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Historiography of science and technology in focus.

A discussion with Professor Robert Fox

Abstract

The article is an extended discussion with a laureate of numerous international distinctions, Professor Robert Fox, about his career, intellectual fascinations, as well as changing methods, styles, approaches and themes in the historiography of science and technology.

Keywords: *Robert Fox, history of the historiography of science and technology, discussion.*

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Dyskusja z profesorem Robertem Foxem o historiografii nauki i techniki

Abstrakt

Artykuł przedstawia obszerną dyskusję z laureatem licznych międzynarodowych wyróżnień, profesorem Robertem Foxem, o jego karierze, fascynacjach intelektualnych, a także o zmianie metod, stylów, podejść i tematów w historiografii nauki i techniki.

Słowa kluczowe: *Robert Fox, historia historiografii nauki i techniki, dyskusja.*



Fig. 1. Robert Fox in the Temple de l'humanité, rue Payenne, Paris, 6 December 2016.
Photograph by David Lebreure. Courtesy of the Maison d'Auguste Comte, Paris.

1. Introduction

MK: Basic information about Robert Fox can be found in the works mentioned in Bibliography A, especially in his *Curriculum Vitae* of 2006, and in his own works mentioned in Bibliography B. I firmly believe that it is worth developing the biographical information with more specific points related to the expertise of a historian of science and the variability of methods or approaches in the study of the history of science – cf. Bibliography C, including Kokowski [1999](#); [2001a](#), pp. 14–21 (in Polish, with a summary in English: [2001b](#)); [2007](#); [2012](#).

2. Career beginnings

MK: The 1950s and 60s in the USA and the Great Britain mark a tipping point in the development of the history of science as an academic discipline. Let me recall selected episodes from the period.

In 1952, 2nd ed. of *Essays in the History of Ideas* by Arthur O. Lovejoy (1st ed. 1948) and 1st ed. of *Augustine to Galileo. The History of Science A.D. 400–1650* by Alistair C. Crombie are published; A.C. Crombie subsequently lectured on the history and philosophy of science at University College, London.

In 1953, *Robert Grosseteste and the Origins of Experimental Science, 1100–1700* by Alistair C. Crombie is issued, and the author moves from University College, London to become the University of Oxford's first lecturer in the history of science (Fox [2006a](#), p. 71).

In 1954, *Science in History* by John Desmond Bernal is published.

In 1956, George Sarton dies recognized as the “father” of the history of science as an academic–university discipline (at least in the USA), and the 3rd ed. of his book *The History of Science and the New Humanism* is published (1st ed. 1931).

In 1957, 2nd ed. of *Augustine to Galileo* (2 vols.) by A.C. Crombie, as well as 1st ed. *From the Closed World to the Infinite Universe* (Baltimore: The Johns Hopkins Press) by Alexandre Koyré, and 1st ed. of *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* by Thomas Samuel Kuhn are published, I. Bernard Cohen writes an article “George Sarton” in *ISIS*.

In 1961, in Oxford, A.C. Crombie together with Rom Harré (who was lecturer in the philosophy of science) organizes the conference entitled “The structure of scientific change”. The conference is attended, among others, by T.S. Kuhn, who lectures on “The Function of Dogma in Scientific Research”.

In 1962, *The Structure of Scientific Revolutions* by T.S. Kuhn is published.

In 1963, the proceedings of the conference mentioned above, entitled *Scientific Change. Historical studies in the intellectual, social and technical conditions for scientific discovery and technical invention, from antiquity to the present* (ed. A.C. Crombie), are published. The publication contributed to the fundamental rethinking of the methodological foundation of the history of science that marked the 1960s and 1970s.

In 1964, Alexandre Koyré dies, having greatly influenced British and American historians of science (cf. I. B. Cohen 1987; Crombie 1987).

On the other hand, in 1957, at the age of 18, you finish the Grammar School and earn the General Certificate of Education with an Advanced Level Record in Greek, Latin and French and a Scholarship Level in Latin and French, and your adventure with science begins with the study of physics; first at [Imperial College of Science and Technology at the University of London](#) (1957–1958) and then in [Oriel College at the University of Oxford](#), where you receive a BA in physics in 1961 and an MA in 1965. In the meantime, from 1961 to 1963, you work as Assistant Master (Physics) in [Tonbridge School, Kent](#) (an elite independent school for boys). Already in 1967 you have a doctorate in the history of science from [the Faculty of History at the University of Oxford](#), supervised by the famous Alistair Cameron Crombie. Your thesis was “The study of the thermal properties of gases in relation to physical theory from Montgolfier to Regnault”.

In this context, I am curious why you chose physics as a field of study and which topics you analyzed in your works towards your first degrees (BA and MA)?

RF: The path that led me to physics was a curious one. My original intention had been to study ancient languages (Latin and Greek) at university. But towards the end of my school days I decided to change to the sciences, which entailed my taking a so-called “transfer year” of mathematics, physics, and chemistry at Imperial College in London in 1957–58. This was part of a short-lived scheme, launched in the mid-1950s, to encourage students who had concentrated on humanities subjects at school to switch to science or engineering at university. After that year I moved to Oxford, where I followed the three-year undergraduate programme in physics from 1958 to 1961. The course covered all the main branches of physics, mainly taught within my college by the physics tutor in Oriel, Dr John Sanders, a specialist in early work on masers. In addition, there were regular practical classes and departmental lectures in [the Clarendon Laboratory](#). But the weekly one-hour one-to-one tutorial with Sanders was what set the pace. There was no “humanities” dimension in the curriculum.

MK: *However, when and why did you become interested in the history of science? Did it happen already before or only during your studies in physics?*

RF: Partly because my original interests at school had been linguistic and literary, I had always been drawn to the more “humane” aspects of physics, to the point that I knew, on graduation, that I did not want to proceed to doctoral work as a physicist. It was while teaching physics at Tonbridge School immediately after graduating that I “discovered” the history of science through chance encounters with books in the school library. The first book on the history of science that I bought was Charles Singer’s *A Short History of Scientific Ideas to 1900* (1958), a work cast very much in the “history of ideas” mold and in the rather Whiggish manner of the time.

MK: *How did you become acquainted with the famous Alistair Cameron Crombie (by the way, I love his writings and I studied them from nearly the beginning of my interest in the history and philosophy of science)?*

RF: I read *Augustine to Galileo* while still teaching at Tonbridge. This contributed to my decision to return to Oxford to work for the doctorate, known as the D.Phil. Fortunately, I was eligible for a doctoral grant from the Department of Scientific and Industrial Research on the basis of my undergraduate work in physics, and the DSIR allowed me to take up the award for an historical thesis. At the time (1963), Alistair Crombie was Oxford’s only historian of science, and he became my supervisor, even though our interests were very different: he was a medievalist and I decided to work on eighteenth and nineteenth-century physics.

MK: *What concerned your studies and research in 1965–1966, when as a Clifford Norton Junior Research Fellow ([The Queen’s College, University of Oxford](#)) you wrote your doctoral thesis?*

Though your work was related to the history of physics in the eighteenth and nineteenth century, particularly in France, did you – at the time – study also the history of science of ancient, medieval and modern times?

Let me ask the question in another way:

- a) *When did you become acquainted with the works of, for example, George Sarton, your supervisor Alistair Crombie, Alexandre Koyré or Thomas Samuel Kuhn?*
- b) *Did you have any other favourite authors at the time?*
- c) *What was your style of research and writing on the history of science back then, and what works influenced your style?*

RF: As a graduate student, I tended to engage with primary sources in a manner that the French would call *explication de texte*, i.e. through a focused, systematic examination of a text. That squared well with Alistair Crombie's approach, not that Crombie imposed any methodological or other constraints on the way I worked on my thesis. He taught rather through example, in his weekly graduate seminar, often conducted with the philosopher of science Rom Harré. There his immense erudition and capacity for philosophically informed analysis shone through a rather diffident manner. Like others in the tiny group of graduate students at the time (just one other doctoral candidate entered in my year), I admired the intellectual ambition that resulted many years later in his three-volume *Styles of Scientific Thinking in the European Tradition* (1994). *Styles* encapsulated his guiding principle that the history of science was and should be, above all else, a history of scientific *thought*, richly contextualized and analysed through an approach that he described as "a kind of comparative intellectual anthropology".¹ Although certain parts of my own work shared something of his perception of science as part of a broader "intellectual culture", I have explored other approaches as well and never wanted to be bound by this or any other methodological school.

To my great good fortune, in 1965 Crombie launched a new advanced paper for final-year undergraduate historians on "The scientific movement of the seventeenth century" and asked me to give tutorials for the paper. Through classes and informal discussion with Crombie, I became familiar with some of his favoured authors: Paul Tannery, E.A. Burt, Pierre Duhem, Robert Lenoble, and Alexandre Koyré, the last two of whom he particularly respected.² Quite independently, I read and admired Thomas Kuhn's *Structure of Scientific Revolutions*, which had been published in 1962, following the paper he had read at the Oxford conference of 1961 on "Scientific change", organized by Crombie and Harré.³

¹ Cf. Crombie 1994, vol. 1, pp. 1–89 ("Historiography of Science").

² Among the scholars mentioned, Robert Lenoble (1902–1959) may be commonly the least known. Cf. his *Mersenne: ou la naissance de mécanique* (1943), and see Lenoble's concise biography: Costabel [1959](#).

³ Cf. Kuhn 1962; 1963.

3. University of Lancaster 1966 to 1988

MK: After the doctorate you moved to the University of Lancaster, where you worked from 1966 to 1988 in [the Department of History](#). For the first few years you continued your research in the history of physics, which is evidenced by the monograph *The caloric theory of gases from Lavoisier to Regnault* (1971; pp. xvi + 378), the article “The Rise and Fall of Laplacean Physics” (1974), and the monograph *Sadi Carnot. Réflexions sur la puissance motrice du feu. Edition critique avec introduction et commentaire, augmentée de documents d’archives et de divers manuscrits de Carnot* (1978, pp. 371); also editions in English (1986), German (1988), and Italian (1992).

But at the end of the 1970s your research interests were expanding with the subject of social and institutional history of science and technology in France. This bore fruit in a collective monograph *The organization of science and technology in France, 1808–1914* (1980; pp. x + 355) co-edited by you and George Weisz.

What caused this change of your interests?

Was it the influence of the books, such as: a) “The Social Function of Science” (1939) or “Science in History” (1954, with four editions until 1971) by John Desmond Bernal, or b) “Scientific knowledge and its social problems” (1971) by Jerome R. Ravetz (known to me from his Copernican research and contacts with my Institute), or c) “Science, Technology, and Society in Seventeenth Century England of 1938” by Robert K. Merton (reprinted in 1970)? Or only the works of the authors of the anglophone sociology of scientific knowledge: David Bloor, Barry Barnes, Hary Collins, Steven Shapin, and Simon Schaffer, and also the authors of laboratory ethnography or ethnomethodology of science: Ludwik Fleck, Bruno Latour, Steve Woolgar, and Karin Knorr-Cetina?

RF: I spent a wonderful last year in Oxford as Clifford Norton Junior Research Fellow at The Queen’s College, before moving to a lectureship in the History Department at the University of Lancaster, which had been founded only two years before (in 1964). Lancaster was one of half a dozen universities, the so-called “plate-glass universities”, that were founded in Britain in the 1960s. The new universities were exciting places, all of them committed to striking out in novel directions, in both research and teaching. Lancaster encapsulated the innovative spirit,

not least in a History Department fashioned by a remarkable founding professor, Austin Woolrych, a specialist on the English civil war. Woolrych had come from the University of Leeds, where he had encountered the distinguished group in History and Philosophy of Science led by Jerry Ravetz. Part of his plan was that the history of science, like other unusual areas of history, should find a home in the Lancaster department, and so it proved. By 1974, with Woolrych's backing, three additional appointments had been made: John Hedley Brooke, Roger Smith, and Peter Harman. To have four historians of science fully integrated in a history department was unique in Britain at the time, and it remains rare even today.

You are quite right about the change in the profile of my interests during those Lancaster years. The change was largely a consequence of my teaching in a history department, with undergraduates who had little background in science. This led me to develop courses that integrated the study of the content of science with perspectives modelled on those of cultural history, as applied in literary history and art history, for example. In that respect, I was responding to the particular circumstances of Lancaster.

Perhaps at this point, a further word about Jerry Ravetz, someone who was a major influence in my own early steps as an historian. I had met Ravetz early in my time as a graduate student and consulted him quite regularly in Leeds (far from Oxford but close to where my parents lived at the time). He was a captivating person, immensely lively, widely read, and unfailingly generous in his comments on what I wrote (which shared common ground with an interest of his at the time, in Joseph Fourier). Despite my having no formal affiliation with him or with the University of Leeds, I am still conscious of my debt to his sparkling intellect and gifts as a teacher. My work on Laplacian physics, in particular, bears many traces of those discussions in Leeds.

4. Oxford University 1988–2006

MK: You have intensified your research, between 1988 and 2006, working as Professor of History of Science and Fellow of [Linacre College at the University of Oxford](#). We can divide your numerous works, published in this period, into four thematic groups:

Social and institutional history of science and technology in France

- 1) *The culture of science in France, 1700–1900* (Aldershot: Variorum, 1992), pp. xiii + 335.
- 2) *Science, technology, and the social order in post-revolutionary France* (Aldershot: Variorum, 1995), pp. xiv + 291.
- 3) Editor (with Anthony Turner) of *Luxury trades and consumerism in Ancien Régime Paris. Studies in the history of the skilled workforce* (Aldershot: Ashgate, 1998), pp. xviii + 307.

History of technical education and applied research in modern Europe

- 1) Editor (with Anna Guagnini) of *Education, technology and industrial performance in Europe, 1850–1939* (Cambridge: Cambridge University Press, and Paris: Editions de la Maison des Sciences de l'Homme, 1993), pp. xiv + 302.

History of technology in modern Europe

- 1) Editor of *Technological change. Methods and themes in the history of technology* (Amsterdam: Harwood Academic, 1996), pp. viii + 271.
- 2) Editor (with Agustí Nieto-Galan) of *Natural dyestuffs and industrial culture in Europe 1750–1880* (Canton, MA: Science History Publications, 1999), pp. xx + 354.
- 3) (With Anna Guagnini) *Laboratories, workshops, and sites. Concepts and practices of research in industrial Europe, 1800–1914* (Berkeley, Ca.: Office for History of Science and Technology, University of California at Berkeley, 1999), pp. x + 214.

History of exact sciences (in France or in Great Britain):

- 1) Laplacian Physics, in: Robert Cecil Olby, John Christie, and Jonathan Hodge (ed.) 1990: *Companion to the History of Modern Science*. London, New York: Routledge, pp. 278–294.
- 2) GILLISPIE, Charles Coulston; FOX, Robert; GRATTAN-GUINNESS, Ivor 1997: *Pierre-Simon Laplace: 1749–1827: a life in exact science*. Princeton, New Jersey, USA: Princeton University Press). ISBN 9780691011851. 2nd ed. 2000, pp. 336.
- 3) Editor of *Thomas Harriot. An Elizabethan man of science* (Aldershot and Burlington, VT: Ashgate, 2000), pp. xii + 317.
- 4) Editor (with Graeme Gooday) of *Physics in Oxford 1839–1939. Laboratories, learning, and college life* (Oxford: Oxford University Press, 2005), pp. xxii + 363.

It is in these works that you developed a methodological perspective that unites cognitive and social considerations. Could you explain your approach in more detail? What are the subsequent development stages of this approach?

RF: The Oxford chair presented special challenges. As a post rooted in the History Faculty (rather than in science or philosophy), it imposed limitations, similar to those in Lancaster, on the kind of history of science that could be taught. Although I did as much undergraduate teaching as I could manage, I saw the development of a graduate programme as the highest priority. Since the chair was the only teaching post in the subject, this entailed collaboration with colleagues in [the Museum of the History of Science](#), [the Wellcome Unit for the History of Medicine](#), and a particularly receptive group of economic historians. The result was what soon developed as a flourishing one or two-year course leading to a Master's degree in the history of science, medicine, and technology, leading on to the research degree of D.Phil. In that multi-disciplinary context, I expanded my long-standing interest in Sadi Carnot and the early history of power technology into a wider engagement with the history of technology. This led on to the major conference of 1993 on "Technological change" and to *Laboratories, Workshops, and Sites* (1999), a book written with Anna Guagnini, who had collaborated with me on the history of technical education and industrial research since my Lancaster days.

In my years in the Oxford chair, I was blessed with a succession of externally funded postdoctoral collaborators. In addition to Anna Guagnini, Agustí Nieto-Galan worked with me on the history of natural dyes, and Graeme Gooday and I collaborated in the general area of the history of physics, including a multi-authored study of physics in Oxford between 1839 and 1939. To my satisfaction, Guagnini, Nieto-Galan, and Gooday all now hold major academic posts, in Bologna, Barcelona, and Leeds respectively.

5. After retiring

MK: After retiring from Oxford University, you are still continuing research into earlier problems, giving lectures (in several countries, particularly in the USA), and writing and publishing works. We can divide these works into three thematic groups:

History of physics:

- 1) Editor (with Jed Z. Buchwald) of *The Oxford handbook of the history of physics* (Oxford: Oxford University Press, 2013), and the author of Chapter 13: Laplace and the Physics of Short-Range Forces, pp. 406–431.

Science and cultural politics in France

- 1) *The Savant and the State. Science and Cultural Politics in Nineteenth-Century France* (Baltimore: Johns Hopkins University Press, 2012), pp. viii + 394.

International science

- 1) Lecture “Science International” (OSU Oregon State University – School of History, Philosophy, and Religion, 2013):
 1. [Mapping the Universe of Knowledge](#) (14 June).
 2. [Universalism in Action – The Age of Exhibitions](#) (17 June).
 3. [Legacy of a Fractured World](#) (19 June).
- 2) Monograph *Science without Frontiers: Cosmopolitanism and National Interests in the World of Learning, 1870–1940* (Corvallis, Oregon, USA: Oregon State University Press, 2016), pp. 168.
- 3) Lecture “Science without Frontiers: Cosmopolitan Ideals and National Interests in the World of Learning, 1870–1940” (given at the Seventh International Conference of the European Society for the History of Science in Prague on 23 September 2016, following the presentation of the Alexandre Koyré medal of the International Academy of the History of Science) and an article under the same title in the *Studia Historiae Scientiarum* (in English and Polish – cf. Fox 2017a; 2017b; see also Fox 2016b).

In my opinion these three thematic groups are mutually complementary. I mean that they describe complementary aspects of the complicated process of developing science in which science interacts with all parts of culture and society. In the case of Laplace, you consider the Laplacian programme of physics, in the case of science and cultural politics in France – the public face of science, and in the case of international science – the cosmopolitan ideals and national interests of science policy. Do you agree with this view?

RF: Retirement gave me the opportunity of completing *The Savant and the State* (2012), on which I had been working for much of my time in the Oxford chair. It also allowed me to accept teaching posts, for a semester in each case, at Johns Hopkins University in Baltimore and East

Carolina University in Greenville (NC) and, for a shorter time, at the Czech National Technical University in Prague. An invitation to give a series of lectures at Oregon State University in Corvallis in the spring of 2013 led on to *Science without Frontiers: Cosmopolitanism and National Interests in the World of Learning, 1870–1940* (2016a). In this short book, which also benefited from a stay at the Chemical Heritage Foundation in Philadelphia in the fall semester of 2013, I explored the relations between the internationalist ideals of science and the national interests that have done so much to frustrate (and occasionally support) those ideals since the later nineteenth century.

In these various contexts, I have tried to convey my conviction that, as historians, we should seek to combine the rigorous study of the cognitive dimension of science with a sensitivity to context conceived in the broadest possible terms. That conviction informed the way I sought to bring science and the “cultural politics” of France together in *The Savant and the State*, as it did on an international scale in *Science without Frontiers*. As to how we achieve that union of content and context, I can offer no prescription. The tools and approaches we use as historians must vary with the historical problem being tackled, and the skill of the historian resides in making a judicious choice from the menu of possible approaches. I see critical eclecticism, with a constant and overriding respect for the sources, as the key to good historical writing.

MK: *In other words, it seems that you are an advocate of «de-centring the ‘big picture’ of modern science,⁴ on the one hand, and of Jack B. Morrell’s style of “treating the content and context of science as facets of a single historical enquiry”,⁵ on the other. Do you agree with this opinion?*

RF: If by de-centring we mean a scepticism with regard to statements about the nature of science that take no account of the culture in which they are made, I suppose I am taking that route. And, on the other hand, yes I do favour an approach of the kind that Morrell articulates, in which content and context are seen as facets of a single historical enquiry. That said, there is perfectly valid research that we do as historians to which context is rather peripherally relevant. When you are

⁴ Cf. Cunningham, Williams [1993](#).

⁵ Cf. Morrell [1971](#); Fox [2011](#).

trying to work through the complicated logic of Sadi Carnot's *Réflexions sur la puissance motrice du feu*, for example, your primary focus must also be the text. Of course, what you write has to be informed by a recognition that the contemporary problems of power technology and the French economy more generally helped to direct Carnot's attention to the subject. But the overriding goal remains an elucidation of the text.

6. Recent years' research interests

MK: *What a research topic has caught your attention in recent years?*

RF: Since the publication of *Savants and Patriots* (2012), I have returned to my long-standing interest in reputation and career-building in science, exemplified in the contextualized microhistory that I am writing in my biographical study of Thomas Garnett.⁶ In a different register, I have also turned increasingly to the international dimensions of science, with special reference to questions concerning the universality of scientific knowledge and its transmission between nations, cultures, and individuals. *Science without Frontiers* (2016a) was one expression of that focus, written (by the end, though not at the start) against the backdrop of my deepening anxiety about the misconceived and profoundly damaging conceptions of national interest exemplified in Brexit and kindred political trends in Europe and across the Atlantic.

That new focus has brought me back to an early interest in Auguste Comte and the tradition of positivist thought not only in France but also in Britain, where the Comtean Religion of Humanity⁷ had significant resonance in the later nineteenth and early twentieth centuries, and Latin America, where positivism became bound up with emerging national identities, notably that of Brazil: remember the words “Order and progress”, Comte's words, on the Brazilian flag, and the Brazilian positivists who more than a century ago fitted out the Temple of Humanity in Paris, now handsomely restored and open to the public (see Fig. 1).

There is certainly something extravagant about Comte's ideas, and overblown in his expression of them; and his authoritarianism has its disturbing side. But his vision of science as a driver and exemplar of

⁶ For further details, see below, chapter 8. “Historiographical genres”.

⁷ Cf. Simons [2017](#).

progress, and his idea of a secular morality rooted in science intrigue me. Today, we may see Comte's analysis of scientific change as limited and un-subtle, and it is easy to understand John Stuart Mill's rejection of the Religion of Humanity as an unwarranted extension of the philosophy. But, as an historian, I am fascinated by the periods and locations in which positivism has had a substantial following (including the Belgium of George Sarton before the first world war), as by the same token I am intrigued by the decline of interest in the positivist tradition since the 1920s.

M.K. I share your interest in Comte's philosophical-religious system and the positivist tradition, including the Belgian-American case of George Sarton. In this context it is worth recalling that Sarton since at least 1918 to his death in 1956 – that is long before Charles Percy Snow – was a propagator of the *New Humanism* linking the so-called two cultures of Snow (1956; 1959). This linkage played a crucial role in developing at Harvard University both the history of science as an academic branch of knowledge by Sarton,⁸ and the Programme for General Education in Science by James Bryant Conant's team.⁹ It was in this intellectual milieu – enriched, among others, also by the reception of Arthur Oncken Lovejoy's history of ideas, and of Ludwik Fleck's understanding of the “genesis of empirical facts” – that Thomas Samuel Kuhn was born as a historian and philosopher of science.¹⁰

Let us now go to the issues that belong to the methodology of the history of science.

7. Trends, currents, approaches and styles in the history of science in Western culture

MK: Let me give a map of different trends, currents, approaches and styles in the history of science in Western culture.

- 1) *historia vitae magistra / judging or monumental, or anniversary, or scientists' historiography of science;*

⁸ Cf. Cohen 1957; 1963; 1984.

⁹ Conant, Roller (eds.) 1948 (and subsequent editions in 1950, 1952, 1953, 1954 and 1957); Cohen, Watson (eds.) 1952.

¹⁰ Cf. the doctoral thesis: Kokowski [2001a](#); [2001b](#) (English summary).

- 2) *positivistic / objectivistic historiography of science, “internal” history of science* (including Crombie’s *comparative historical anthropology of science*);
- 3) *Marxist historiography of science* (influenced by *The Social and Economic Roots of Newton’s Principia* (1931) by a soviet historian of science Boris Hessen);
- 4) *critical or new historiography of science (and technology)*:
 - a. *intellectual history, history of ideas*;
 - b. *historicized philosophy of science*;
 - c. *Anglophone social studies of science or sociology of scientific knowledge, based on the Strong Programme and the Empirical Programme of Relativism and developed in opposition to the so-called Weak Programme of Sociology of Science of Robert K. Merton, which left the cognitive content of science out of sociological account*;
 - d. *ethnographic studies of “laboratory life”, ethnomethodology of science*;
 - e. *social construction of technology*;
 - f. *French tradition of epistemology*;
 - g. *historical epistemology*;
 - h. *social and cultural context of science, “science in context”, “cultural history of science”*: “exploring the ways in which major developments in the sciences emerge from intensely local and contingent circumstances” (M. N. Wise 2017) or “which focus on the cultural significance and development of science within its particular, local context” (Shapiro 1998);
 - i. *a material turn in the historiography of science or experimental history of science*;
 - j. *a spatial turn in the historiography of science or historical geography of science*,¹¹
 - k. *rhetorical historiography of science*;
 - l. *feminist science studies*;
 - m. *comparative historiography of science*;
 - n. *anti-history of science, and ANTI-history of science and technology*
 - o. *microhistory of science and technology*.
- 5) *critique of anti-whiggish historiography of science*;
- 6) *scientometric, algorithmic historiography*;

¹¹ I am grateful to Jan Surman (from Herder Institute, Marburg, Germany) for drawing my attention to this term.

- 7) *the postmodernist historiography of science or the academic left historiography of science* (propagated by postmodernist philosophers, but severely criticised by the so-called *Friends of Science* in the 1990s quarrel of the so-called *Science Wars*).

In the methodological approach developed in my doctoral thesis, all such trends may be considered from one point of view, i.e. “a research hermeneutics”, that is a hermeneutics used by the researcher or all the interpretative tools used by the researcher at the stage of his repeated attempts to comprehend the subject under study.¹²

In this context, could you put your own approach / style on this map of trends, currents, approaches, and styles mentioned above? In other words, could you describe what research hermeneutics you applied during your career? And, how, against the background of the trends, currents, approaches, and styles mentioned above, you see your research hermeneutics? Could you also show the dynamics of the development of your views at this point of your career?

And, did “The Styles of Scientific Thinking in the European Tradition” vol. I–III, by your teacher, Alistair Cameron Crombie (who worked on them at least since 1976 and which were published in 1994) influence your own style? I mean particularly the chapter “Historiography of science” with subchapters “Science in Intellectual Culture” and “Intellectual and Moral Commitments of Science; Levels of Historical Investigation; the Variety of Scientific Methods”¹³, and the idea of comparative historical anthropology of science.

RF: All the approaches you mention are capable of resulting in good work, and most of them at various times and with regard to particular problems have done so. But I come back to my contention that, as working historians, our task is to establish as broad an historiographical menu as we can muster, and then to select from the menu an approach suited to the question we are trying to answer. Hence my preference for methodological openness and flexibility, and my suspicion of modishness, a point that I tried to make some years ago in an article on the emergence of the history of science in the European intellectual tradition in *Minerva*¹⁴. So, yes, sensitivity to our historiographical options

¹² Cf. Kokowski [2001a](#), pp. 14–21; [2001b](#) pp. 316–317 (English summary).

¹³ I made a review of this monograph – cf. Kokowski 1995.

¹⁴ Cf. Fox 1973.

is indispensable to our task; it is the essential scaffolding of historical inquiry, but expendable once the structure is complete, and never an end in itself.

8. Historiographical genres

MK: *Well-known historiographical genres include, among others: a) chronicles and annals, b) narrative histories, c) dynastic histories, d) cultural and literary history, e) historical encyclopedias, f) world histories, g) local histories, h) biography, i) prosopography or collective biography, and j) doxography composed of placita (writings about views); peri haereseon (writings about schools of thought) and the so-called diadochai (successions – writings in the line of succession of the masters and students).*¹⁵

What kinds of historiographical genres did you apply in your own works?

RF: Pursuing the last question you raised with me, I have tried to remain eclectic, though (I hope) never uncritical, in my choice of approaches. When I wrote about Mulhouse in *The British Journal for the History of Science* (1984),¹⁶ I focused closely on science and industrial technology in one rather small town at the eastern extremity of France. My work on Sadi Carnot (1978), on the other hand, was primarily textual, though set in the context of French industry after the collapse of First Empire. And most recently, in *Science without Frontiers* (2016a), I have taken a transnational perspective. Currently, I am working, in a very different register, on a biography of Thomas Garnett,¹⁷ a late-eighteenth-century chemist and physician whose career and work I am trying to situate in the geographical regions he traversed (northern England, Glasgow, and London) as well as in science of the time and the wider context of the emergence of a distinctive “Romantic” approach to the study of nature. In all these examples, the result is a mixture of narrative and analysis, though narrative and analysis informed by the specific questions I have chosen to ask of my sources.

¹⁵ Cf. Meisami, Starkey 1998, p. 289; Stone 1971; Diels 1879.

¹⁶ Cf. Fox 1984.

¹⁷ Cf. *Wikipedia* 2017b.

9. Historiographical narratives

MK: As methodologists of history know, there are many problems with historiographical narratives. Let it suffice to mention the difference between a literary narrative and a historical one, the issue of constructivism and narrativism of a historical narrative, the issue of general terms and periodisations, the narrative substances (such as “revolution”, “evolution” or “progress”) in a historical narrative, the myth of “hard historical facts” or “pure facts” (free of any theoretical or philosophical interpretation or generalisation) of a historical narrative; the truth of so-called “historical sentences” and historical narrative.¹⁸

Did you in your work on the history of science and technology notice such problems, devote your attention to them, and commented on them in an open way? And much more particularly, did you use historiographical narrative from the ‘actors’-eye view? What do you think about Whiggish and anti-Whiggish interpretations in history of science (cf. Harrison 1987)?

RF: One principle that has always weighed with me, especially in the more textually focused work I have done, lies in the importance of what Quentin Skinner and other intellectual historians have discussed as the “recovery of intention”.¹⁹ That implies something like trying to get into the mind of the writer or speaker, in order to identify the purpose and meaning of what he or she wrote or said. Although such a position entails a suspicion of the snares of Whiggish interpretations, the challenge of avoiding “presentist” tendencies in our choice of what historical episodes to study remains formidable. Most of us make a deliberate effort to avoid privileging success stories; to some extent, that is what I tried to do in my first book (on a “wrong” theory), *The Caloric Theory of Gases* (1971). But we cannot study everything, and there will always be a tendency to favour areas of science that have yielded “correct”, or at least fruitful, results or theories.

¹⁸ Cf., for example, Topolski 1968; 1978; 1983; 1998; White [1984](#); McCullagh [1987](#); Kokowski [2007](#).

¹⁹ Cf. Skinner [1969](#), p. 49; Boucher [1985](#), pp. 204–206.

10. Science Wars

MK: *Did the so-called Science Wars known from the USA and France of the 1990s²⁰ also take place in the United Kingdom? And, if they did not, could you explain why? Are your works related to the topic of the so-called Science Wars?*

RF: Yes, the Science Wars debate had its echoes in Britain, although France and the USA always struck me as the epicentres of the exchanges. I think that Alan Sokal and his collaborators put their finger on an unhealthy trend in certain traditions of writing about the history and sociology of science: their accusations of empty jargon and an inadequate command of the content and practices of science were not wholly unfounded. In saying that, I am mindful of the reactions of several scientists of my acquaintance, who do not recognize their own procedures in the historical and sociological analyses of their work. It would not be for me to judge how far such perceptions are justified. But what most concerned me in these reactions and the whole Science Wars episode was the break-down of any real meeting of minds. The sociologically informed analyses may have had their failings. But the blanket rejection of the “social science” approach by critics, within and beyond the scientific community, was at best unhelpful. The need for dialogue between the “cultures” (in C. P. Snow’s sense) was and is what the debate exposed.

11. A university teacher of history of science

MK: At least for sixty years the history of science has been a legitimate research and university discipline in Great Britain and the United States. In marked contrast, in Poland one cannot study the history of science at any university. Throughout your career you have been teaching the history of science: first at the University of Lancaster, where from 1966 to 1988 you climbed the career ladder from Lecturer, then Senior Lecturer, then Reader, to Professor of the History of Science. In 1988 you were Visiting Professor of the History and Public Understanding of Science at Imperial College of Science and Technology, University

²⁰ Cf. Gross, Levitt [1994](#); Harwit 1996; Linenthal, Engelhardt (eds.) 1996; Sokal [1996](#); [2017](#); Sokal, Bricmont 1997/1998; Carrier et al. (eds.) [2004](#); *Wikipedia* [2017c](#).

of London, and then – between 1988 and 2006 – Professor of the History of Science at the University of Oxford. Then, after retiring from that position, you became Visiting Professor at Johns Hopkins University (2007), East Carolina University (2009), and the Czech National Technical University (2013) and Horning Visiting Scholar at Oregon State University (2013).

In this context, let me ask a very simple-sounding question: does the university really need courses on the history of science (on an undergraduate and/or graduate level)?

RF: I think I *would* talk in terms of need. Science and technology have been such a major presence in the fashioning of the modern world that any university programme in history, or in many other areas of the humanities, that does not take account of this would appear seriously incomplete. In the sciences and engineering too, I would urge the desirability of students having some exposure to the history, philosophy, and sociology of science. Without that exposure, it is hard to convey a sense of the nature of the scientific enterprise and the complex processes that at different times and in different circumstances promote or undermine consensus. Courses that treat the historical, philosophical, and sociological aspects of science can also help to counter the separation of the culture of science from other realms of culture and so to respond to the anxieties voiced by Snow. At a practical level, we should also bear in mind that many science students do not go on to be scientists. For them, exercises in writing and speaking about broader science-related issues have real career value, in addition to the importance they have for them, as for all of us, as citizens.

MK: *How did you teach the history of science? Have you ever referred to the new humanism of George Sarton, the two cultures of Snow and the third culture of Snow, to Lovejoy's history of ideas, or Harvard Course of General Education of Conant's group (cf. Cohen, Watson (eds.) 1952)?²¹ Did you discuss in your lec-*

²¹ These ideas are very close to me, I encountered them working on my doctoral thesis (cf. Kokowski [2001a](#); [2001b](#); among others, having regard to the historical facts, I introduced the terms: *the two cultures of Sarton-Snow* and *the third culture of Sarton-Snow*), and, recently, I have formulated *a model of a university of new humanism* in which the science of science and the new humanism of George Sarton are the keystone of the whole university (cf. Kokowski [2015b](#)).

tures also issues, methods or approaches with a range of the other disciplines included in the science of science, that is philosophy of science, sociology of scientific knowledge, policy of science, etc.?

RF: In teaching, I have stayed close to the principles that have guided my own research and writing. I have tried to introduce students to the different genres of the history of science and technology, without prescribing a right or wrong path to follow. So, yes, I always began my Master's teaching with a session on George Sarton before moving on to examples of writing by Koyré and Lovejoy in the history of ideas tradition. In talking about these historians, I thought it important for their work, like the science they wrote about, to be set in a context of time, place, and social location. Talking about Sarton, for example, led on to a discussion of the resurgence of positivist thinking in continental Europe at the end of the nineteenth century and the devastating consequences of the Great War, in response to which Sarton articulated his dream of a "new humanism" that would transcend the boundaries of nation and culture. Context was similarly crucial for an understanding of the significance of the series of *Harvard Case Histories in Experimental Science*²², a product of James Bryant Conant's General Education in Science programme for enlightening American non-scientists in the principles of science after the Second World War. The fact that these *Histories* were written for such a specific purpose made them products of their time, and their influence on more recent historiographical debates has inevitably diminished. That cannot be said, however, of one of Conant's authors, Thomas Kuhn, whose *Structure of Scientific Revolutions* had roots in his work for the Conant's Programme in General Education in Science. Well over half a century after its publication in 1962, *Structure* is still recognized as marking one of the greatest historiographical turning points in our field, and I always found it a wonderful text to teach from. In addition to its dazzling chronological span, it offered a perfect springboard for considering the subsequent, even more sociologically informed approaches of the kind pioneered by Michel Foucault, Steven Shapin, Simon Schaffer, and Bruno Latour, among others.

²² Cf. Conant, Roller (eds.) 1948 (and subsequent editions in 1950, 1952, 1953, 1954 and 1957).

Teaching the history of technology, as I did in the Oxford Master's programme, also allowed for critical reflexion on the social construction of technology and on broader conceptions of the history of technology exemplified in the idea of networks, as developed quite independently by Thomas Hughes in the USA²³ and François Caron in France.²⁴ In all this, and whatever the methodologies under review, I always insisted that programmatic statements and historiographical reflexion should not be seen as ends in themselves. The acid test of any approach must be how well it works in the hands a master of the historian's craft. We must not forget that the pioneer of the *Annales* school, Marc Bloch, was the author not only of *Apologie pour l'histoire ou Métier d'historien* (published posthumously in 1949) but also (in the history of technology) of a classic study of the medieval watermill.²⁵ Important though Bloch's methodological writings were, his scholarly publications were what made him the great historian he was.

MK: When we look at the organizational development of the history of science in the last century in English-speaking countries, we see that what was founded at the beginning were the departments (or units, centres, institutes) of the History of Science and Methodology,²⁶ and then they developed into units of the History of Science and Technology, followed by the History and Philosophy of Science (and Technology), then the History and Sociology of Science (and Technology), then the History and Cultural Studies of Science, then – the Science and Technology Studies, and finally, became units of the Science, Technology and Society.

What do you see as the ideal provision for a unit, centre, department, institute of the history of science at the university? Should such a unit be independent of the unit of the philosophy of science or sociology of scientific knowledge? Which faculty should it

²³ Cf. Hughes 1983; Bijker, Hughes, Pinch (eds.) 1989.

²⁴ François Caron deployed his model of networks (*réseaux*) to particularly good effect in his *Histoire de l'exploitation d'un grand réseau: la Compagnie du chemin de fer du Nord, 1846–1937* (1973) and much of his subsequent work, notably the collective volume, *Histoire générale de l'électricité en France*. Tome I 1881–1919, of which he was joint editor (1991).

²⁵ Cf. Bloch 1949; 1985.

²⁶ The first such unit in England was the Department of the History and Method of Science, University College London founded in 1921.

be affiliated with, or should it rather be an inter-faculty unit? Do you think it makes sense to create units focused on purely scientific research of the history of science?

RF: My own experience has been unusual, at least in the British context, in that I spent all my career (apart from brief periods at the [Cit  des Sciences et de l’Industrie in Paris](#) and the [Science Museum in London](#) between 1986 and 1988) in either a Department ([at Lancaster](#)) or a Faculty ([at Oxford](#)) of History. Since I always regarded the history of science as first and foremost a branch of history, indeed a central element in human history, that proximity to other practicing historians was one I welcomed. For me, it had both a logic (since we are, after all, historians) and the practical advantages of easy contact with the leading trends and debates in the historical profession. In the USA (though less in the UK), History Departments are now increasingly open to the appointment of historians of science. I see this as a welcome development, although it does bear the risk of the neglect of the most scientifically demanding aspects of science and technology, especially those of our own twenty-first century. Other institutional settings can be made to work, of course, and (as with historiography) I think we should avoid a prescriptive, “one fits all” view. As historians, we have no choice but to adapt to the circumstances we face in our various institutions. If a separate institute or an inter-faculty unit seems the best way forward, that is the way to go. Once again, flexibility and sensitivity to local opportunities are the key.

12. Museums, the history of science and science communication

MK: You also worked in science museums: in 1988, as an assistant director and Head of the Research and Information Services Division, [Science Museum, London, Great Britain](#), and from 1989 to 2003, as a member of the Wissenschaftsbeirat and Kuratorium of [the Deutsches Museum, Munich, Germany](#).

What should be the role, in your opinion, of science museums? And in this context what do you think about the dialectic – between the history of science and technology on the one hand and science communication and public understanding of science on the other – that currently prevails in these museums?²⁷

²⁷ Cf. Durant (ed.) 1992; Boon [2010](#), p. 111; Fox [2008](#), pp. 174–175.

RF: I feel strongly that the museums in and for which I have worked have an important mission in remedying the grievous lack of public understanding of, or even interest in, science and its place in society. The aim should certainly not be to “fly the flag” for science; it is far more important to promote an informed critical attitude, by which on the other hand, and emphatically, I do not mean a spirit of hostility. Museums with this mission can be a place where scientists, engineers, and commentators on science and technology can engage with the non-scientific general public.

Incidentally, when I speak here of museums, I have in mind those that maintain and exploit a collection. A proper element of interpretation is laudable, of course; collections should not exist solely as uninterpreted source materials for the specialist and the scholar. But there is ample evidence that, when attractively displayed, objects can “speak”, and do so with a special voice and in a variety of ways. Many of them can engage by their beauty, in the manner of the Medici collection in [the Museo Galileo in Florence](#) or the George III collection at [the Science Museum, London](#). But they can do more than appeal to our aesthetic sense. Well presented, they can also tell us much about the society in which they were made and used: about the patrons who commissioned them, the visitors and lecture-audiences who learned from them, and the remarkable craftsmen who made them. Inevitably, such displays lend themselves to a certain degree of hero-worship, simply because the instruments that survive tend to be associated with the “success stories” of science. The same is true of the sites of scientific and technological achievement that have become an increasingly important adjunct to traditional museum displays in recent years: it is hard to imagine that Justus von Liebig’s laboratory in Giessen,²⁸ for example, would have survived had it not been recognized as a major precursor of our modern chemical research laboratory.

So there are limits to the understanding that museum collections and sites can convey. Survivals from scientific failures or false starts are not numerous. Much of the delicate apparatus with which Louis Pasteur disproved the ideas of spontaneous generation in the 1860s is available for us to see in his apartment in [the Institut Pasteur in Paris](#) (another great

²⁸ Cf. [“Justus von Liebig in his Laboratory at the Chemical Institute of the University of Giessen”](#) (c. 1840) – the colored wood engraving by Wilhelm Trautschold (1815–1877), available at *The German History in Documents and Images website*.

site of scientific achievement incidentally); by contrast, disappointingly little survives from the experiments of his unsuccessful and far less well known adversary in that debate, Félix Pouchet.²⁹

Modern “black box” technology highlights another limit to the value of museum displays. Placing a computer or iPhone in a showcase will do little to convey the principles, still less the historical significance of the object displayed. Here, the instrument cannot even begin to “speak” on its own, and there is no alternative to simulation and explanation. But there remain many areas of science and technology in which a “real” object will have a greater impact than any simulation could possibly achieve. My contention, therefore, is simply that objects have a special power and should be used wherever possible. In saying that, incidentally, I am quite deliberately entering an implicit plea for museums to cherish their collections and avoid the all too seductive solution to reducing them or committing them to burial in an inaccessible off-site store.

13. The public understanding of science (PUS)

MK: *During your academic career you also dealt with the public understanding of science, being a British specialty established in the 1980s. Could you sketch this current of knowledge and explain why it is so important nowadays?*

RF: My first encounters with the public understanding of science movement date from the 1980s, during my time at [the Cité des Sciences et de l’Industrie in Paris](#) and then at [the Science Museum in London](#), where my post as assistant director was twinned with a visiting professorship in the history and public understanding of science at [Imperial College](#). At the time, the PUS movement was in its infancy. It was only recently, in 1985, that an influential report by the Royal Society had highlighted the dangers of an ever-widening gulf between the seemingly remote world of science and technology and the everyday lives of citizens whose limited command of science risked engendering suspicion, even hostility towards the scientific enterprise.³⁰ Responses to that report

²⁹ Cf. [Wikipedia 2017f](#).

³⁰ On the impact of the 1985 report, see the first-hand account by Walter Bodmer (2010), the first chairman of the Royal Society’s Committee on the Public Understanding of Science.

were vigorous and immediate. One of the most important of them, within the Royal Society itself, was the inauguration of the Michael Faraday Lecture and Medal in 1986.³¹ This annual award, which recognizes excellence in communicating science to UK audiences, has done much to make the work of scientists and engineers better-known among the general public. Some early PUS initiatives, however, were seen to have weaknesses. One was a tendency for PUS to be conceived as an essentially one-way exercise in which suitably digested expert knowledge passed from the scientific community to the lay audience, with little possibility of dialogue. In response, through the 1990s, the tone of PUS changed. The launch of the journal *Public Understanding of Science* under the editorship of my successor at the Science Museum, John Durant, encouraged a more reflective approach. And other contributors to the movement, including the Wellcome Trust, the British Association for the Advancement of Science, the Royal Institution, and in due course my own university, where a chair of the public understanding of science was created in 1995, have reinforced the new focus, placing the emphasis firmly on engagement and a belief that the scientific community can and should learn from public opinion as well as informing it. Among numerous initiatives to this end over the last twenty years, the Wellcome Trust's continuing programme of public engagement has been exemplary in both purpose and execution. Now, as science and technology impinge ever more closely on our lives, the need for the kind of dialogue that the programme has sought to promote remains as great as it was when the Trust launched its public engagement initiative in the 1990s.³²

14. Work as the editor

MK: You have an extensive experience as an editor of the *British Journal for the History of Science* (1971–1977), and the *Notes and Records of the Royal Society Journal of the History of Science* (2008–2014), the editor or a co-editor of 8 monographs, and a member of the editorial boards of *History and technology*, *Archives internationales d'histoire des sciences*, *Revue de*

³¹ Cf. [Royal Society Michael Faraday Award](#).

³² For further details on PUS, see: The Royal Society [1985](#); the journal *Public Understanding of Science* (established 1992); Durant (ed.) 1992; Bauer, Allum, Miller 2007; Fox [2008](#); Bauer [2009](#).

synthèse, *Revue d'histoire des sciences*, *La revue pour l'histoire du CNRS*, *Annals of science*, *Physis*, *Sciences et techniques en perspective* and a member of the Scientific Council of the [Prace Komisji Historii Nauki PAU / the *Studia Historiae Scientiarum*](#).

What would be your practical advice for young editors and young publishers? What do you think a new journal would have to do to win the respect of serious researchers and readers?

RF: Very properly, journals have different functions and styles. *The British Journal for the History of Science*, which I edited in the 1970s, was intended primarily for specialist historians of science, and it retains that orientation today. *Notes and Records*, as I conceived it, had a similar mission, though with a slight twist. While articles certainly had to meet the highest scholarly standards that we could achieve, my aim as editor was to publish articles of interest beyond the community of professional historians. One constituency (among others) of which I was keenly aware was that of the Fellows of Royal Society, many of whom read and generally liked *Notes and Records*. Reaching out to scientists is something that historians have not always been very good at doing. As the history of science has become “professionalized” over the last half century, we have risked losing sight of the world beyond our disciplinary borders. Of course, that does not mean a journal should “dumb down” what it offers. But what it publishes should be written in an accessible and (crucially) jargon-free manner.

15. Favourite works

MK: *Considering the label “Robert Fox’s favourites” applied by the Royal Society Publishing,³³ could you mention your most favorite works in history of science and explain why they are so important for you? Does the article “Professors Robison and Playfair, and the Theophobia Gallica: natural philosophy, religion and politics in Edinburgh, 1789–1815” (1971) by Jack B. Morrell belong to this group?*

RF: Your question turns my thoughts particularly to books that have “accompanied” me for long parts of my career, and I shall limit myself to those. Among them, Thomas Kuhn’s *Structure of scientific revolutions*

³³ Cf. Fox 2011.

(1962) has a special place. *Structure* appeared in the year when my thoughts first turned to the history of science, and it has been a landmark in our field ever since, despite a long history of criticism and re-thinking, not least by Kuhn himself. With respect to my more specific interests, Thomas Hughes's *Networks of Power* (1983) became a special favourite when the focus of my work was turning towards the history of technology. For me, *Networks of Power* epitomized a meticulously documented work that carried a strong thesis and set technical detail in a social, economic, and political context of great richness. With respect to my long-standing engagement with the history of science in France, I could mention virtually any of the books by Charles Gillispie, who was first a mentor, then the colleague with whom I had my closest intellectual bond over many years: his *Science and polity in France: the end of the Old Regime* (1980) and *Science and polity in France: the revolutionary and Napoleonic years* (2004) are exemplary in their rigour, elegance, and sensitivity to French culture, of which he was such an admirer. In the history of physics, I am spoilt for choice. In a field notable for authors who have resolutely (and in my view admirably) kept the content of a pretty difficult area of science at the centre of their concerns, I would mention an unusual book by one of those authors: Russell McCormmach's *Night thoughts of a classical physicist* (1982), an exploration of the mind of an imaginary German physicist reflecting, in 1918, on his half-century of work in a classical physics now threatened by the rising tide of "new" physics.

In a very different genre, though for the similar reasons, I have always admired the work of Martin Rudwick, despite their remoteness from my own special interests. His books – *The great Devonian controversy: the shaping of scientific knowledge among gentlemanly specialists* (1985), *Bursting the limits of time: the reconstruction of geohistory in the age of revolution* (2005), and *Worlds before Adam: the reconstruction of geohistory in the Age of Reform* (2008) – convey the importance of complete mastery of the science, in his case geology, combined with an extreme sensitivity to the wider realms of society and culture.³⁴

Among articles, it happens that in recent years I have been asked to select favourite contributions to the two journals I have edited: *The*

³⁴ Cf. Rudwick 1985; 2005; 2008.

British Journal for the History of Science (1971–77) and *Notes and Records. The Royal Society Journal of the History of Science* (2008–14). In the case of *BJHS*, I chose Steven Shapin’s “[Property, patronage, and the politics of science: the founding of the Royal Society of Edinburgh](#)” (1974).³⁵ From articles that appeared in *Notes and Records*, though in this case before my time as editor, I opted for Jack Morrell’s “[Professors Robison and Playfair, and the *Theophobia Gallica*: natural philosophy, religion and politics in Edinburgh, 1789–1815](#)” (1971).³⁶ Both, as it happens, were studies of Edinburgh science at the turn of the nineteenth century. But what really united them in my eyes was with their keen sensitivity to the complex political and religious interactions in the late Scottish Enlightenment; in fashioning that perspective, they were ground-breaking studies. Moreover, and this is something I appreciate in all scholarly writing, they were beautifully written.

16. Organizational activity in scientific societies

MK: Let me quote excerpts of your CV:

- [British Society for the History of Science](#): Member of Council 1970–1978 & 1980–1983, Vice-President 1977–1978 & 1982–1983, President 1980–1982;
- International Congresses of History of Science: Leader of UK delegation to XVIIth, XVIIIth, XIXth, and XXth International Congresses (Berkeley 1985, Hamburg/Munich 1989, Saragossa 1993, and Liège 1997);³⁷
- Member and at times the Vice-Chairman too of the Management Committee of the Centre de Synthèse, Paris³⁸ (1986–2014);

³⁵ Cf. *British Journal for the History of Science* [2017b](#), “Robert Fox (1971–1976)”, p. 174.

³⁶ Cf. *Fox* [2011](#).

³⁷ Cf. International Union of the History and Philosophy of Science. Division of History of Science 1985; 1989; 1993; 1997.

³⁸ This institution was founded in 1925 by Henri Berr, but was preceded by the journal *Revue de Synthèse Historique* (founded in 1900 and edited by Henri Berr). Regarding the journal (until 1930), interactions between Berr and the Annales School, and also the exciting history of the Centre de Synthèse (unfortunately sketched only until 1960), cf. Berr [1930](#); Febvre [1930](#); Cole [2005](#); Burguière [2009](#); Chimisso [2016](#).

- [International Union of the History and Philosophy of Science](#): First Vice-President of Division of History of Science 1989–1993, President of [Division of History of Science](#) 1993–1997, President of IUHPS 1995–1997;
- Member of the Advisory Committee of the Association pour l'histoire de l'électricité en France, 2001–2014;
- [European Society for the History of Science](#): Founding President 2003–2006, Vice President 2006–2009.

Your organizational activity in scientific societies both in Great Britain, and abroad is thus truly remarkable; you have an extensive expertise in this domain.

In this context, how would you rate the development of the history of science and technology in the UK and in France on the one hand and in the rest world on the other hand?

And what would be your advice for a society in the history of science and technology to promote this discipline in current culture better, both in academia (universities, scholar societies, research institutes) and beyond?

RF: What strikes me most forcibly, whether in the USA, Britain, or France, is the still growing interest of the history of science and technology as a field of research. The annual meetings of the History of Science Society in the USA and the British Society for the History of Science are huge affairs, and I was recently at the immensely ambitious biennial meeting of the [Société française d'histoire des sciences et des techniques](#) in Strasbourg. The only conclusion we can draw from these national meetings, as also from gatherings of the European Society for the History of Science (last year in Prague, for example, following earlier congresses, including the remarkable one organized in Kraków), is that scholarly interest in the history of science and technology is at an all-time high. What worries me, however, is mounting evidence of the difficulty that champions of HST have in securing the survival of posts in the subject. Somehow, we need to persuade educational decision-makers that what we do, in our teaching and research, is capable not only of advancing the “academic” understanding of a still understudied aspect of human history but also of engaging a general educated public beyond our immediate speciality. We must redouble our efforts

to address audiences on as many fronts as possible and somehow strike a balance between our scholarly interests and “outreach”. The success of recent semi-popular works on the history of science and technology (often written by those who are not professional historians of science) points to possible opportunities. Dava Sobel (with *Longitude*, 1995) and Richard Holmes (with *The Age of Wonder*, 2008) are just two authors whose high profile demonstrates the existence of a substantial audience for well-written history of science.³⁹

17. The organizer or co-organizer of many scientific conferences

MK: *Which conferences were particularly important to you and could you explain why? Do you count into this group the conference “Technological Change”, which took place between 8 and 11 September 1993 in Oxford and was conceived as a sequel to the conference “The structure of the scientific change” organized by your teacher A.C. Crombie and Rom Harré in Oxford in 1961? Why was it an important conference?*

RF: The conference of 1993 was planned as a forum, on the international stage, for the methodological debates and new problems that at the time were doing so much to transform the field of the history of technology. In this respect, the conference and the collective volume, *Technological Change*, that followed were a natural sequel to the *Scientific Change* volume of 1963. As had already happened in the history of science, traditional disciplinary boundaries and entrenched habits of reading and citation in the history of technology had been subjected to mounting challenges through the 1980s. The trend was one that Anna Guagnini and I followed in our own writing at the time, begun in a collaboration going back to 1983 and continued in preparation for the lectures we gave at the Summer School in the History of Science at the University of Uppsala in 1990. It was a fundamentally reworked version of this series of lectures that eventually appeared as *Laboratories, Workshops, and Sites: Concepts and Practices of Research in Industrial Europe, 1800–1914* in 1999. In this book, Guagnini and I combined two

³⁹ Cf. Sobel 1996 & *Wikipedia* [2017d](#); Holmes 2008 & *Wikipedia* [2017e](#).

guiding principles that, in different forms, were beginning to attract attention among historians of technology at the time. One was that the history of applied research was best cast in systematically interdisciplinary terms; this entailed bringing together, as we did, perspectives drawn not only from the history of science and technology but also from economic, business, and social and political history. The other principle was that the analysis should have a strong comparative dimension. In *Laboratories, Workshops, and Sites*, we drew particularly on evidence from Britain, Germany, France, Italy, and Belgium. In that way, we identified what we saw as distinctively European patterns that had little in common with what had been commonly (though not always convincingly) presented as the model of the successful American company with its well-endowed in-house research laboratory and structures for exploiting sophisticated science-based innovation. Across Europe, we saw evidence of very different notions of research, often aimed at adaptation and tasks of improvement that had little to do with fundamental innovations of the kind that became a hall-mark of the success of, for example, General Electric in the USA after the Great War.

MK: And what is your opinion, from the perspective of time, about the 2nd International Conference of the European Society for the History of Science entitled *The Global and the Local: The History of Science and the Cultural Integration of Europe*, which we organized together with our colleagues on September 6–9, 2006 in Kraków, Poland?⁴⁰

I mean, in particular, two problems: on the one hand, that fact that *the topic of this conference* can be easily attributed to *critical or new historiography of science*, especially *historical geography of science*, *comparative historiography of science*, *cultural history of science*, and *social studies of science*. And on the other hand, the *crucial idea of the conference*, i.e. *a critical dialogue*: a) between Europe and other continents, b) between different parts of Europe, c) between different countries both the big and the small; and finally, d) between the circles of diverse languages both national tongues and characterizing different disciplines.

Do you think that these problems are strangely still up to date and relevant in our current, turbulent times?

⁴⁰ Cf. Kokowski (ed.) [2006–2008](#); [2008](#).

RF: As a committed European, I found it a privilege to be involved with the European Society for the History of Science from the earliest informal discussions in 2003 and then to become the society's first president in the following year. In the fifteen years or so since the preliminary talks, with Claude Debru and others, that led to the founding of the society, the ESHS has acquired stability and a rhythm of major biennial conferences, interspersed with more finely focused meetings, usually on themes of special interest to the current president. The society seeks explicitly to acknowledge and encourage work being done across Europe and make research in our continent more widely known. The conference organized in Kraków in 2006 was exemplary in this respect. The act of meeting in Poland reinforced the transformed vision of Europe, following the separation between East and West that had blighted intellectual exchanges within our continent for all too long. Moreover, the chosen theme of "The global and the local" epitomized the resolve of all of us in the ESHS to transcend frontiers not only between European nations but also between Europe and other continents of the world. Sadly, the resurgent nationalisms of recent years present a potential threat to that ideal. But I believe that the ESHS stands as an affirmation of the importance of a cultural "European project" with a place for all the many linguistic and cultural traditions that enriched the gathering in Kraków, as they have enriched the ESHS's other meetings.

18. Bibliometry, scientometrics and history of science, technology and medicine

MK: As one of a group of 74 editors representing 56 of the world's leading journals in the history of science, technology and medicine, you were a signatory of the appeal entitled "Journals under Threat: A Joint Response from History of Science, Technology and Medicine Editors".

The appeal rejected the division of journals into "three leagues", which was adopted in the ERIH's methodology, and the subsequent suggestion that only the first-league journals could receive financial subsidies, and did not accept the results of such classification with regard to journals from the history and philosophy of science, technology and medicine.

The article claims among others that:

We live in an age of metrics. [...] Such exercises as ERIH can become self-fulfilling prophecies. If such measures as

ERIH are adopted as metrics by funding and other agencies, then many in our field will conclude that they have little choice other than to limit their publications to journals in the premier division. We will sustain fewer journals, much less diversity and impoverish our discipline (Cook *et al.* [2009](#), p. 1).

[...]

Great research may be published anywhere and in any language. Truly ground-breaking work may be more likely to appear from marginal, dissident or unexpected sources, rather than from a well-established and entrenched mainstream". [...] We, among others, have asked the compilers of the ERIH to remove our journals' titles from their lists (Cook *et al.* [2009](#), p. 2).

Could you explain, in this context, what you think about the usage of bibliometrics to evaluate science, especially the history of science? How are you finding the Web of Science, Scopus, or Google Scholar as a means to accomplishing this task? And what is your opinion about the Matthew effect in science (Merton [1968](#); [1988](#)) and the Matthew Matilda effect in science (Rossiter [1993](#))?

RF: The protest that I signed, as editor of *Notes and Records*, along with more than 70 other editors of journals in the history, philosophy, and sociology of science, technology, and medicine, was a response to the particularly insidious form of bibliometric evaluation launched as the European Research Index in the Humanities. Of the ERIH's pernicious mechanisms for evaluation, the most damaging was the division of the journals in our field into three categories. The implication that an article published in an A-graded journal was necessarily worth more than one in a C-journal betrayed profound ignorance of the nature of research in the humanities. It led to an undervaluing of work published in minority languages, meaning *de facto* in any language other than English, and in journals that, because of their linguistic or subject focus, had a limited circulation. Such myopic prejudices betray the foundational ideals of the free and respectful exchange of ideas and people to which, in humanistic disciplines such as ours, we must surely subscribe.

Following the protest, the UK's Arts and Humanities Research Council agreed to ignore the ERIH's evaluations. Yet grounds for anxiety

remain. There is all too much evidence that the evaluation of humanities research in my own country is still being coloured by intuitive perceptions of a hierarchy of journals, rather than a focus on the quality of individual articles. For me, the whole bibliometric approach is deeply flawed. Books and articles in the humanities simply do not lend themselves to quantitative evaluation. I understand, of course, that work in our field must be judged, especially where the allocation of scarce funds is concerned. But judgements that do not rest on careful reading and assessment by peers in the field can never carry conviction. While I recognize the attendant danger of a “Matthew effect” and the risk of giving undue credit to well-known authors, currently fashionable approaches, and works published by prestigious presses (a very real risk for historians as well as it is for scientists), I believe we cannot shirk our collective responsibility to make the best judgements of quality that we can, giving time to the task and, where necessary, laying aside our personal methodological preferences.

MK: I really enjoy your words because I observe in the world, including Poland, the true flood of absurd admiration for bibliometry and scientometrics. In this context, the number of citations is mistakenly treated as an objective measure of the quality of considerations. Fortunately, there are still many serious scholars who deny such nonsense.⁴¹ On the other hand – in these days of revolutionary computer-led change – I am an advocate of technical refinement of our publications (including precise bibliographic information and metadata), of the establishment of high quality of national and international electronic databases and repositories in the area of science studies (including history of science), and of development of international cooperation in this field of knowledge.

19. Fame, freedom, and advice of the Master

MK: You are a well-known and widely respected person in the international circle of historians of science, which is reflected in numerous awards and honours you have received. Two distinctions from 2015 and 2016, have a symbolic meaning in our branch of knowledge: the George

⁴¹ Cf. the review articles: Kokowski [2015b](#); [2015c](#); [2015d](#) (bibliography).

Sarton Medal and the Alexandre Koyré Medal are awarded, respectively, by the History of Science Society (HSS) and the International Academy of the History of Science. At the same time, your colleagues and friends know very well that your professional successes did not spoil your character – you still are a very kind, polite, joyful man with a very positive, open attitude towards all people.

Could you give us – especially young people and young researchers – any advice, from this perspective?

RF: I hope that what I have said will have conveyed my commitment to openness. That means openness of many kinds, including openness between generations. It is an immense privilege to see younger scholars tackling new problems and fashioning new approaches, often in contexts (strikingly in the UK in recent years, in departments of English) that I would once have not seen as natural settings for research in our field. One consequence has been a move away from the expectation, which prevailed when I began doctoral research some 65 years ago, that historians of science and technology would share a common core profile of experience and reading. Those working in the broad field of the history of science today are likely to have less of that common ground, methodologically and in their interests, than we did even as late as the 1970s; what was once a rather close-knit (and arguably somewhat separate) intellectual community has become more permeable. But I see that as a natural, and generally welcome, consequence of the expansion of our field.

20. Dreams of the scholar

MK: *As a scholar do you have any dreams? What do you think about the ideal of cosmopolitanism in science nowadays and the idea of diamond open access (i.e. without any expenses for authors and readers)?*

RF: Despite the impediments and pitfalls, I adhere strongly to the ideal of free exchange and openness in a Republic of Letters adapted to our twenty-first century. That ideal entails our taking full account of the diverse interests of large and small countries and major and minority languages, as well as the disparities in economic circumstance. It also obliges us to engage with the technological advances of the

“information age” as a way of promoting not only communication of every kind but also the accessibility of as much as possible of the sum of human knowledge. It follows that some measure of open access must be a shared goal, with “diamond open access” as the ultimate objective. That said, in my time as editor of *Notes and Records*, I recognized the difficulties that even an institution as committed to its wider international obligations as the Royal Society faced in attempting to move towards the higher degrees of access, while maintaining its income stream. But at least the vision is there. And that must surely encourage us to refresh our hopes of a new international order with the peaceful domain of culture, rather than economic or geopolitical interest, at its heart.

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