, the agricultural biologist Trofim Lysenko, with Stalin's backing, orchestrated a campaign to discredit scientific theories deemed in conflict with revolutionary Marxism. Mendelian genetics in particular was identified as 'bourgeois pseudoscience', with a number of leading Russian scientists being sent to prison and labour camps.[50] When Dale learned of the imprisonment and death of the geneticist Nikolai Vavilov, he wrote to the Soviet Academy of Science in protest. Resigning his membership in 1948, Dale complained:

[45] Ibid., pp. 167–72.
[46] Tansey, 'Charles Sherrington, E. D. Adrian, and Henry Dale', p. 195.
[47] Dale, 'Experiment in medicine', p. 167.
[48] Ibid., p. 168.
[49] Henry Dale, 'Anniversary address by Sir Henry Dale', *Proceedings of the Royal Society*, 179 (1942), pp. 254–5.
[50] For the history of Soviet genetics in this period, see David Joravsky, *The Lysenko affair* (Harvard, MA, 1970); Loren Graham, *Science in Russia and the Soviet Union* (Cambridge, 1993), pp. 121–36; and Michael Gordin, *The pseudoscience wars: Immanuel Velikovsky and the birth of the modern fringe* (Chicago, IL, 2012), pp. 79–105.

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This is not the result of an honest and open conflict of scientific opinions; Lysenko's own claims and statements make it clear that his dogma had been established and enforced by the Central Committee of the Communist Party, as conforming to the political philosophy of Marx and Lenin…Since Galileo was driven by threats to his historic denial, there have been many attempts to suppress or to mutilate scientific truth in the interests of some extraneous creed, but none has had a lasting success.[51]

Here, we see how Dale brought together his understanding of scientific freedom in the past and present. Whether in the twentieth century or the sixteenth century, the enemy of scientific progress was 'dogma', a word Dale returned to time and time again. Progress could only be secured, Dale argued, if scientists were free to conduct research without political or practical ends in mind.

III

By the end of the 1940s, the history of science was dominated by the history of ideas.[52] In Paris, Alexandre Koyré published his influential *Études galiléennes* (1939), arguing that Galileo's work was best understood as an intellectual project, part of the 'geometrization of space', rather than a response to early modern manufacturing concerns. 'Galileo did not learn his business from people who toiled in the arsenals and shipyards of Venice', concluded Koyré.[53] In the United States, George Sarton cultivated a similar philosophy of history in his three-volume *Introduction to the history of science* (1927–48). Sarton even suggested collaborating with Arthur Lovejoy, founder of the *Journal of the History of Ideas*, in which a number of Koyré's essays later appeared.[54] Back in Cambridge, Alfred Rupert Hall was appointed to the first lectureship in the history of science in 1950.[55] A trained historian, rather than a scientist, Hall too placed the emphasis on ideas over industry. Hall's *Ballistics in the seventeenth century* (1952) followed Koyré in denying the link between the technologies of early modern warfare and new developments in the sciences.[56] Socialists such as the journalist James Crowther and the mathematician Samuel Lilley certainly continued to press for a history which emphasized the relationship between science, society and industry. But they were in the minority, and largely excluded from new

[51] Quoted in Andrew Brown, *J. D. Bernal: the sage of science* (Oxford, 2006), p. 304.

[52] Shapin, 'Discipline and bounding', p. 342.

[53] James Stump, 'History of science through Koyré's lenses', *Studies in the History and Philosophy of Science*, 32 (2001), pp. 243–63, at pp. 244–8.

[54] John Clark, 'Intellectual history and the history of science', in Richard Whatmore and Brian Young, eds., *A companion to intellectual history* (Chichester, 2016), p. 155; Shapin, 'Discipline and bounding', p. 341; and Simon Schaffer, 'Lovejoy's series', *History of Science*, 48 (2010), pp. 483–94, at p. 485.

[55] Alfred Rupert Hall, 'Beginnings in Cambridge', *Isis*, 75 (1984), pp. 22–5.

[56] Clark, 'Intellectual history and the history of science', pp. 162–4.

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university positions designed to cement the status of the history of science as a discipline.[57]

The history of ideas continued to dominate, not just the history of science, but the pages of the *Historical Journal* more generally throughout the 1960s and 1970s.[58] However, this period also marked a fundamental shift in the nature of intellectual history, one that had a profound effect on the history of science. In 1969, Quentin Skinner published his seminal article, 'Meaning and understanding in the history of ideas', in the journal *History and Theory*. This exemplified a new contextualist approach to intellectual history. Skinner argued that the history of ideas needed to abandon its focus on 'perennial problems' and 'universal truths'. Instead, in order to understand the meaning of an historical text, one needed to 'trace the relations between the given utterance' and the 'wider *linguistic* context'.[59] In making this argument, Skinner drew heavily on John Austin's *How to do things with words* (1962). At its core, Skinner's methodology advocated recovering something very specific: not simply what was said, but what an author intended to *do* by saying it. This was the distinction between what Austin called 'locution' (saying) and 'illocution' (meaning). In practice, this involved interpreting particular speech acts within the intellectual context of the time, rather than assuming that modern notions of the 'state', 'virtue', or even 'politics' could be applied to past texts.[60]

Today, Skinner is rightly remembered as having played a pivotal role in the development of intellectual history in Britain. Along with John Dunn and John Pocock, Skinner helped establish the Cambridge School of political thought. Grounded in a contextual study of the language of politics, the Cambridge School continues to shape the writing of intellectual history today.[61] However, what is less often acknowledged is the relationship between Skinner's writings and the history of science. Skinner himself was profoundly influenced by new approaches to the history of science, particularly the work of Thomas Kuhn in the 1960s.[62] At the same time, Skinner's writings provided a resource for historians of science looking to understand what constitutes

[57] Vidar Enebakk, 'Lilley revisited: or science and society in the twentieth century', *British Journal for the History of Science*, 42 (2009), pp. 563–93; and Shapin, 'Discipline and bounding', p. 339.

[58] The *Cambridge Historical Journal* became the *Historical Journal* in 1958. Despite the change of name it continued to 'enjoy the support of the Cambridge Historical Society' and was 'edited in Cambridge', *Historical Journal*, 1 (1958), p. 6.

[59] Quentin Skinner, 'Meaning and understanding in the history of ideas', *History and Theory*, 8 (1969), pp. 3–53.

[60] James Tully, 'The pen is a mighty sword: Quentin Skinner's analysis of politics', in James Tully, ed., *Meaning and context: Quentin Skinner and his critics* (Cambridge, 1988); and Maria Pallares-Burke, *The new history: confessions and conversations* (Cambridge, 2002), pp. 213–19.

[61] Gary Browning, *A history of modern political thought: the question of interpretation* (Oxford, 2016), pp. 67–88.

[62] 'I remember being overwhelmed by Kuhn's *The Structure of Scientific Revolutions* when I read it in the mid-1960s', Pallares-Burke, *The new history*, p. 231. Kuhn is also cited in Skinner, 'Meaning and understanding', p. 7.

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anachronism and how to place science in context.[63] Skinner is therefore indicative of the reciprocal relationship between methodological innovation in the history of science and the wider historical profession. It is no coincidence, therefore, that one of Skinner's earliest articles in the *Historical Journal* was on the topic of early modern science. Titled 'Thomas Hobbes and the nature of the early Royal Society', the article was published just a few months after 'Meaning and understanding'. It is the first time Skinner explicitly puts his methodology into practice.

The article opens with a deceptively simple question: 'Why was Thomas Hobbes never made a Fellow of the Royal Society?' Skinner then reviews typical answers to this question. The first possibility, advanced by historians such as Maurice Goldsmith in *Hobbes's science of politics* (1966), is that Hobbes was excluded because his ideas were in conflict with those promoted by the Royal Society. Natural philosophers like Robert Boyle adopted a Baconian programme of experimentation, taking seriously the motto of the Royal Society: *nullius in verba* (on no one's word). In contrast, Hobbes promoted a philosophy of nature which was much more Cartesian. He believed that nature could be conceived of as a machine. Political philosophers could then make deductive inferences concerning the relationship between the nature of the machine and the structure of society, exactly as Hobbes did in *Leviathan* (1651). This clash – between deductive and inductive approaches to nature – came to a head when Boyle claimed to have created a vacuum using an air-pump at the Royal Society in 1660. Hobbes rejected Boyle's claims, and even went as far as to argue that experimental approaches to nature were dangerous, promoting political instability. Ultimately, for those working within the tradition of the history of ideas, it was Hobbes's philosophy of nature that explained why he was never made a Fellow of the Royal Society. Hobbes's exclusion was, as Goldsmith argued in *Hobbes's science of politics*, 'an accurate indication of his separation from the scientific opinion of the day'. Or, as Skinner put it more pithily, Hobbes was not made a Fellow 'because he was not a proper scientist'.[64]

Skinner then goes on to reject this explanation as a textbook example of anachronism. It mistakes the values of the Royal Society in the seventeenth century with those of the Royal Society in the present:

> To think of the original Royal Society as being in essence a professional body, operating strictly professional standards for entry (as the Royal Society clearly does now) is to apply to the original Society a paradigm for the understanding of its nature which is not merely derived from specifically twentieth-century experience, but

[63] Nick Jardine, 'Uses and abuses of anachronism in the history of the sciences', *History of Science*, 38 (2000), pp. 251–70; Nick Jardine, 'Etics and emics (not to mention anemics and emetics) in the history of the sciences', *History of Science*, 42 (2004), pp. 261–78; and Nick Tosh, 'Anachronism and retrospective explanation: in defence of a present-centred history of science', *Studies in the History and Philosophy of Science*, 34 (2004), pp. 647–59.

[64] Quentin Skinner, 'Thomas Hobbes and the nature of the early Royal Society', *Historical Journal*, 12 (1969), pp. 217–39, at pp. 217–34.

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which is demonstrably inapplicable as a description of the Society's seventeenth-century nature.[65]

In making this argument, Skinner directly cites 'Meaning and understanding'. It was 'clearly anachronistic' to assume that Boyle excluded Hobbes for failing to stand up to twentieth-century scientific standards. Intriguingly, Skinner also hints at the influence that new work in the history of science had on his thinking at the time. His repeated use of the word 'paradigm' is a reference to Thomas Kuhn's incredibly influential *The structure of scientific revolutions* (1962). The core of Kuhn's argument was that scientific change could best be understood as the transition between incommensurable worldviews, or 'paradigms' as he called them. Paradigms are incommensurable in the sense that the language used to describe the world in one is incompatible with the language used to describe the world in the other. (For example, the difference between descriptions of time in classical vs. relativisitic mechanics.)[66] In this sense, there is a clear link between Skinner's intellectual history and Kuhn's history of science: both rely on identifying the appropriate linguistic context in which ideas are produced. Both were also drawing on the philosophy of language, particularly the later Wittgenstein. In subsequent interviews, Skinner acknowledged that his account of Hobbes at the Royal Society was heavily influenced by reading Kuhn, 'especially his insistence that we should never read back into earlier societies the paradigms we have for understanding our own societies and its institutions and practices'. Indeed, when spending time at the Institute for Advanced Study at Princeton in the 1970s, Skinner even had an office next to Kuhn. Alongside Kuhn, Skinner remembered how his colleagues at Princeton, particularly the anthropologist Clifford Geertz and the philosopher of science Richard Rorty, helped convince him that there was no 'trans-cultural notion of rationality'. Instead, Skinner realized that 'the question of what it is rational for someone to believe primarily depends on what else they believed, and not at all straightforwardly on something called the evidence or the facts'.[67]

[65] Ibid., p. 233.

[66] Thomas Kuhn, *The structure of scientific revolutions* (Chicago, IL, 1962). On the uses of Kuhn's work in the history of science and beyond, see Robert Richards and Lorraine Daston, eds., *Kuhn's* Structure of Scientific Revolutions *at fifty: reflections on a science classic* (Chicago, IL, 2016). What Kuhn in fact meant by 'paradigm' has been debated at length. Famously, one scholar identified at least twenty-one different uses of the term in Kuhn's book; see Margaret Masterman, 'The nature of a paradigm', in Imre Lakatos and Alan Musgrave, eds., *Criticism and the growth of knowledge* (Cambridge, 1970). Unsurprisingly, Skinner's use of the term is similarly varied. He most often uses 'paradigm' to refer to the choice of analytic terms employed by contemporary historians ('apply to the original Society a paradigm for the understanding of its nature'). In contrast, other historians of political thought, such as John Pocock, used 'paradigm' to refer to the early modern intellectual context itself; see Richard Whatmore, 'Introduction', in John Pocock, *The Machiavellian moment: Florentine political thought and the Atlantic republican tradition* (Princeton, NJ, 2016), p. ix.

[67] Pallares-Burke, *The new history*, pp. 231–8.

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Back at Cambridge, Skinner also moved between worlds occupied by historians of science and historians of political thought. His article on Hobbes at the Royal Society had originally been written and presented as part of a monthly seminar series on the history of science run by Robert Young at King's College, Cambridge.[68] This series was meant as an antidote to the more traditional history of ideas promoted by Rupert Hall.[69] Young, part of a new wave of Marxists, wanted to put the politics back into science. He found Skinner's and Dunn's intellectual history appealing because 'both stressed that ideas do not beget ideas but that people do so in particular historical contexts and that the meaning of those ideas is exquisitely bound to the particularity of those contexts'.[70] The keyword here is 'context', a category of analysis that came to dominate the history of science in the 1970s and 1980s.[71] Young's own work rested on placing political and scientific thought in a 'common context', as he argued in the case of Charles Darwin and Thomas Malthus in *Darwin's metaphor* (1985).[72] Perhaps most famously, Steven Shapin and Simon Schaffer, writing in *Leviathan and the air-pump* (1985), argued that the debate between Hobbes and Boyle needed to be understood within the 'political context' of Restoration England. At a time of great social and religious instability, 'Boyle's experimentalism and Hobbes's demonstrative way were both offered as solutions to the problem of order.'[73] Similarly, Lawrence Goldman's 1983 article in the *Historical Journal* opened by citing Skinner's 'Meaning and understanding' before suggesting how such a method could be extended to the history of the social sciences. Goldman argued that, like earlier intellectual historians and scientists, sociologists were guilty of writing 'a history that current practitioners can understand in terms of their own research interests'. Instead, the history of the social sciences needed to

[68] On the history of this seminar series at King's, see Robert Young, *Darwin's metaphor: nature's place in Victorian culture* (Cambridge, 1985), pp. 177–9; and Shapin, 'Discipline and bounding', p. 343.

[69] Young later described Rupert Hall's work as 'bourgeois historiography of science'; Young, *Darwin's metaphor*, p. 219.

[70] Ibid., p. 176. Young and Skinner did, however, differ on what exactly constituted the context. Skinner, a 'non-Marxist', saw his approach as a counter to the determinism of Marxist intellectual history. He chose to emphasize the 'linguistic context' over 'religious, political and economic factors'. In contrast, Young wanted an approach which 'routinely considered social and political factors in scientific research'. In the end, Young found Dunn, Skinner, and the rest of the Cambridge School to be 'politically aloof'. Young, *Darwin's metaphor*, pp. 23 and 273; Skinner, 'Meaning and understanding', p. 3; and Pallares-Burke, *The new history*, p. 220.

[71] The journal, *Science in Context*, was founded specifically to accommodate this perspective in 1987. On the limits of 'context' as a category of analysis, see James Secord, 'Knowledge in transit', *Isis*, 95 (2004), pp. 654–72; and Peter Galison, 'Ten problems in the history and philosophy of science', *Isis*, 99 (2008), pp. 111–24, at pp. 112–13.

[72] Young, *Darwin's metaphor*, pp. 23–55.

[73] Steven Shapin and Simon Schaffer, *Leviathan and the air-pump* (Princeton, NJ, 1985), pp. 21 and 99.

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recover the 'intentions' of past historical actors 'by closely examining the language used to characterize this new discipline'. Goldman, like Skinner and many subsequent historians of science, therefore paid close attention to the use of particular terms as well as their origins. (We learn that the word 'sociology' was coined in 1839 by Auguste Comte, whilst the first English use of 'social science' appeared in 1836.) By placing science in its appropriate 'context', Goldman ultimately concluded that 'there was no unitary social discipline in nineteenth-century Britain but a series of separate intellectual ground-plans for a science of society'.[74]

The meaning of 'context' certainly changed considerably between the 1960s and 1980s. Whilst Skinner and his followers foregrounded intellectual and linguistic contexts, later historians of science such as Shapin and Schaffer placed greater emphasis on social and political contexts.[75] But nonetheless, we can see how from the 1960s onwards interactions between intellectual history and the history of science produced new ways of approaching both subjects. Skinner, and more broadly the Cambridge School of political thought, provided a major reassessment of the place of Hobbes in early modern science. At the same time, Skinner himself was profoundly influenced by new developments in the history and philosophy of science, particularly those originating in the United States with Kuhn and Rorty, but also in Cambridge with Young. The success of Skinner's history of political thought was, by his own admission, therefore at least partially dependent on insights developed to tackle the problems of interpreting early modern science.[76]

IV

By the late 1970s, the history of science was well established as an independent discipline in Britain and the United States. Dedicated departments had been founded at Harvard, Pennsylvania, Cambridge, Leeds, University College London, and Edinburgh.[77] These new departments were increasingly staffed

[74] Lawrence Goldman, 'The origins of British "social science": political economy, natural science and statistics, 1830–1835', *Historical Journal*, 26 (1983), pp. 587–616.

[75] Additionally, whilst Skinner concluded that the Royal Society's status as a 'gentleman's club' was at odds with its scientific role, Shapin and Schaffer showed how these functions reinforced one another. Restoration science was a product of gentlemanly codes of conduct, Steven Shapin, 'The house of experiment in seventeenth-century England', *Isis*, 79 (1988), pp. 373–404, at p. 390; and Shapin and Schaffer, *Leviathan*, p. 14.

[76] Pallares-Burke, *The new history*, pp. 231–8.

[77] Alfred Rupert Hall, 'Beginnings in Cambridge'; William Smeaton, 'History of science at University College London: 1919–1947', *British Journal for the History of Science*, 30 (1997), pp. 25–8; Eugene Garfield, 'The life and career of George Sarton: the father of the history of science', *Journal of the History of the Behavioral Sciences*, 21 (1985), pp. 101–17; Mayer, 'Setting up a discipline'; Anna-K. Mayer, '"I have been very fortunate…" Brief report on the BSHS oral history project: the history of science in Britain, 1945–1965', *British Journal for the History of Science*, 32 (1999), pp. 223–35; Steven Shapin and Simon Schaffer, 'Up for air', in Steven Shapin and Simon Schaffer, *Leviathan and the air-pump* (Princeton, NJ, reissue 2011), pp. xx–xxvi.

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by professional historians and sociologists rather than by scientists. This brought a measure of freedom. Arguments and methods no longer needed to match the expectations of scientists, and indeed often ran counter to the typical narratives of progress and modernization.[78] We have already seen how approaches in the history of political thought, not to mention anthropology and the philosophy of language, provided a resource for historians of science looking to rethink the relationship between text and context. Similar methodological innovations also followed from social history. The 1960s and 1970s saw the rise of a new generation of social historians, particularly in Britain. Typically associated with the Left, historians including Christopher Hill, E. P. Thompson, and Eric Hobsbawm provided a major reassessment of the relationship between class, capital, and the formation of the modern world. But it was not just the Marxists. More traditional social historians, including J. H. Plumb at Cambridge and Asa Briggs at Sussex, continued to promote economic and institutional analyses of society throughout the post-war period.[79] Once again, the history of science played a significant role in this historiography. Hill, in a debate which extended across several issues of *Past and Present*, argued that both the Protestant Reformation and the Scientific Revolution needed to be understood as responses to the shift from agricultural to industrial labour.[80] Similarly, Hobsbawm's work on the industrial revolution and Thompson's work on romanticism provided a Marxist reassessment of the history of technology and nature respectively.[81] Briggs too, particularly in his history of the British Broadcasting Corporation (BBC), paid close attention to the role of technology and media in shifting social relations during the twentieth century.[82]

[78] Shapin and Schaffer, 'Up for air', p. xxi.

[79] Miles Taylor, 'The beginnings of modern British social history?', *History Workshop Journal*, 43 (1997), pp. 151–76.

[80] Christopher Hill, 'Puritanism, capitalism and the scientific revolution', *Past and Present*, 29 (1964), pp. 88–97; Hugh Kearney, 'Puritanism, capitalism and the scientific revolution', *Past and Present*, 28 (1964), pp. 81–101; Christopher Hill, *Intellectual origins of the English revolution* (Oxford, 1965); Barbara Shapiro, 'Latitudinarianism and science in seventeenth-century England', *Past and Present*, 40 (1968), pp. 16–41. This debate also engaged authors in the *Historical Journal*; see John Morgan, 'Puritanism and science: a reinterpretation', *Historical Journal*, 22 (1979), pp. 535–60. The link between Puritanism and the scientific revolution had been suggested by Robert Merton, drawing on Max Weber, in the 1930s; see Steven Shapin, 'Understanding the Merton thesis', *Isis*, 79 (1988), pp. 594–605; and Robert Merton, 'Science, technology and society in seventeenth-century England, *Osiris*, 4 (1938), pp. 360–632.

[81] Eric Hobsbawm, *Industry and empire: from 1750 to the present day* (London, 1969); and Edward Palmer Thompson, *Witness against the beast: William Blake and the moral law* (Cambridge, 1994).

[82] Asa Briggs, *The history of broadcasting in the United Kingdom* (5 vols., Oxford, 1961–95); and Jean Seaton, 'Asa and the epochs: the BBC, the historian, the institution and the archive', in Miles Taylor, ed., *The age of Asa: Lord Briggs, public life and history in Britain since 1945* (Basingstoke, 2015).

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This combination of social history and the history of science also found its way into the *Historical Journal*. In 1971, Roy MacLeod published 'The Royal Society and the government grant', a classic example of social and economic history applied to the history of science.[83] The article, a close analysis of the development of government funding for science in the nineteenth century, was written whilst MacLeod worked at the Science Policy Research Unit (SPRU), established in 1966 at the University of Sussex. Briggs, then writing his history of the BBC, had encouraged MacLeod to come to Sussex following a Ph.D. at the University of Cambridge. Briggs had also played an important role, along with the economist Christopher Freeman, in setting up the SPRU, where MacLeod set to work.[84] Initially, the SPRU was closely allied to the politics of Harold Wilson's Labour government. Wilson famously argued that British socialism needed to be grounded in the 'white heat' of a new 'scientific revolution'. For Labour in the 1960s and 1970s, scientific and technical progress had the potential to bring about sweeping changes in the structure of society, reducing economic inequality and increasing social mobility. This programme was closely tied to the expansion of higher education in post-war Britain, with the foundation of a number of new universities including Sussex. Briggs, a life-long Labour party supporter, saw the history of science and technology as a guide to this kind of politics.[85] The SPRU promoted a toned-down version of the history of science promoted by Marxists like Bernal in the 1940s. This was a history of science with a practical aim in mind. MacLeod and Briggs were not interested in the meaning of early modern intellectual culture. What they wanted was a history of science which could inform policy decisions in the present.

This is all reflected in MacLeod's article. He focuses exclusively on the details of institutional and economic development. The article proceeds almost year-by-year, from the foundation of the government grant to the Royal Society under Lord John Russell's government in the 1840s through to the consolidation of public funding for science in the run-up to the First World War. The article is full of economic data and tables, setting out trends in public science funding and institutional hierarchies. MacLeod makes no apology for concentrating on what he calls 'external factors', without any attention to the

[83] Roy MacLeod, 'The Royal Society and the government grant: notes on the administration of scientific research, 1849–1914', *Historical Journal*, 14 (1971), pp. 323–58.

[84] David Edgerton, *Warfare state: Britain, 1920–1970* (Cambridge, 2006), pp. 299–301; Miles Taylor, 'Introduction: Asa Briggs and public life in Britain since 1945', in Taylor, ed., *The age of Asa*; Matthew Craghoe, 'Asa Briggs and the University of Sussex, 1961–1976', in Taylor, ed., *The age of Asa*; and Ângela Campos, *SPRU history project: a report on its qualitative angle*, August 2016 www.sussex.ac.uk/webteam/gateway/file.php?name=spru-history-report-qualitative-campos-2016.pdf.

[85] David Edgerton, 'The "white heat" revisited: the British government and technology in the 1960s', *Twentieth Century British History*, 7 (1996), pp. 53–82; Norman Vig, *Science and technology in British politics* (Oxford, 1968), pp. 34–54 and 81–103; and Harold Wilson, 'Labour and the scientific revolution', in *Report of the sixty-second annual conference of the Labour Party* (London, 1963), pp. 139–40.

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intellectual content of science. While he admitted that 'externalist history, or the history of scientific institutions…cannot attempt to account for the highly individual, subjective, creative flashes of scientific insight', he nonetheless argued 'it is still important to enquire into the complex social conditions which affect the rate and direction in which discovery may occur'.[86] This distinction, between 'internal' and 'external' factors, was typical of the historiography of science in the 1960s and 1970s. Historians argued over whether social or intellectual pressures should take precedence. One of the great achievements of the later sociology of science was to break down this divide. But in the context of Cold War science policy, such a distinction made sense.[87] It allowed historians like MacLeod to cut away the content of science and offer policy recommendations for impatient civil servants.

MacLeod's goal was to 'assess the relative influence of institutions on scientific progress' and 'to analyse the elusive consequences of decisions taken'. In adopting this approach, MacLeod offered 'a means of examining growth patterns in science over time'. Broadly, MacLeod argued that the Royal Society had initially been resistant to accepting public funds. Traditionally, the authority of a man of science rested on his gentlemanly status. The professionalization of science in the nineteenth century challenged this model of scientific prestige.[88] For Victorian gentlemen, it also raised questions of control and corruption. Government funding might bring 'personal jobbery and bureaucratic formalism' to scientific institutions, thus undermining established norms of conduct and patronage. Nonetheless, as the Victorian era wore on, both the state and the scientists came to accept closer institutional and financial links. MacLeod argued that this was a result of two major pressures. First, competition with other European states brought scientists and politicians together. There was a worry throughout the nineteenth century that French and German science, with state backing, was overtaking that in Britain. Whether true or not, this provided a powerful piece of rhetoric for those keen to increase state funding for science. Second, criticism of the elite nature of the Royal Society opened up science, and its funding, to a wider range of practitioners. Men like Thomas Henry Huxley argued that science should not simply be the preserve of the London elite, where most of the initial state funding was concentrated, but should be distributed regionally and across different social groups.[89]

MacLeod then used this broad historical analysis to draw more pointed policy conclusions. First, he argued that state funding of science had been partly

[86] MacLeod, 'The Royal Society and the government grant', pp. 323–4.

[87] Shapin, 'Discipline and bounding'; Shapin, 'Understanding the Merton thesis'; and Shapin and Schaffer, 'Up for air', pp. xiv–xv.

[88] The history of professionalization was a major topic in social history in this period, including in the history of science. This extended to the *Historical Journal*; see Roy Porter, 'Gentlemen and geology: the emergence of a scientific career, 1660–1920', *Historical Journal*, 21 (1978), pp. 809–36.

[89] MacLeod, 'The Royal Society and the government grant', pp. 323–58.

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responsible for the 'growth of different disciplines in science'. Second, MacLeod argued that public funding of science was also responsible for the growing divide between 'pure science and technology'. Third, MacLeod raised 'the problem of regionalism', whereby state funding tended to be concentrated in London, and failed to permeate across northern Britain, as well as Ireland and Scotland. And finally, MacLeod questioned whether public funding of science functioned as a form of state support or 'more as a reward system'. Referencing the American sociologist Robert Merton, MacLeod even suggested that the history of the Royal Society might reveal 'a Victorian version of the "Matthew Rule"', supporting established scientists rather than 'encouraging unorthodox, "revolutionary", ideas'.[90] These conclusions reflected the politics of the SPRU. How to fund science in the context of emerging disciplines, often with significant differences in economic cost, proved an enduring policy question, one just as applicable to the 1970s as to the 1870s. Similarly, how to ensure that the benefits of science and technology were spread evenly across the United Kingdom, particularly in the north and Scotland, was a constant preoccupation of the Wilson government. The decline in traditional British industries – textiles, coal mining, steel manufacture – was not good for Labour, something the party learned to its cost following the Winter of Discontent and the 1979 General Election. And finally, we see the re-emergence of tensions between state planning and scientific freedom. Wilson's policy advisers, including those at the SPRU, recognized the need to balance state aid with intellectual creativity. Examining the history of the government grant in the nineteenth century provided an opportunity to reflect on the workings of state funding and scientific freedom in the present.[91]

V

The 1990s marked the 'end of social history'. Or at least some people thought so.[92] In place of society and economics, historians increasingly turned towards culture as a guide to understanding the past.[93] The cultural turn had its origins in a variety of intellectual movements, many stretching back to the 1960s and 1970s. In France, the growth of postmodernist and poststructuralist philosophy, exemplified by the work of Michel Foucault and Jacques Derrida, forced historians to rethink many of the categories underpinning classic social analysis. 'Society', 'class', even the 'individual', were all revealed as products of

[90] Ibid., pp. 354–8.

[91] Edgerton, 'The "white heat" revisited'; Vig, *Science and technology in British politics*, pp. 34–54 and 81–103; and Wilson, 'Labour and the scientific revolution', pp. 139–40.

[92] For an overview of this debate, see Patrick Joyce, 'The end of social history?', *Social History*, 20 (1995), pp. 73–91.

[93] Geoff Eley, 'Is all the world a text: from social history to the history of society two decades later', in Terrence McDonald, ed., *The historic turn in the human sciences* (Ann Arbor, MI, 1996).

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Enlightenment discourse, cultural constructs rather than structural elements of human life. In Britain, a similar rejection of structural analysis helped usher in the 'new' social history, with its focus on language, popular culture, and everyday experience. At the same time, in the United States, many cultural historians drew on the work of anthropologists, particularly Clifford Geertz and his 'interpretative theory of culture'. Drawing on all these traditions, cultural history really took off in Britain and the United States in the 1980s and 1990s. With the growth of neoliberalism and the collapse of the Soviet Union, cultural analysis found its place in a world obsessed with individualism.[94] Like anthropologists, historians now looked to examine how identities, such as gender and race, were constructed through cultural representations. There was a certain literary bent to this early work, with a focus on the cultural meaning of texts. However, cultural history soon expanded beyond literary texts: visual culture, material culture, practices, and performance all came to be understood, again, in the mode of the anthropologist, as central to the construction of meaning.[95]

The key term here was 'construction'. Whereas previous generations of historians of science had been content to work with 'internal' and 'external' categories, this division looked increasingly suspicious to cultural historians. In what sense could science be located within a pre-existing social context, when cultural historians had shown that society itself was a construct? Instead, historians of science from the 1980s onwards started producing new studies showing how everything from protons to hormones needed to be understood as culturally constructed. At this time, historians of science were particularly well placed to make this move, as many were already dealing with similar theoretical problems through the sociology of scientific knowledge (SSK). Grounded in sociology, but also drawing on anthropology and postmodernism, SSK advanced a radical rethinking of notions of truth and objectivity, as well as a breakdown of the division between the scientific content and context. Truth and knowledge were social or cultural constructs, rather than fixed elements of reality. Pioneers of SSK included David Bloor in Edinburgh, Harry Collins in Bath, and, through his work on actor-network theory, Bruno Latour in Paris.[96] Much of the most original

[94] Daniel Rodgers, *Age of fracture* (Harvard, MA, 2003), pp. 1–14 and 77–110.

[95] Lynn Hunt, ed., *The new cultural history* (Berkeley, CA, 1989); Peter Burke, *What is cultural history?* (Cambridge, 2004), pp. 1–5, 30–46, 74–95; and Geoff Eley, *A crooked line: from cultural history to the history of society* (Ann Arbor, MI, 2005), pp. 115–82. For a discussion of the future of cultural history, see Peter Mandler, 'The problem with cultural history', *Cultural and Social History*, 1 (2004), pp. 94–117.

[96] For an introduction to this historiography, see Jan Golinski, *Making natural knowledge: constructivism and the history of science* (Cambridge, 1998). Key texts include Harry Collins, *The golem: what everyone should know about science* (Cambridge, 1983); David Bloor, *Knowledge and social imagery* (Chicago, IL, 1991); Barry Barnes, David Bloor, and John Henry, eds., *Scientific knowledge: a sociological analysis* (Chicago, IL, 1996); Bruno Latour, *Science in action: how to follow scientists and engineers through society* (Harvard, MA, 1987). For a critical assessment, see Ian Hacking, *The social construction of what?* (Harvard, MA, 1999).

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work in the history of science in the 1980s and 1990s followed, more or less explicitly, the methodology advanced by proponents of SSK and cultural history.[97] With science understood as part of wider culture, historians of science found it easier to work with and publish alongside general historians. This is reflected in a flurry of articles on the cultural history of science published in the *Historical Journal* in the 1990s and 2000s.[98]

These articles also reveal the range of approaches to cultural history that historians of science engaged with, ranging from book history to imperial history. Aileen Fyfe's article, 'Reading children's books in late eighteenth-century dissenting families', is a classic example of a technique of cultural history applied to the history of science.[99] The history of the book, and more specifically the history of reading, had been pioneered as a method of cultural history by scholars of eighteenth-century France, particularly Robert Darnton and Roger Chartier. Darnton and Chartier had showed how events like the French Revolution had their origins in culture rather than simply economic or social change. What is more, both Darnton and Chartier emphasized the history of the book as a way to recover cultural meaning.[100] Darnton also engages seriously with the relationship between knowledge and print, particularly in his study of Diderot's *Encyclopédie* (1751–72).[101] Fyfe follows this methodology, citing Darnton's influential collection of essays *The great cat massacre* (1984). However, she applies it to a very different context. Fyfe reconstructs the cultural meaning of children's books on natural history in eighteenth-century Britain. This, by her own admission, is a difficult task. Children rarely leave a record of their reading experiences. So instead, Fyfe follows cultural historians like Darnton in reconstructing attitudes towards reading and associated practices. She skilfully shows how different religious groups promoted a variety of approaches to reading children's books, reflecting different attitudes to the relationship between God, morality, and the natural world in eighteenth-century Britain. Drawing on techniques pioneered by literary scholars and the

[97] Confusingly, historians of science often talk of the 'social construction' of science: at other times, 'cultural construction'. This reflects the influence of both SSK and cultural history.

[98] Aileen Fyfe, 'Reading children's books in late eighteenth-century dissenting families', *Historical Journal*, 43 (2000), pp. 453–73; Sujit Sivasundaram, 'Trading knowledge: the East India Company's elephants in India and Britain', *Historical Journal*, 48 (2005), pp. 27–63; David Gange, 'Religion and science in late nineteenth-century British Egyptology', *Historical Journal*, 49 (2006), pp. 1083–103; Sadiah Qureshi, 'Robert Gordon Latham, displayed peoples, and the natural history of race, 1854–1860', *Historical Journal*, 54 (2011), pp. 143–66; Harriet Lyon, 'The Fisherton monster: science, providence, and politics in early Restoration England', *Historical Journal*, 60 (2017), pp. 303–32.

[99] Fyfe, 'Reading children's books'.

[100] Robert Darnton, *The forbidden best-sellers of pre-revolutionary France* (New York, NY, 1996); Robert Darnton, *The great cat massacre and other episodes in French cultural history* (New York, NY, 1984); Roger Chartier, *The cultural origins of the French Revolution* (Durham, NC, 1991).

[101] Robert Darnton, 'What is the history of books?', *Daedalus*, 111 (1982); and Darnton, *The great cat massacre*, pp. 191–214.

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new cultural history, Fyfe establishes this argument through the close reading of an individual text, John and Anna Aikin's *Evenings at home* (1792–6).[102]

It was not just children's books or 'forbidden bestsellers' which caught the attention of cultural historians. The history of the book opened up the study of popular culture more generally. Throughout the 1990s and 2000s, historians expanded their understanding of 'texts' to include theatre, music, and art. Nowhere was this more powerful than in the history of race. Historians of science had always been well placed to engage with the history of racial thought. Typically, they did so from the perspective of intellectual history, charting major theoretical changes in how concepts of race, ethnicity and human difference were understood. Nancy Stepan's *The idea of race in science* (1982) epitomizes this approach. She identifies an intellectual shift from social understandings of race in the eighteenth century towards more biological understandings of race in the nineteenth.[103] However, the turn towards cultural history opened up a radically different way of approaching the history of race. Rather than studying elite intellectual change, historians of science argued that racial thought was embodied in popular culture. Exhibitions, museums, posters, and songs all worked to construct racial difference. Popular culture was not marginal, but actually played a major role in the development of scientific ideas.

These arguments are all developed in detail by Sadiah Qureshi in her article entitled 'Robert Gordon Latham, displayed peoples, and the natural history of race, 1854–1866'. The article focuses on the work of the ethnologist Robert Gordon Latham, author of *Natural history of the varieties of man* (1854) and curator of the court of natural history at the Crystal Palace in Sydenham in 1854. Qureshi makes the case that the Crystal Palace, and other exhibitions of displayed peoples, were sites for the making of scientific and racial knowledge. Adopting the techniques of the cultural historian, Qureshi recovers in great detail the layout of particular exhibits and analyses images in popular magazines. She draws on a contemporary guidebook to reveal how models of Zulus, San, and Javanese peoples were incorporated into the natural history display at the Crystal Palace. However, as Qureshi herself notes, 'visitors did not necessarily follow the advice of guidebooks'. Qureshi therefore balances her analysis of the exhibition space with an examination of other contemporary documents: satirical images from *Punch* and reviews of the natural history court in Victorian periodicals. Together, these sources of popular culture allow Qureshi to establish a much more complex history of racial thought, challenging the earlier work of Stepan. As Qureshi concludes, 'the

[102] Fyfe, 'Reading children's books'.

[103] Nancy Stepan, *The idea of race in science: Great Britain, 1800–1960* (London, 1982). See also Robert Young, *Colonial desire: hybridity in theory, culture and race* (London, 1995); Hannah Augstein, ed., *Race: the origins of an idea, 1760–1850* (Bristol, 1996); and George Stocking, *Victorian anthropology* (New York, NY, 1987).

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mid-nineteenth-century saw a substantial *proliferation*, not homogenization, of intellectual and methodological approaches accompanying the scholarly study of human variation'.[104] Here, we see exactly how cultural history augments the existing intellectual history. By drawing on cultural artefacts – guidebooks, images, diaries, and letters – Qureshi is able to show how meanings associated with human difference proliferated. This then challenges an older narrative which appears increasingly homogenizing and monolithic.[105]

Qureshi's attention to race points towards another major development in the historiography of science in the 1990s: the turn towards imperial and global history. Throughout the Cold War, historians in the West tended to assume that science was something done in Europe, and its history therefore centred on Britain, France, Italy, and Germany.[106] We have seen this already in the articles published in the *Historical Journal*. From Broad in the 1940s to MacLeod in the 1970s, the history of science was a history of white men in Europe. (Not coincidentally, it was also a history written by white men in Europe.) Following the fall of the Berlin Wall, cultural history opened up an intellectual space in which a variety of other geographies and identities could be incorporated: women, people of colour, and colonized subjects all found a place in the history of science in the 1990s and 2000s. This is reflected in a series of articles published in the *Historical Journal* which connected science to the cultural history of empire.

In 2005, Sujit Sivasundaram published 'Trading knowledge: the East India Company's elephants in India and Britain'. The article argues that, in the context of nineteenth-century natural history, 'collaborations between colonizer and colonized' produced 'hybrid forms of natural knowledge'. British naturalists drew on earlier Mughal understandings of elephants as symbols of military power, whilst also examining ancient Hindu texts for clues as to classification. In making this argument, Sivasundaram explicitly draws on cultural history. He is interested in 'the representation of the non-European', and

[104] Qureshi, 'Robert Gordon Latham', pp. 143–66. Qureshi develops this argument at greater length in Sadiah Qureshi, *Peoples on parade: exhibitions, empire and anthropology in nineteenth-century Britain* (Chicago, IL, 2011).

[105] See also Mandler, 'The problem with cultural history', pp. 96–103, which addresses the relationship between new histories of race and cultural history. This argument is also developed in Sujit Sivasundaram, 'Race, empire and biology before Darwinism', in Denis Alexander and Ronald Numbers, eds., *Biology and ideology from Descartes to Dawkins* (Chicago, IL, 2010).

[106] Prior to the Cold War, historians of science tended to be internationalists and their histories reflected this. George Sarton's monumental *Introduction to the history of science* (3 vols., Baltimore, MA, 1927–48) addressed Egyptian, Islamic, Chinese, Japanese, and Indian science, as did the series of 'critical bibliographies' he published in *Isis* from 1913 onwards, George Sarton, 'Bibliographie analytique des publications relatives à l'histoire, de la science parues depuis le 1st janvier 1912', *Isis*, 1 (1913), pp. 136–88; Marwa Elshakry, 'When science became Western: historiographical reflections', *Isis*, 101 (2010), pp. 98–109; and Ronald Numbers, 'The American History of Science Society or the International History of Science Society? The fate of cosmopolitanism since George Sarton', *Isis*, 100 (2009), pp. 103–7.

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makes the case for the study of popular alongside elite culture. But Sivasundaram also pushes cultural history beyond its typical European focus. 'Cultural historians of Britain', Sivasundaram argues, should 'pay attention to the arrival of Indian traditions in the metropole. Knowledges from colonized lands were appropriated and reinvented, they did not disappear powerless in the face of conquest, and neither were they isolated to colonial territories.'[107]

Here, Sivasundaram is responding to an earlier tradition within the history of science and empire. In 1967, George Basalla published 'The spread of Western science'. An influential diffusionist account, Basalla's model was made up of three phases. In phase one, 'the non-scientific society or nation provides a source for European science'. In phase two, European science is done in the colonies, with the establishment of laboratories and universities. In phase three, post-colonial states 'struggle to achieve an independent scientific tradition'. In identifying these three phases, Basalla drew direct inspiration from Walt Rostow's *The stages of economic growth: a non-communist manifesto* (1960). Basalla's article was in effect a Cold War science policy document: its chief aim was to explain and recommend colonial and post-colonial science policies which could aid the United States in its fight against communism. Basalla notes that the Soviet Union had recently 'reached, and in some cases surpassed, the science of Western European nations'. Basalla concludes his article with an assessment of those countries which he believed were most likely to develop independent scientific traditions. He claims 'China and India, and perhaps some South American and African countries' show 'great potential for future scientific growth'. However, he then acknowledges the 'major obstacles to be overcome before they establish their independent scientific cultures'. The fear running throughout Basalla's article is the possibility of Soviet intervention in post-colonial science: here 'independent scientific cultures' meant independence from communism.[108]

This model remained incredibly influential in the historiography of science well into the 1980s. Earlier histories of science and empire tended to follow Basalla in treating the relationship between colony and metropole as relatively fixed and one-way.[109] But with the fall of the Soviet Union, Basalla's model looked increasingly outdated. The development of post-colonial and subaltern studies, alongside the new imperial history of Catherine Hall and Kathleen Wilson, provided an alternative framework for understanding science and

[107] Sivasundaram, 'Trading knowledge', p. 30.

[108] George Basalla, 'The spread of Western science', *Science*, 156 (1967), pp. 611–22. For the Cold War origins of Basalla's model, see Elshakry, 'When science became Western'; and Kapil Raj, 'Beyond postcolonialism and postpositivism: circulation and the global history of science', *Isis*, 104 (2013), pp. 337–47. For the relationship between the Cold War and historians' understanding of what constitutes science, see Joel Isaac, 'The human sciences in Cold War America', *Historical Journal*, 50 (2007), pp. 725–46.

[109] For example Nathan Reingold and Marc Rothenberg, eds., *Scientific colonialism* (Washington, DC, 1987).

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empire.[110] Instead of emphasizing the power of the metropole over the colony, post-colonial scholars and new imperial historians alike recovered the variety of ways in which colonized people exerted agency, both in the colony but importantly back in Britain too. This challenged the one-way model for the flow of scientific knowledge, and opened up a space in which a variety of knowledge traditions could contribute to the making of modern science, both in the colony and the metropole.[111] When Sivasundaram talks of 'hybrid forms of natural knowledge' and the role of 'Indian traditions in the metropole', this is exactly what he has in mind. For Sivasundaram, India already had an independent scientific tradition, well before the arrival of the British. Crucially, this argument relies on a move that became increasingly common following the cultural turn. Sivasundaram frames his argument in terms of 'knowledge' rather than 'science'.[112] This allows him to incorporate a much broader range of sources and historical actors. He analyses East India Company records, articles published by the Royal Society in London, Vedic texts, and Mughal histories. This attention to a range of alternative European and non-European sources, read against one another, came to characterize one mode of writing the global history of science.[113] It also allowed historians of science, like Sivasundaram, to make serious contributions to South Asian and imperial history more generally. Indeed, Sivasundaram's career reflects this. After coming to Cambridge to study engineering and the natural sciences, he completed a Ph.D. in the Department of History and Philosophy of Science under James A. Secord. However, following a research fellowship, Sivasundaram joined the Department of International History at the London School of Economics. He then returned to Cambridge, but this time to join the History Faculty as Lecturer and then Reader in World History. Today, he is co-editor of the *Historical Journal*. Sivasundaram's transition, from scientist to historian, exemplifies a trend in which historians of science increasingly found their home in history departments.[114] With science firmly established as part of wider culture, the history of science was finally considered a legitimate subject for general historical study.

[110] Catherine Hall, *Civilizing subjects: metropole and colony in the English imagination, 1830–1867* (Cambridge, 2002); Kathleen Wilson, ed., *A new imperial history: culture, identity and modernity in Britain and its empire* (Cambridge, 2004); Dipesh Chakrabarty, *Provincializing Europe: postcolonial thought and historical difference* (Princeton, NJ, 2000).

[111] For an overview of this historiography, see Raj, 'Beyond postcolonialism'. For examples of this new approach in action, see Gyan Prakash, *Another reason: science and the imagination of modern India* (Princeton, NJ, 1999); and Kapil Raj, *Relocating modern science: circulation and the construction of knowledge in South Asia and Europe, 1650–1900* (Basingstoke, 2004).

[112] On the implications of this move, see Lorraine Daston, 'The history of science and the history of knowledge', *Know*, 1 (2017) pp. 131–54.

[113] Sujit Sivasundaram, 'Sciences and the global: on methods, questions, and theory', *Isis*, 101 (2010), pp. 146–58.

[114] Aileen Fyfe and Sadiah Qureshi both followed similar career trajectories, as have I.

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VI

In the 1940s, the editor of the *Historical Journal*, Herbert Butterfield, argued the history of science was too important to be left to scientists. He was right.[115] However, Butterfield probably would not have predicted that, once the history of science professionalized, it would transform the wider discipline of history itself. Today, the co-editor of the *Historical Journal*, along with two other members of the editorial board, are trained historians of science.[116] This 'Retrospect' essay has examined the history of that transformation: from the history of science as a discipline practised primarily by scientists to one that has found its place within the wider historical profession today. Drawing on articles published in the *Historical Journal*, it has necessarily been an exercise focused on the historiography of science as it developed in the Anglophone world. Different traditions in the historiography of science developed in continental Europe, India, China, Africa, and the Americas during the same period.[117] More work still needs to be done to integrate these historiographies alongside the one I have recovered here.[118] More broadly, this essay argued that we need to situate the development of the history of science within the broader historiographical landscape. Historians of science have spent a lot of time examining the connection between their discipline and philosophy, anthropology, and sociology. But they rarely reflect on the relation between the history of science and the discipline of history itself. This essay has demonstrated how major changes in the practice of history, from the intellectual history of the 1960s through to the cultural history of the 1990s, transformed the ways in which the history of science was written. It also demonstrated that the intellectual traffic was two-way. Work in the history of science played a major role in the development of 1960s political thought, 1970s social history, and 1990s cultural history. The history of science therefore played an important role in the development of twentieth-century historiography more generally. In concluding, I want to offer some thoughts on the future of the history of science. What has been gained and what has been lost over the last seventy years?

To begin with, there is clearly a growing separation between the history of science and the philosophy of science. Whereas in the 1940s someone like C. D. Broad could write the history of science, and draw philosophical

[115] Steven Shapin, 'Why scientists shouldn't write history', *Wall Street Journal*, 13 Feb. 2015.

[116] Sujit Sivasundaram (co-editor), James A. Secord (editorial board), and Emma Spary (editorial board).

[117] See Dhruv Raina, *Images and contexts: the historiography of science and modernity in India* (Oxford, 2010); Iwo Amelung, 'Historiography of science and technology in China', in Jing Tsu and Benjamin Elman, eds., *Science and technology in modern China, 1880s–1940s* (Leiden, 2014); and David Serlin, 'Confronting African histories of technology: a conversation with Keith Breckenridge and Gabrielle Hecht', *Radical Science Review*, 127 (2017), pp. 87–102.

[118] This was the theme of a series of Arts and Humanities Research Council workshops, convened by Sujit Sivasundaram and Simon Schaffer, on 'Exploring traditions: sources for the global history of science', held between 2013 and 2014 in Cambridge and Delhi.

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lessons, today most philosophers concern themselves only with conceptual analysis of scientific terms. Their work is, almost by definition, ahistorical. This is not just a problem in the history of science. Philosophers and historians more generally used to work much more closely together. But historians of science and philosophers of science are still often housed in the same department, as they are in Cambridge. The question of the value of these two disciplines working together is therefore more pertinent.[119] For some, the solution is to practise an 'integrated history and philosophy of science', as advocated by Hasok Chang. In many ways, this operates rather like Broad's philosophy of science in the 1940s and Kuhn's in the 1960s. It uses historical episodes to draw philosophical conclusions concerning the nature of science, whether that be in terms of method or concepts. Chang in particular advocates using the history of science as a guide to discovering forgotten scientific theories that might be useful today. Here the history of science works for the scientist as well as the philosopher.[120] Whilst one can see the attraction of this approach, it is ultimately hard to square the circle of a commitment to historicism with a desire for trans-historical philosophical conclusions. Much of the work since the 1960s, and particularly following the cultural turn of the 1990s, has demonstrated that there are very few general conclusions to be drawn concerning the sciences. Once the meaning of science proliferates, as it did under the cultural turn, it is hard to sustain many philosophical conclusions, even those concerning specific scientific concepts such as fitness or quantum states. All that is left is a rather empty commitment to pluralism.

What then for the history and philosophy of science? My view is that historians of science and philosophers of science could find common ground in the field of political philosophy. Indeed, the history of political thought is one of the few sub-disciplines which still maintains an active connection to philosophy. Scholars following Skinner have demonstrated how you can balance a commitment to historicism with a desire to understand political concepts more generally. For example, they are able to chart the history of liberalism in order to understand better the ways in which liberalism functions in the present, even offering normative judgements about what could be done to improve a particular political philosophy.[121] One could imagine a similar move in the history of science. Whereas much of the current philosophy of science is concerned with

[119] On the history of this separation, see Jan Golinski, 'Thomas Kuhn and interdisciplinary conversation: why historians and philosophers of science stopped talking to one another', in Seymour Mauskopf and Tad Schmaltz, eds., *Integrating history and philosophy of science: problems and prospects* (Dordrecht, 2012); and Stephen Toulmin, 'From form to function: philosophy and history of science in the 1950s and now', *Daedalus*, 106 (1977), pp. 143–62.

[120] See Hasok Chang, *Inventing temperature: measurement and scientific progress* (Oxford, 2004); and Mauskopf and Schmaltz, eds., *Integrating history*.

[121] See particularly Duncan Bell, *Reordering the world: essays on liberalism and empire* (Princeton, NJ, 2016); and Quentin Skinner, *Liberty before liberalism* (Cambridge, 1998). My hope is that historians of science can draw inspiration and contribute further to the project of writing a global intellectual history, see Samuel Moyn and Andrew Sartori, eds., *Global intellectual history*

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analysing scientific concepts, there is little attention to what underlying political philosophies these represent. To take one example, the debate between proponents of biological and cultural evolution is seen as a conceptual problem.[122] But historians of science would argue that there are different political philosophies underlying these theories.[123] How might this work in practice? One could imagine a team of historians and philosophers charting the changing nature of evolutionary theory, and the underlying political philosophies accompanying these theories. They could then use this history to explore the politics of biological and cultural evolution today, even suggesting how the debate between different theorists might be resolved at a political rather than conceptual level. What we need then is not an integrated history and philosophy of science but a political philosophy of science.

This leads me to a broader point about the nature of the history of science today. As this collection of articles demonstrates, the history of science as a discipline developed in response to Cold War politics. From an emphasis on socialist planning to a concern with post-colonial science, the clash between communism and capitalism structured how historians approached the sciences. Within this context, much of the history of science worked as a form of critique against the scientific establishment. With the threat of nuclear war, the growth of the military-industrial complex, and the immense power invested in scientists, engineers, and doctors, it is easy to understand why. To an extent, this continued in the immediate aftermath of the fall of the Berlin Wall. If anything, the 1990s and early 2000s produced an even greater concentration in the power of science. Pharmaceuticals, mobile phones, and the internet all penetrated deeper and deeper into everyday life. Once again, historians of science, particularly feminist and post-colonial scholars, used history as a form of critique. Yet at the same time, many found the new cultural approach to society, filled with competing identity and truth claims, profoundly troubling. The 'science wars' and 'culture wars' of the 1990s highlight this best. They also reveal the ways in which neoliberalism permeated the historiography. For better or worse, everything was up for grabs in the marketplace: identity, culture, even truth itself.[124]

The world, however, has changed. But has the history of science? Today, the problems we face are less a consequence of scientific power and more a problem of wilful ignorance. We live in an era of 'fake news' and climate

(New York, NY, 2013). I make this case in James Poskett, 'Phrenology, correspondence and the global politics of reform, 1815–1848', *Historical Journal*, 60 (2017), pp. 409–42.

[122] Tim Lewens, *Cultural evolution: conceptual challenges* (Oxford, 2015).

[123] On the history of biological and cultural approaches to evolution, see Henrika Kuklick, 'The theory of evolution and cultural anthropology', in Aldo Fasolo, ed., *The theory of evolution and its impact* (Heidelberg, 2012).

[124] For an overview of these debates, see Keith Ashman and Phillip Barringer, eds., *After the science wars: science and the study of science* (London, 2000); James Hunter, *Culture wars: the struggle to define America* (New York, NY, 1991); and Rodgers, *Age of fracture*, pp. 1–14 and 77–110.

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change denial. We are told by politicians that people have 'had enough of experts'.[125] International technology companies refuse to pay tax, whilst university funding is cut. For those of us teaching the history of science, the critiques popular in the 1980s and 1990s do not seem to hold the same weight with students. What good is it telling an undergraduate, born in the twenty-first century, that we should be suspicious of scientific truth. They live in a world in which the president of the United States thinks the same thing, albeit for very different reasons. How then can we write the history of science in a post-communist and post-truth world? It would be a mistake, for sure, to abandon critique. Whilst some are tempted to return to the old model of history as a cheerleader for science, what we actually need is more politics, not less. My view is that the history of science needs to re-engage with political thought. The cultural history of the 1990s, whilst it produced a range of important contributions, many of which I rely on in my own work, was nonetheless of its time. It also had the effect of shifting the emphasis away from political philosophy. That is not to say that the cultural history of science was apolitical. Far from it. The cultural history of science was wrapped up in a range of political endeavours, particularly feminism and post-colonialism. However, in placing an emphasis on culture over society and economics, the cultural history of science ended up privileging individual identities over collective experience. Much of the history of science written today, including my own, is micro-history. How to reconcile the close study of individual experience with a wider appeal to the 'big picture' is one of the great challenges facing historians of science.[126]

I would argue that the best way to do this is to take inspiration from new work in the history of political thought and political philosophy. The time in which historians of science operated like political philosophers was relatively short lived.[127] In the 1940s, socialist historians of science like Bernal and Needham certainly saw the history of science as a way of doing politics. The same was true of Robert Young and the many students he inspired at Cambridge in the 1970s.[128] But since the 1980s, the history of science and the history of political thought have drifted apart. At Cambridge, this division is reinforced through separate departments, book series, and degree courses. Few historians of science see their work as part of political philosophy today. And few historians of political thought demonstrate serious engagement with the history of science. When they do write about the sciences, the focus tends to be on the economic and social sciences over disciplines such as mathematics, chemistry, and

[125] Michael Gove, interview with Faisal Islam, Sky News, broadcast 3 June 2016.

[126] James Secord, 'Introduction: the big picture', *British Journal for the History of Science*, 26 (1993), pp. 387–9.

[127] For a rare example of philosophy of science explicitly pitched as political philosophy, see Joseph Rouse, *Knowledge and power: towards a political philosophy of science* (Ithaca, NY, 1987).

[128] For example Roger Cooter, *The cultural meaning of popular science: phrenology and organization of consent in nineteenth-century Britain* (Cambridge, 1984); and Adrian Desmond, *The politics of evolution: morphology, medicine and reform in radical London* (Chicago, IL, 1989).

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physics.[129] There is also a tendency to approach the sciences from the perspective of the history of ideas, with little recognition of the ways in which historians of science since the 1980s have increasingly framed their work in terms of material culture or practice. My contention is that both groups – historians of science and historians of political thought – still have a lot to learn from one another. Historians of science could better engage with broader questions in political philosophy, whilst historians of political thought might begin to treat ideas as embodied in objects and practice rather than simply language.

If this proposition is so attractive, why is it not happening more regularly?[130] Part of the problem is institutional. But this divide is also a product of the historiography itself, something this essay has tried to correct. We tend to think of the history of science as a separate discipline from the start, whereas in fact there has been a series of exchanges with the wider historical profession stretching right back to the 1930s. At the same time, classic texts like *Leviathan and the air-pump* are remembered as works of social or cultural history, rather than part of the history of political thought.[131] But the success of *Leviathan and the air-pump* in particular relied on bridging the divide between how we write about politics and how we write about science. It was not simply a micro-historical episode in Restoration culture. Rather, the history of the dispute between Boyle and Hobbes revealed something about the political philosophy of Restoration England writ large.[132] The aim of the book, as Shapin and Schaffer set out in the original text, was to 'show the nature of the intersection between the history of natural philosophy and the history of political thought and action'.[133] Significantly, this was also an argument grounded in the study of scientific practice and instrumentation rather than ideas and language. A history of 'action' as much as 'thought'. Perhaps this is why it met with such a cool reception amongst traditional Hobbes scholars at the time.[134]

[129] See, for example, chapters on the 'social sciences' and 'nature' in Gareth Stedman Jones and Gregory Claeys, eds., *The Cambridge history of nnineteenth-century political thought* (Cambridge, 2011), as well as other volumes in the same series.

[130] A number of historians of political thought and historians of science are crossing this divide, often combining training in both disciplines or working collaboratively. Recent exemplars include Alison Bashford and Joyce Chaplin, *The New Worlds of Thomas Robert Malthus: rereading the* Principle of Population (Princeton, NJ, 2016); Fredrik Albritton Jonsson, 'Rival ecologies of global commerce: Adam Smith and the natural historians', *American Historical Review*, 115 (2010), pp. 1342–63; and Katrina Forrester and Sophie Smith, eds., *Nature, action and the future: political thought and the environment* (Cambridge, 2018).

[131] Articles collected together in *Isis* special issue 'A second look: *Leviathan and the air-pump*', including John Heilbron, 'Comment: a last judgement', *Isis*, 108 (2011), pp. 122–3; and Trevor Pinch, 'Comment: all pumped up about the sociology of scientific knowledge', *Isis*, 108 (2011), pp. 127–9.

[132] Shapin and Schaffer, 'Up for air', pp. xli–xliv.

[133] See also 'That is why the materials in this book are contributions to political history as well as to the history of science and philosophy', in Shapin and Schaffer, *Leviathan and the air-pump*, pp. 21 and 332.

[134] Shapin and Schaffer, 'Up for air', pp. xxxiv–xxxvii.

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What then for the history of science? Rather than celebrating or challenging scientific authority, the history of science should help scholars and students understand exactly what is at stake politically when people invoke scientific theories or make truth claims. When Donald Trump denies climate change, he is making a political claim, not just about the climate, but about the nature of society. (The same is true of course of the Intergovernmental Panel on Climate Change.) Furthermore, climate change is not just an idea. It is embodied in a whole range of material practices: from thermometers in the Arctic to international meetings of scientific experts.[135] For me, then, the history of science is a particularly powerful way of doing the history of political thought and action. It grounds ideas in the material world in a way few disciplines can match. What remains is to transform the history of science into a way of doing political philosophy as well.[136]

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[135] Naomi Oreskes and Erik Conway, *Merchants of doubt: how a handful of scientists obscured the truth on issues from tobacco smoke to global warming* (New York, NY, 2010).

[136] Recent work at the intersection of environmental history and political philosophy is exemplary in this respect, see Katrina Forrester and Sophie Smith, 'History, theory and the environment', in Forrester and Smith, eds., *Nature, action and the future.*

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