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RESEARCH ARTICLE

Conversation: the history of science and the 'big picture'

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Abstract

This conversation article brings together six of the original contributors to the 1993 *Getting the Big Picture* special issue of the *British Journal for the History of Science*. The contributors introduce their personal memories of the 1991 conference panel which formed the basis of that special issue. They also discuss the wider intellectual, institutional and political contexts of writing the history of science during the 1980s and 1990s, before concluding with reflections on the future of the discipline. The conversation was held live online via Microsoft Teams in March 2023. A professional transcript was produced by Sarah King. The transcript was then edited by James Poskett for length and clarity, before final edits were made by the contributors.

James Poskett: Let's start with you, Jim. Could you give us a sense of the context and motivation for organizing the original 1991 'Big Picture' meeting?

James Secord: The 'Big Picture' meeting was organized as a meeting of the British Society for the History of Science at the Science Museum, London in 1991. It came out of my experience having taught for six years at Imperial College, where I was teaching natural-science students. I realized that it was very difficult to find good introductory survey texts of any sort at that point. The most obvious ones were the *Cambridge History of Science*, as it was called then, which were a series of short books written by the previous generation of scholars. Many of them weren't bad books, but, to be honest, the narrative within them felt really rather tired compared to what was available within the specialist secondary literature. A lot of this reflected the huge changes in the history of science in the late 1970s and the 1980s, and particularly the rise of the cultural history of science, the increasing awareness of questions of gender, and a real turn toward material questions of practice. All of these things were pretty invisible when you turned to books by, say, Richard Westfall or Allen Debus.

Around the same time, I was also involved in organizing a meeting on the relationship between the history of science as a discipline, and its various publics. It seemed to me that this problem of how you tell the story of science to public audiences was really important in relation to the 'big picture'.

It was also the case that I was very aware of the contrast between my own research, which was often very micro – I was focused on a study which dealt with about eighteen months of history within Victorian Britain. On the one hand, I was doing that, and on the other hand, I was teaching the history of Babylonian mathematics and post-Maxwellian electrodynamics and continental drift, and all sorts of different subjects. I wasn't quite sure how to fit these together into any kind of meaningful picture.

James Poskett: Thanks, Jim. The relationship between teaching and research clearly structured a lot of those original 'big-picture' conversations. Perhaps the other contributors could give us a sense of how they came to these questions, both institutionally and intellectually?

Andrew Barry: I'd recently taken up a lectureship in sociology at Brunel University. But actually I came to sociology having read history and philosophy of science (HPS) at Cambridge, and written essays on the sociology of scientific knowledge with Nick Jardine and critical theory with Mike Power. The question of the relationship between HPS and social theory was there, even if it remained on the margins of HPS at the time. And then I moved out of history of science into something like science and technology policy and sociology. There was a fertile area of dialogue between the history of science and sociology. The Brunel department included Steve Woolgar, of course, who had co-written, with Bruno Latour, *Laboratory Life*, and Nikolas Rose, one of the leading commentators on Foucault.[1] The nexus of relations between Foucault and Latour was how I entered into sociology.

One further thing that strikes me about my experience as an undergraduate in the early 1980s is that, as I was taught it, the history of science ended in about 1850. There were signs that this was changing. Donna Haraway wrote about the cyborg in the context of the Cold War, and people like Jon Agar and David Edgerton were concerned with the entanglement of the history of science and the history of the twentieth-century British state. I was concerned with the European Union and its formation as an infrastructural space, just as historians of science were beginning to investigate the importance of infrastructure for empire. For me, it was also about placing the history of the sciences in the post-war period as absolutely integral to understanding late twentieth-century political history.

The other context, which I think is important to realize, is of course the aftermath of the early 1980s. This was a period of politicization and conflict, which bears comparison with today. Questions about the relationship between science and empire, pollution, the politics of nuclear power and so forth – these were live issues. For me, these questions disturbed the hermetic boundary of the history of science.

We were also writing in the context of post-structuralism, feminism, postcolonialism, postmodernism, and so on. This was a time when the 'big picture' was being interrogated and rethought in a whole range of disciplines from emerging scholarship. The history of science was engaging in that debate, which was also happening in parallel in sociology and cultural studies and across the humanities.

Andrew Cunningham: When I was an undergraduate student at Oxford, studying history, I did a special subject on the scientific revolution. It was absolutely marvellous, but it was run by, how can I put it, the worst teacher in the world. From there I went to Imperial College London, where the Halls were teaching: Alfred Rupert and Marie Boas. Of course, each of them had written a book about, respectively, the scientific revolution and the

1 Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, London: Sage, 1979.

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scientific renaissance. Rupert was just a student of Herbert Butterfield. So I was getting all the old 'big-picture' stuff, as it were, straight from the horses' mouths. Eventually, following the money, I did a PhD in the history of medicine and then came to the Wellcome Unit for the History of Medicine in Cambridge.

That's not where I wanted to be, but I will say it worked out really fine because we had the liberty to teach what we wanted in the history of medicine and indeed in the history of science. I was teaching a wider variety of topics there than I think anyone else in the country in the history of medicine, which also worked out well for me. I also met Perry Williams in Cambridge during that time. Perry and I sort of complemented each other in this way – I came from the past and Perry came from the relative present, in that I'd been teaching medicine as natural philosophy with Roger French, which led to our book on the natural philosophy of the friars, and Perry was working on nineteenth-century science and religion. Together, through teaching, we worked out the issues with the big picture and why it was in the wrong place. The issue for me was always about natural philosophy.

Perry Williams: One thing which was particular about Andrew and my experience is that a large number of our students at the Wellcome Unit in Cambridge were medical students. They were not going to become professional historians of science. We couldn't kid ourselves with this. They were going to become doctors. We had them for a limited period and we wanted to teach them the most useful, the most interesting, the most life-enhancing things we could offer these kids who were going to become the doctors of the future. I know that was very much at the forefront of our mind. So we really wanted to address big questions, such as, 'Why is our medicine based on science?'

Ludmilla Jordanova: Re-reading all the papers, I was very struck by how we were all shaped by the social relationships we had, and especially institutional settings. As a third-year undergraduate doing history and philosophy of science at Cambridge, I was supervised by Roy Porter. He made it very clear that he was a historian first and a historian of science second. And then in January 1980, I joined a history department and I've never practised inside a unit devoted to the history of science, medicine or technology since.

All of this does raise some quite fundamental questions about what the history of science really is. For me, it's definitely part of some larger agglomeration. Alongside history there were also links with anthropology and sociology. This was already happening in Cambridge, because of the presence of people such as Marilyn Strathern in the Department of Social Anthropology. I think I was programmed by the time I left Cambridge, which was in 1978, to see the history of science as very deeply connected with anthropology, sociology and mainstream history, as well as women's studies. So for me, the boundaries of this thing called 'the history of science' were very, very fuzzy right from the beginning.

John Christie: Like the other contributors, my immediate motivation came out of teaching. For many years, I taught a final-year undergraduate course on the historiography of science at the University of Leeds. The first half of it covered old history-of-science writings. It started in the early eighteenth century, and it ended in about 1945. Then I used the second half of the course to tackle more or less contemporary themes. I also taught courses in the Department of Philosophy at Leeds – straight philosophy courses, as well as specialized courses for students doing English literature. I was reading and

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teaching texts such as Alasdair MacIntyre's *After Virtue* and Paul Ricoeur's three-volume *Time and Narrative*.[2] So in my original paper, the language of 'emplotment' and 'configuration' comes fairly directly out of reading and teaching MacIntyre and Ricoeur. It's those pedagogical contexts which shaped it. I was trying to meet pedagogical needs of different kinds across two disciplines: history of science on one side, and philosophy on the other.

James Secord: I want to pick up again on the point about pedagogy. Many of the students we taught came into the history of science as natural-science students, certainly the ones I taught at Imperial College at the time, and later at Cambridge. That had really profound effects. Our students were presented with these incredibly rich stories in the history of science. But then, when it came to generalizing out of these, it was philosophical and anthropological work by Foucault and Latour that was seen to be the place to look. Not general narrative histories, such as William H. McNeill's *The Rise of the West*.[3] Historians of science didn't really read that kind of material.

It's best exhibited for me by the fact that, when Bruno Latour's 'Give me a laboratory and I will raise the world' was published, it suddenly felt like a ladder out of this hole of case studies.[4] Networks were suddenly going to solve the problem. All of that was going to be the way forward. But it ended up becoming a kind of shortcut for people. And I think one of the things that's been really difficult for the history of science in the last twenty years is that our students now realize that, to do the job they want to, they need to go through a whole period of post-undergraduate and quite often post-PhD historical training. That means getting the right languages; it means mastering these general histories. I think most of the successful students that I've had in the last twenty years or so have gone through that period of retraining and retooling in order to really do the history of science they want to.

Ludmilla Jordanova: I do think there's something to be said about the broader discipline of history as a whole, beyond the subdiscipline of the history of science. After all, the discipline of history never turned its back on big pictures. In fact, I think that the history of history shows us that big pictures of one kind or another have been absolutely a staple of the wider historical field.

When I was teaching early modern survey courses, I would tell the students that the key to understanding early modern societies was taxation. And I used to do this to provoke them into understanding that economic phenomena are fundamental; they're not the tiresome add-on bits. For people like James Poskett and myself, who have to teach undergraduate survey courses in history departments, we use a lot of big-picture histories. That's part of our job. But I don't see the insights of those big-picture histories translated into the history of science. And I'm wondering why that is.

For me, one of the big-picture historians I most admire is the late lamented Christopher Bayly. Maybe it's easier for historians of science to pick up on elements that they find in people like Foucault than it is for them to pick up on the quite elaborate thinking about the structural elements of societies found in Bayly's *Birth of the Modern World*.[5]

2 Alasdair MacIntyre, *After Virtue: A Study in Moral Theory*, London: Duckworth, 1981; and Paul Ricoeur, *Time and Narrative* (tr. Kathleen McLaughlin and David Pellauer), 3 vols., Chicago: The University of Chicago Press, 1984–88.

3 William H. McNeill, *The Rise of the West: A History of the Human Community*, Chicago: The University of Chicago Press, 1963.

4 Bruno Latour, 'Give me a laboratory and I will raise the world', in Karin Knorr-Cetina and Michael Mulkay (eds.), *Science Observed: Perspectives on the Social Study of Science*, London: Sage, 1983, pp. 141–70.

5 Christopher Bayly, *The Birth of the Modern World, 1780–1914: Global Connections and Comparisons*, Oxford: Blackwell, 2004.

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James Poskett: Yes, I often think about the relationship between the history of science and the wider historical profession. I studied natural sciences at Cambridge, then did a PhD in the Department of History and Philosophy of Science. I now teach in a history department. I teach history undergraduates. I don't teach scientists. To bring it round to Chris Bayly, when I was doing my PhD, I used to go to the weekly world history seminar, which Bayly ran at St Catherine's College, rather than the history and philosophy of science seminar on Free School Lane. Tellingly, the two seminars took place at the same time on the same day. So you really did have to choose between world history and the history of science. That was still the case until very recently.

James Secord: One of the first things that I did when I joined Cambridge in 1992 was approach Bayly so that we could have a joint meeting on science and empire. This was actually the first real contact in post-1800 science in a serious way between the History Faculty and the Department of History and Philosophy of Science in Cambridge. I remember Chris Bayly saying, 'Most of your friends and colleagues must be geologists.' And I just said, 'Well, no. I have a history degree from Princeton; I studied under all sorts of eminent historians, like Lawrence Stone and Robert Darnton and so forth. That was the world I came out of.'

My sense is that historians of science were actually pretty assiduous about reading works in the immediate area of their historical studies. If you were studying seventeenth-century alchemy in 1995, you would know a lot about early modern England. All sorts of aspects of it, not just things that had to do with chemistry and alchemy and so forth. But the problem was that that's where your historical knowledge tended to stop.

James Poskett: Perhaps we can move on to talk a bit more about the content of your papers and your memories of the 1991 meeting itself. Andrew and Perry, your paper is today the most-cited article in the *British Journal for the History of Science*. How was it received on the day?

Andrew Cunningham: Perry and I were working in this area, and because we're a team, I gave the first part of the talk and Perry gave the second part. We were absolutely astounded at the reaction, because we thought this was all common sense, that this was what people were thinking, but it turns out they weren't. And it was the most exciting presentation I've ever given in that respect.

Perry Williams: 'Exciting' is one word for it! As Andrew says, we went in with a fair degree of confidence. This was stuff which seemed fairly familiar to us, and we'd shared it around the Department of History and Philosophy of Science in Cambridge. We'd been giving lectures based on this for some time. But I remember the Q & A afterwards being almost entirely hostile. I said to Andrew afterwards, 'Look, don't be too discouraged because I think all of the responses can be summarized as either, "this is obviously untrue," or "we know this all already."' And Andrew said, 'I just hope it doesn't go from, "this is obviously untrue" to "we know this all already," without there ever having been a moment at which we said it.'

James Poskett: Did you ever think of writing more on the topic?

Perry Williams: We were actually going to write a book on this, which was going to be called *The Invention of Science*. Our plans for the book had got to quite an advanced stage; we'd actually got as far as finding an agent, and we produced a detailed book proposal, a couple of sample chapters. The agent was keen, found us a publisher, who was

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keen, who sent it out to a reader. The reader's report came back, and said basically nobody in the profession of the history of science is going to agree with this. That put a lid on it as far as that publisher was concerned and it sent us back to square one. We got rather discouraged at that point, because we couldn't think how to rework it. We'd more or less put it in a box and forgotten about it, when Jim's invitation came along.

And then after the reception to our talk – which reminded me of the opening titles of the 1970s television series *Kung Fu*, the bit where they're all throwing spears at him and he's knocking them away – we realized that we'd need to argue our case much more tightly and rigorously. So the article as published bears only a tangential relationship to what we actually delivered at the conference, which was really quite casual. Some of the argument is the same, but the manner of it is very, very different; we had to make it super strong. And we're deeply grateful to have had the chance to take a second crack at it.

James Poskett: John Pickstone sadly passed away in 2014. I wonder if anyone could share their memories of Pickstone's paper. Notably, he did later turn his paper into a book, with the publication of *Ways of Knowing*.[6]

James Secord: John Pickstone had a very strong interest in trying to think about why people approached the world in the way they do. It was expressed through the way he brought categories, largely those that had been used in the history of medical practice, into the history of science. One of the really positive outcomes of the 1991 meeting was Pickstone's *Ways of Knowing* book, and the various articles that went into that as well. In many ways the meeting crystallized people's fundamental motivations for why they went into the history of science in the first place. In that sense, the big-picture meeting helped address these larger questions, such as why these subjects really matter.

Ludmilla Jordanova: A quite immediate response might be that, for my paper, working on gender was connected with second-wave feminism. But I don't think that's really enough, because actually second-wave feminism could have led to much more emphasis on the many important women left out of the history of science, as you find in work by people such as Margaret Rossiter. That was never particularly my interest, although I think it's incredibly useful.

I was interested in themes such as reification, which comes up in my paper, and also commodification. And I have to say that I think one of the most saddening realizations that I've had over my career is that these are still very acute problems, and in some ways have got worse in terms of the packaging of women as objects of consumption. I really did think around the time that this paper was written that gender relations were going to change fundamentally. I think they have changed in some ways: for instance with current debates around transsexuality and transgender rights, which are actually, I think, quite compatible with what I say in the paper. But I don't think that they have changed in terms of the ease with which women can become professionals or assume authority. So some of those more abstract questions on gender and science are still at the heart of contemporary societies.

James Poskett: Absolutely, Ludmilla. Your paper seems very much on the cusp of second- and third-wave feminism, if you like. I was really struck by your use of Marilyn Strathern, and the way in which you talk about not just gender, but issues of sexuality,

6 John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine*, Manchester: Manchester University Press, 2000.

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these more fluid boundaries that were influencing how feminism was developing around 1990. Could you give us a little bit more perspective on that?

Ludmilla Jordanova: Yes, I did work with Marilyn Strathern. In fact, the first 'proper' thing I published was in *Nature, Culture and Gender*, which Marilyn and Carol MacCormack edited.[7] Carol and I were working at the same college in Cambridge, where I was a research fellow. If you talk to someone every single day about their fieldwork in Sierra Leone it really does affect you. I also supervised a ton of PhDs when I was at Essex. I think I did so partly because there weren't many other people available to supervise in some of these areas related to gender and science in the 1980s. I supervised what I think was the first PhD on the history of infertility, with Naomi Pfeffer, who then wrote a book called *The Stork and the Syringe*.[8] I also supervised a PhD on cross-dressing, so that idea of fluidity was being explored. Before then, at Oxford, I'd supervised Ornella Moscucci's work on the history of gynaecology, again quite an innovative thing. Once you've spent years working with someone, even if it's not your area of research, it is sort of infused into you. I think more and more that it's these sustained personal relationships that matter.

I also started working in the history of art, and I do think the history of art is a rather interesting field to compare with the history of science. For me, one of the key figures has been Michael Baxandall, particularly the way that he has paid attention to forms of thinking. You find it in Baxandall's *Painting and Experience in Fifteenth-Century Italy*, which sounds as if it ought to be incredibly specialized, but actually has the most broad ramifications.[9] It's about people who show us how we can do particular kinds of work with categories and how the categories, although they are abstract, are also built into everything else – institutions, careers, identities and so on. I think that's the problem that I keep coming back to.

James Poskett: We've discussed the personal, the intellectual and the institutional contexts. Maybe we could move on to the wider social and political context of the early 1990s. How did that shape your approach to the history of science and the 'big picture'?

Perry Williams: In 1993 we were coming to the end of a very long period of Conservative government in this country dominated by the three Thatcher administrations. This was a period of sustained challenge to universities. There was increasing pressure on universities to become more cost-efficient, to justify themselves in economic terms. The demand for academics to justify themselves was probably fair enough, but we certainly wanted to resist the idea that the only sort of academic work which was any good was the kind which would produce money. John Pickstone's conference presentation, but not his published paper, actually included a reference to Andrew Davies's television series *A Very Peculiar Practice*, which was set in a university. It was a satirical examination of the Thatcherite efforts to reform and reshape universities. So that was another little political reference.

Andrew Barry: The other thing, of course, is the Research Assessment Exercise (RAE) came in in 1986. My PhD supervisors developed techniques of research assessment, and one of

7 Carol MacCormack and Marilyn Strathern (eds.), *Nature, Culture and Gender*, Cambridge: Cambridge University Press, 1980.

8 Naomi Pfeffer, *The Stork and the Syringe: Political History of Reproductive Medicine*, Cambridge: Polity Press, 1993.

9 Michael Baxandall, *Painting and Experience in Fifteenth-Century Italy: A Primer in the Social History of Pictorial Style*, Oxford: Clarendon Press, 1972.

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their concerns was to provide a set of 'radical statistics' which might disrupt established hierarchies using some objective measures. Of course, the self-satisfaction of established institutions was rather disturbed by the neoliberal onslaught which happened in the 1980s and 1990s. Yet, despite the rhetoric of neoliberalism, both research assessment and the later introduction of high student fees in 2011–12 acted to reinforce disciplinary boundaries at the time when arguments within disciplines themselves pointed in the other direction. So there has been a complex play of forces. In effect, major shifts in government policy associated with neoliberalism unintentionally served to reinforce disciplinary boundaries at a time when strong arguments were being made for greater interdisciplinarity across both the social sciences and humanities.

John Christie: If you lived in Leeds and Sheffield in the 1980s, what was crystallizing things for you was the miners' strike. If you lived in Yorkshire, that was on the street, that was just outside the door. I taught a course for engineers called 'Technology and Society' so I decided to use the miners' strike in it. I was able to use the work of academics in Leeds working in the Adult Education Department and they were used to working on Coal Board material. You may remember Arthur Scargill's number of seventy pit closures and the loss of 70,000 jobs. What my adult education colleagues had done was to locate that in the very specific techno-scientific context of the mine operating system (MINOS). This is where you just sat and controlled everything from a computer control room up at the top of the pit. Hardly anyone knew about it – not many people seem to know about it now – but if you were doing history of techno-science in Yorkshire in the 1980s it was very crystallizing.

James Poskett: Let's talk about what happened after 1993. How did the field develop? And why are we still struggling to articulate 'big-picture' histories of science thirty years on?

Ludmilla Jordanova: I did a little survey recently for my own purposes of books on the scientific revolution and what they say about William Harvey. I was struck by the very little change that there has been since 1993. I think to address that we need a completely different way of approaching what we do, which is to understand the publishing industry, to understand the media, to understand how museums get recognition for exhibitions, and how they write labels and panels and that kind of thing. At the same time, I'm passionate about the history of science, but I'm not very invested in the idea of the history of science as a ring-fenced discipline. I think we ought to be more proselytizing for the value of the history of science.

Perry Williams: I pretty much left the discipline after the conference. That wasn't a reaction to our hostile reception! I'd actually been planning my exit for some time and writing the book with Andrew – which turned into the conference paper, which turned into the article – was going to be the last major piece of history of science that I did. So I've been out of the field for many years now, but I am delighted to see the new work in the post-imperial direction because we were gesturing towards that in the last sections of our paper.

The only other thing I wanted to throw in was a point of comparison following up on what Ludmilla said about the history of art. Two or three years ago, the BBC did an effort to go back to Kenneth Clark's *Civilisation* series, which is a big-picture history of art. Faced with the challenge, they called it *Civilisations* – plural – and they got three presenters: Simon Schama, Mary Beard and David Olusoga, who all had completely different styles. All good shows in their own way, but did it add up to a new big picture? I don't think so. I think that's an interesting comparison which we might make. And actually, in our book,

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the book which never got written, the first chapter was going to be 'The invention of art'. And in our fantasies, that book was going to be tied into a thirteen-part television series.

James Secord: My sense is, during the last decade or so, things have changed quite dramatically. The field is moving in ways and it isn't exactly clear where it's all going to end up. It's easy to summarize in terms of moving away from the European focus of the history of science, but it's actually much more complicated. I do often feel that a lot of the really important lessons of the work that was done during the 1970s and 1980s are in danger of being lumped under a rather simplistic Eurocentric dimension. There are some real issues here about how the new history of science, if it's going to retain that title, is actually going to proceed forward.

John Christie: I think I'll come at this through my post-retirement experiences. I was enabled to continue working relatively productively by spending a lot of time at the Max Planck Institute for the History of Science in Berlin. What I most enjoyed about that was the age range of our colleagues; there was everyone from retired people like me, like David Bloor, who was often there at the same time, and people who were just starting their PhDs. And I noticed both from my Berlin experiences across the 2010s and also from a lot of conference attendance, particularly in the United States, that the preoccupations of my younger colleagues were really quite dramatically different from my own. They were very much preoccupied with, broadly speaking, what you might call postcolonial perspectives on history, particularly histories of imperialism and the role of the sciences and techno-sciences. With a kind of – I don't mean this to sound critical – a fetishization of Indigenous knowledge. So though I'm pleased in a sense that highly politicized themes are taken up and with excellent work, I do see limitations in it. I see problems with bringing it together for any kind of new and convincing 'big picture'.

The other thing is that the old pictures didn't go away. The 2010s saw at least six publications which are reviving older 'big pictures', such as H. Floris Cohen's *How Modern Science Came into the World* and David Wootton's *The Invention of Science.*[10] And more recently, a rather different book, with a very different perspective, Jürgen Renn's *Evolution of Knowledge: Rethinking Science for the Anthropocene*, which is kind of a big picture as well.[11] This is a book sensitive to younger colleagues' enthusiasm for the Anthropocene concept, which is indeed a global concept.

Andrew Barry: I've partly returned to an interest in the history of science through trying to investigate a question, which has become significant recently, which is the question of the strange absence of chemistry in the lexicon of the social sciences. We have the existence of physical anthropology and biogeography, but no 'chemical anthropology' or 'chemical geography' and so forth. The historical point I've started with is an isolated lecture by William Jackson Pope, president of the Chemical Society in Cambridge at the end of the First World War. Pope, who was also a government scientific adviser on weapons, talks about chemical geography as basically about the geography of the distribution of resources in empire.

10 H. Floris Cohen, *How Modern Science Came into the World: Four Civilizations, One 17th-Century Breakthrough*, Amsterdam: Amsterdam University Press, 2010; and David Wootton, *The Invention of Science: A New History of the Scientific Revolution*, London: Penguin, 2015.

11 Jürgen Renn, *The Evolution of Knowledge: Rethinking Science for the Anthropocene*, Princeton, NJ: Princeton University Press, 2020.

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One big picture, of course, has been the emergence of the Anthropocene, which you could translate as the chemical transformation of the planet. The Anthropocene and the debates around it are markers of the fact that the techno-sciences are much more intricately bound up with the history of the planet than they once were. The chemical transformation of plants, oceans, materials and so forth has become much more central to the history of the sciences. But I would say that there are multiple big pictures, particularly prompted by a return of thinking about the relation between capitalism and environmental change, but also clearly around rethinking colonialism and imperialism.

James Secord: One of the things to stress is the sheer difficulty of the retooling that's under way. I think the lack of support for whole areas of study, for fields like Assyriology or the understanding that people might have of Mayan culture, or the difficulty of finding people who know about ancient Chinese history within European or American institutions, these are things that are really important to bring out. I think we need to accompany our geographical extension of the history of science with a temporal extension to really understand how these systems work.

Andrew Barry: The other problem, which I think Jim has raised powerfully, is that the scholarship is becoming more professional and technical, and, particularly given the necessity of moving away from non-Eurocentric history of science, the need for proficiency in languages becomes more extended and demanding as well. So there is kind of the contradiction, in which we're still wedded to a funding model, which primarily focuses on the three- or four-year PhD, which is completely inadequate in most domains. Moreover, outside history – certainly in my discipline, geography – there's very little concern with language training. These are profound structural problems in the ways in which training happens in British universities.

James Secord: Another thing that happened around 1993 was the turn to practice. Interestingly, that movement wasn't as closely related to practice in everyday life as it could have been. One of the most positive things that's happened in the last ten years or so is the increasing attention to subjects like practical agriculture and food. It's not just that science is part of everyday life, it's that there isn't a division between those two things. This is significant, because it's much easier to write a *longue durée* history of something like food, health or sex than it is to write a *longue durée* history of, say, ideas of the infinite or evolutionary theory. It seems to me that the really foundational things that people have in common over many cultures and periods are really these practical concerns.

That ultimately relates back to the point that Andrew Barry was making about the Anthropocene. We can see it in my own field of the history of geology. For many years, people said that the rise of geology had little or nothing to do with mining. Well, a year after I retired from Cambridge they had a meeting in the Department of History and Philosophy of Science on the history of coal. It was absolutely unthinkable for the nearly thirty years that I was actually teaching that such a meeting could have been organized and that anyone would have wanted to come. So it's really fantastic to see that kind of change. I think a lot of it does come from the acute awareness that people have, rightly, of the impending environmental catastrophe.

Andrew Barry: I like this theme of rethinking the history of science through the history of everyday experiences and substances and so forth, which cut across the boundaries

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between specialist and non-specialist forms of activity. At the height of the COVID-19 pandemic, in late 2020, the faculty in University College London ran a course which drew together all of the social sciences, science and technology studies, history and the history of science into a rich account of the historical experience of pandemics. This was not a synthetic totality or synthesis of a singular big picture. But in effect kind of a big picture did emerge from a set of fragments. All of these interventions were in conversation with present concerns with pandemics and planetary health – by bringing together fragments it was possible to interrogate and re-pose contemporary problems.

James Poskett: I couldn't agree more. That's my motivation for writing the history of science, and for convening this special issue. I see the 'big picture' as a way in which historians of science can speak to the present in lots of different ways. It's about contemporary political challenges, which in my view are a contest over the relationship between past, present and future.

That just leaves me to say thank you again to you all for joining me today. It has been really wonderful to hear your memories of the original 1993 *Getting the Big Picture* special issue of the *British Journal for the History of Science*. Only time will tell if we're still discussing these issues in another thirty years.

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