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FASHIONING THE DISCIPLINE: HISTORY OF SCIENCE IN THE EUROPEAN INTELLECTUAL TRADITION

ABSTRACT. This paper offers personal reflections on the fashioning of the history of science in Europe. It presents the history of science as a discipline emerging in the twentieth century from an intellectual and political context of great complexity, and concludes with a plea for tolerance and pluralism in historiographical methods and approaches.

INTRODUCTION

For the current generation of Oxford postgraduate students, the early 1960s, when I began my own doctoral work in Oxford, belong to the shadowy prehistory of our discipline. Students know that Thomas Kuhn's *Structure of Scientific Revolutions* was first published in 1962,¹ and they perceive the book's appearance as an important event. For most of them, *Structure* marked a glimmer of true enlightenment after a long dark age of positivistic chronicling or empty generalization that had passed for the history of science. Many of them, in fact, see the academic history of science of today as born with Kuhn. I can well understand why they do so. I found it exhilarating to take my own first steps as an historian at a time when Kuhn's ideas were entering the lists of methodological debate. And I recall the effort I made to assimilate the notion of paradigm shifts and the distinctions between normal and revolutionary science and, rather unconvincingly, to work some primitive Kuhnian notions into my thesis.

More than forty years on, I recount this in my teaching. I also recount the guidance I received from my supervisor, Alistair Crombie, who insisted that the history of science was first and foremost a history of scientific thought and practices and that the political, social, economic, and other contexts were relevant

¹ T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, and Toronto: University of Toronto Press, 1962). The second and third editions of the book (Chicago and London: University of Chicago Press, 1970 and 1996) include a 'Postscript', written in 1969.

only in so far as they helped the historian to fashion the kind of resolutely intellectual analysis that appeared many years later in the three volumes of his magisterial *Styles of Scientific Thinking*.² I remain convinced that work in the genre in which Crombie excelled, still has much to offer. Young historians whom I encounter impoverish their menu of methodological options if at some stage they do not engage with the finely focused study of texts and ideas such as characterized not only *Styles*, but also the much earlier *Robert Grosseteste and the Origins of Experimental Science, 1100–1700*, or that masterpiece of synthesis, *Augustine to Galileo*.³

However, when I set reading from Crombie's *oeuvre* today, the exercise is usually seen as an immersion in a lost world. Crombie's methodological references, in particular, are unfamiliar to all but the most adventurous students. Few, if any, will have read Paul Tanery, Pierre Duhem, or R.G. Collingwood, all central figures in Crombie's intellectual pantheon. Most have at least heard of Alexandre Koyré, a younger member of the pantheon, although only specialists in the early modern period have read Koyré seriously.

When I commend Crombie's writings to students, I find myself fighting against an all-too-exclusive commitment to what Dominique Pestre has called the 'new history of science', with its different canon of authorities committed to drawing not only upon philosophy, as Crombie did, but also upon approaches nurtured within sociology, anthropology, literary theory, psychology, and economics.⁴ I welcome this multiplication of the interfaces between the history of science and what (for want of an ideal term) I class as more 'social' or 'cultural' approaches to the study of the past. The bridge to interdisciplinary styles exemplified in the history of the book, the history of collecting and taste, and the history of conversation has greatly enriched our discipline: James Secord's *Victorian Sensation* is an illustration of the intricately layered interpretations that can be drawn from a highly focused study—in this

² A.C. Crombie, *Styles of Scientific Thinking in the European Tradition: The History of Argument and Explanation especially in the Mathematical and Biomedical Sciences and Arts*, 3 vols. (London: Duckworth, 1994).

³ A.C. Crombie, *Robert Grosseteste and the Origins of Experimental Science, 1100–1700* (Oxford: Clarendon Press, 1953) and *Augustine to Galileo: The History of Science, A.D. 400–1650* (London: Falcon Press, 1952).

⁴ D. Pestre, 'Pour une histoire sociale et culturelle des sciences: nouvelles définitions, nouveaux objets, nouvelles pratiques', *Annales: Histoire, Sciences sociales*, L^e année (3), (1995), 487–522. This issue of *Annales* also contains historiographical articles by Ilana Löwy, Antoine Picon, Yves Cohen, and Alessandro Mongili.

case, of the production, reading, and reception of Robert Chambers' *Vestiges of the Natural History of Creation*.⁵ What concerns me, is not the proliferation of such new approaches, but rather the fragmentation that afflicts our community if one or other set of approaches, 'new' or 'old', is deemed in some exclusive sense to be superior to another. The history of science is fragmented enough by language and national traditions; we should surely not aggravate this condition by intolerance when it comes to our choice of historiographical styles. The more options we have as historians, the better. For different questions call for different methodological tools, and we need as broad a repertoire as possible.

Of course, a broad church can all too easily degenerate into flabbiness. Intolerance, on the other hand, leaves us with a church fragmented, as occurred in the early summer of 1931, when the Second International Congress of History of Science met in London. That Congress remains a notable event in the history of our discipline, as the occasion on which an eight-man Soviet delegation arrived with papers that were to shock an audience (most of which was made up of scientists, with a small number of historians) hitherto unfamiliar with Marxist notions of dialectical materialism, substructure and superstructure, and so on.⁶ In fact, only four of the eleven papers

⁵ J.A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago and London: University of Chicago Press, 2000). See also Secord's introduction to his edition of R. Chambers, *Vestiges of the Natural History of Creation and Other Evolutionary Writings* (Chicago and London: University of Chicago Press, 1994), ix–xlvi, and his 'Knowledge in Transit', *Isis*, XCV (4), (2004), 654–672.

⁶ In an extensive literature on the Congress, see P.G. Werskey, 'Introduction: On the Reception of *Science at the Cross Roads* in England', in *Science at the Cross Roads: Papers presented to the International Congress of the History of Science and Technology held in London from June 29th to July 3rd, 1931 by the Delegates of the U.S.S.R.* (London: reprinted by Frank Cass, 1971), xi–xxix, and Werskey, *The Visible College* (London: Allen Lane, 1978), 138–149. This reprint of *Science at the Cross Roads*, in a series edited by Roy MacLeod, contains a facsimile of the original Soviet papers, published under the same title in 1931 under the imprint Kniga (England). Some of the papers, or extended extracts from them, were also published in *Archeion*, XIV (2), (1932), 271–288 and 497–534. More recent work on the Congress includes Anna-K. Mayer's 'Fatal Mutilations: Educationism and the British Background to the 1931 International Congress for the History of Science and Technology', *History of Science*, XL (4), (2002), 445–472, and her 'Setting up a Discipline, II: British History of Science and "the End of Ideology", 1931–1948', *Studies in History and Philosophy of Science*, XXXVA (1), (2004), 41–72 (esp. 48–53). Here, and in all my reflections on the Congress, I have drawn upon Christopher Chilvers' Oxford DPhil dissertation (forthcoming), "'Something Wicked this Way Comes": The 1931 Congress and the Russian Delegation".

were recognizably Marxist in style. Yet, the conflicts between the Soviets and the organizers of the Congress were brutal and public. In an atmosphere of open hostility, the president of the Congress, the British historian of medicine Charles Singer set the tone by telling the Soviet delegation (which, he said, had failed to confirm its plans by the due date) that the programme was full, and that their papers could not be accommodated. The eventual compromise—the papers were moved to a special session organized for the last day of the Congress—fell far short of the Soviet delegates' expectations. On the day in question, a Saturday, most of those attending were away on a sight-seeing trip in Oxford, and even in the special session only a few minutes were allocated for the presentation of each paper.⁷

The affair did not go unnoticed. The right-wing press applauded Singer's firm response, while the *Daily Worker* articulated the left-wing sense of outrage at what it saw as an assault on the principle of free expression.⁸ The Soviet delegates, for their part, took matters into their own hands through such limited interventions in the normal sessions as they were allowed to make, and (at Lancelot Hogben's suggestion) hastily arranged for the printing of their papers, which they then distributed as individual items on the Saturday and as the bound volume, *Science at the Cross Roads*, three days later.⁹

Of the papers printed, the best known is that by Boris Hessen, on 'The Social and Economic Roots of Newton's *Principia*'.¹⁰ Hessen's rooting of the *Principia* in the social and economic context of late 17th-century mercantile England has always attracted mixed reviews. But to contemporaries on the Left, the interpretations of Hessen, Nikolai Bukharin, and other members of the Soviet delegation came as a revelation of what the history of science could become. In 1934, Joseph Needham, a Christian and a socialist brought up on a classic left-wing diet of Bernard Shaw and

⁷ For the programme of the special session, see *Archeion*, XIV (4), (1932), 532.

⁸ 'Soviet Delegates pulled up', *Morning Post*, 6 July 1931, and 'Only a Few Minutes Each: How Soviet Scientists were treated at Congress', *Daily Worker*, 6 July 1931. Both articles are discussed in C.A.J. Chilvers, 'The Dilemmas of Seditious Men: The Crowther-Hessen Correspondence in the 1930s', *The British Journal for the History of Science*, XXXVI (4), (2003), 417–435 (at 426), and more fully in Chilvers, *op. cit.* note 6.

⁹ Werskey, *The Visible College*, *op. cit.* note 6, 140.

¹⁰ Boris Hessen, 'The Social and Economic Roots of Newton's *Principia*', in *Science at the Cross Roads*, *op. cit.* note 6, 147–212.

H.G. Wells, used a 'preliminary note' to his *History of Embryology* to acknowledge the explanatory potential of Marxist historiography.¹¹ At the same time, the crystallographer J.D. Bernal and the journalist J.G. Crowther were taking steps along the same road.¹² Yet it seemed that Singer's way of writing the history of science had won the day. Given the momentous events coursing through Russia, a ban on foreign travel meant that there was no Soviet presence at the International Congresses of History of Science that followed in 1934 and 1937. This made it easy for Singer and others who found the Marxist writings fatally imbued with 'ideology' to go on writing a history of science that was serenely unmarked by the departures of Hessen and his colleagues. It must have appeared that an early foray into Soviet-style contextualism had been seen off.

In seeking to understand why the Marxist historiography caused such a stir in 1931, we must go beyond Singer's conservative cantankerousness. One explanation lies in the sensitive climate through which Britain was passing: the General Strike of 1926 was still a vivid memory, and tensions arising from the Depression were soon to result in the creation of a National Government.¹³ In more limited terms, the Marxist approach clashed head-on with the prevailing historiography of science that had its home in the main international body of the discipline, the International Academy of the History of Science. The leaders of the Academy favoured a style with deep roots in the philosophically informed European and American approach to the history of ideas, going back to the beginning of the century. Institutionally, however, the history of science was a newcomer, a product of the Sixth International Historical Congress of 1928 in Oslo. There, Aldo Mieli, one of the most remarkable European pioneers of

¹¹ J. Needham, *A History of Embryology* (Cambridge: Cambridge University Press, 1934), xv–xvi. However, the main body of the book, which drew heavily on historical material in the first volume of Needham's *Chemical Embryology*, 3 vols (Cambridge: Cambridge University Press, 1931), shows no evidence of Marxist influence.

¹² Bernal's work reached fruition in *The Social Function of Science* (London: Constable, 1939). On Crowther's engagement in Marxist approaches to the history of science, see Chilvers, 'The Dilemmas of Seditious Men', *op. cit.* note 8.

¹³ The political and economic context of the 1931 Congress is treated in Werskey, 'Introduction', *op. cit.* note 6, xix, Werskey, *The Visible College*, *op. cit.* note 6, 138–149, and Chilvers, *op. cit.* note 6.

our discipline,¹⁴ had brought to fruition his vision of an organization that would promote the history of science on an international scale. The Comité International d'Histoire des Sciences, whose creation Mieli masterminded in Oslo, was transformed into the Academy in the following year in Paris, on the occasion of the First International Congress of the History of Science. As its permanent secretary-general, Mieli—with an international network of allies (many of them mathematicians, although Mieli began life as a chemist)—went on to impose an indelible mark on the history of science as an academic pursuit.

This is not to say that Mieli fashioned the history of science *de novo*. He drew not only on approaches pioneered in the 19th century by Auguste Comte in France and William Whewell in Britain, but also on more recent developments in the history of science within a variety of national contexts. I refer later to the founding, in 1901, of the Deutsche Gesellschaft für Geschichte der Medizin und der Naturwissenschaften (German Society for the History of Medicine and Science), and a full assessment of Mieli's contribution would have to take account of important initiatives in Scandinavia, The Netherlands, and central and Eastern Europe, as well as the work of 'émigré Europeans', such as George Sarton—now resident at Harvard after fleeing from Belgium—with *Isis*, in 1914.¹⁵ The fact

¹⁴ On Mieli, see P. Sergescu, 'Aldo Mieli', *Archives internationales d'histoire des sciences*, III^e année (12) (1950), 520–535. Among more recent studies, see C. Pogliano, 'Aldo Mieli, storico della scienza (1879–1950)', *Belfagor*, XXXVIII (1983), 537–557, which includes an exhaustive bibliography of Mieli's publications, and A. Di Meo, 'Aldo Mieli: La storia della scienza tra «programma nazionale» e «internazionalismo»', in Di Meo (ed.), *Cultura ebraica e cultura scientifica in Italia* (Rome: Editori Riuniti, 1994), 211–220. I am grateful to Anna Guagnini for drawing my attention to the work of Pogliano and Di Meo.

¹⁵ On Sarton, see note 21. The early history of the discipline in Denmark, Sweden, Finland, and Norway is well treated in Tore Frängsmyr, 'History of Science in Scandinavia', *Archives internationales d'histoire des sciences*, XXXV (114–115), (1985), 400–407. For more detailed accounts of the discipline in Sweden, see the chapters by Gunnar Broberg, Frängsmyr, and Gunnar Eriksson in Tore Frängsmyr (ed.), *History of Science in Sweden: The Growth of a Discipline, 1932–1982* [Uppsala Studies in History of Science, 2] (Uppsala, 1984), and Tore Frängsmyr, 'Intellektuell Historia', in *Svensk Idéhistoria: Bildning och Vetenskap under Tusen*, År, 2 vols (Stockholm: Natur och Kultur, 2004), vol. 2, 360–365. The history of the discipline in The Netherlands between 1913 and 1963 is treated in a collection of essays, edited by B.P.M. Schulté: *Vijftig Jaren Beoefening van de Geschiedenis der Geneeskunde, Wiskunde en Naturwetenschappen in Nederland, 1913–1963* (Leiden: Genootschap voor Geschiedenis der Geneeskunde, Wiskunde en Naturwetenschappen, 1963).

is, however, that Mieli's personal vision was powerful and captivating to the leaders of the emerging discipline. It was of a history that would be 'general' in the sense that it would embrace all the sciences and demonstrate their essential unity as pursuits that progressed through the application of a unified concept of scientific method. This not only privileged the evolution of ideas, but more contentiously conceived a path of ineluctable progress towards an ever more accurate understanding of nature. Progress could be hindered, as it had been by the Great War and the obscenities of Fascism, and was soon to be by National Socialism. But the natural order of things was as clear to Mieli as it had been to a lineage of thinkers in the positivist tradition going back to comte.

In the event, Mieli's vision was repeatedly clouded by personal tragedy. When he founded the Academy in the late 1920s, he was fifty and in his prime. In 1919, he had launched the journal *Archivio di storia della scienza* (renamed *Archeion* in 1927), which he edited for more than two decades. In 1928, sensing the cumulative pressures on one who was a socialist, a Jew, and a militant homosexual, Mieli abandoned Fascist Rome for Paris, where he was taken under the wing of Henri Berr, also Jewish, at the Centre International de Synthèse in the historic Hôtel de Nevers in the Rue Colbert.¹⁶ Although his years in Paris were active and productive, Mieli's existence (as head of the History of Science section of the Centre International de Synthèse) was always precarious, and in 1939 he was on the move again. This time his destination was Argentina, where, as a professor in a newly created Instituto de Historia y Filosofía de la

Footnote 15 continued

See also Bert Theunissen, 'Journals of the History of Science in The Netherlands', in Marco Beretta *et al.* (eds.), *Journals and History of Science* (Florence: Leo S. Olschki, 1998), 197–210, and Ernst Homburg's forthcoming article 'Boundaries and Audiences of National Histories of Science: Insights from the History of Science and Technology of the Netherlands', awaiting publication (in Greek) in *Neusis*. There is a rich bibliography in 'Literature on the History of Science, Medicine and Technology', in Klaas van Berkel *et al.* (eds.), *A History of Science in the Netherlands: Survey, Themes and Reference* (Leiden: Brill, 1999), 631–643. I am grateful to Ernst Homburg and Jo Wachelder for guiding me to these sources.

¹⁶ On Berr's role in the fashioning of the history of science, see Michel Blay, 'Henri Berr et l'histoire des sciences', in Agnès Biard, Dominique Bourel, and Eric Brian (eds.), *Henri Berr et la culture du XX^e siècle* (Paris: Albin Michel, 1997), 121–137; also P. Chalus, 'Henri Berr (1863–1954)', *Revue d'histoire des sciences et de leurs applications*, VIII (1), (1955), 73–77. On the Hôtel de Nevers and its place in the early history of the discipline, see Robert Halleux, 'Adieu à la rue Colbert', *Archives internationales d'histoire des sciences*, no. 145 (2000), 241–243.

Ciencia at the Universidad Nacional del Litoral in Santa Fe, he continued to edit *Archeion* until, in 1943, political changes deprived him of his post and forced publication to cease. Thereafter, financial hardship and illness increasingly clouded his existence, reducing him from 1947 until his death in 1950 to house-bound immobility.¹⁷

Mieli's rootlessness exacted a heavy price, and his scholarly life was littered with unfinished ventures – including just one volume of a projected 20-volume history of scientific thought, published in 1916; one volume of a four-volume *Manual of history of science* in 1925; and two volumes of an eight-volume *Panorama* of the History of Science (1945–1946). These were works of their time, in that they were all-encompassing and grandiose in conception, both thematically and chronologically. But they were not destined to have an enduring influence: the volumes that did appear are now little read and hard to find.

The legacy that did not disappear was Mielei's internationalism. This he shared with the remarkable circle that formed around the Academy and *Archeion*, in which scholars in the European tradition found common ground with North American scholars, most of them (like Mieli) with interests in ancient, medieval, and early modern history. On the European side, his leading contemporaries were Gino Loria (professor of geometry in Genoa and the author of a 1000-page study of Greek mathematics),¹⁸ Charles Singer (professor of the history of medicine at University College London),¹⁹ and the French historian of chemistry, Hélène Metzger.²⁰ In the United States of America, the main links (through correspondence, reading, and such personal

¹⁷ The intensity of Mieli's scholarly humanitarian and political ideals comes across – with a profound sense of frustration – in the autobiographical reflections that he published towards the end of his life. See Aldo Mieli, 'Digressions autobiographiques sous forme de préface à un panorama général d'histoire des sciences', *Archives internationales d'histoire des sciences*, no. 3 (April 1948), 494–505.

¹⁸ A. Natucci, 'Gino Loria (1862–1954)', *Revue d'histoire des sciences et de leurs applications*, VII (4), (1954), 372–374.

¹⁹ Singer's contributions to the early history of the discipline, in its international as well as its British dimension, are perceptively analysed in G.N. Cantor, 'Charles Singer and the Early Years of the British Society for the History of Science', *The British Journal for the History of Science*, XXX (1), (1997), 5–23. This special issue of the BJHS marked the fiftieth anniversary of the founding of the British Society for the History of Science in 1947. It contains several relevant contributions, including the presidential address that Singer delivered to the Society on 4 May 1948; see note 39.

²⁰ A useful source on Metzger is G. Freudenthal (ed.), *Études sur/Studies on Hélène Metzger* (Leiden: Brill, 1990), especially Freudenthal's 'Hélène Metzger: Éléments de biographie', 197–208.

contacts as were possible) were with George Sarton;²¹ the German-born historian of medicine and admirer of the Soviet system of medical care, Henry Sigerist;²² and the medievalist Lynn Thorndike.²³

Although Mieli's internationalism survived, the 1930s were not an easy time. An especially painful episode involved Karl Sudhoff, among his closest collaborators in *Archeion*, who had been a leading promoter (in 1901) of one of the earliest national societies of the history of science, the Deutsche Gesellschaft für Geschichte der Medizin und der Naturwissenschaften.²⁴ Sudhoff's espousal of the Nazi cause—'a foolish deed of his senility', as Sarton called it—went against everything that Mieli and his circle stood for, and it nearly destroyed the Academy.²⁵ Plans for the congress over which Sudhoff was due to preside in Berlin in September 1934 were proceeding well by January 1933,²⁶ but within 6 months, Sudhoff's collaborator, Paul Diepgen, had written to Mieli, formally withdrawing the German invitation and proposing a postponement of 1 or 2 years.²⁷ His explanation turned on the international situation and the difficulties of travelling. The reality was that Singer and the other leading figures in the Comité International d'Histoire des

²¹ Among the many accounts of Sarton's life and influence, see especially the contributions to the memorial issue of *Isis*, XLVIII (3), (1957), and A.W. Thackray and R.K. Merton, 'On Discipline Building: The Paradoxes of George Sarton', *Isis*, LXIII (219), (1972), 473–495.

²² Informative obituaries on Sigerist include the addresses gathered as 'In memory of Henry E. Sigerist M.D., D.Litt., LL.D., D.Sci., Dr.h.c.', *Bulletin of the History of Medicine*, XXXI (4), (1957), 295–308, and M.I. Roemer, 'Henry Ernest Sigerist: Internationalist of Social Medicine', *Journal of the History of Medicine and Allied Sciences*, XIII (2), (1958), 229–243.

²³ Thorndike was one of the seven original 'effective' members of the Comité International d'Histoire des Sciences who were elected in August 1928 during the Historical Congress in Oslo (along with Mieli, Abel Rey, Sarton, Sigerist, Singer, and Karl Sudhoff; see the list in *Archeion*, XI (1), (1929), 84–85). On Thorndike's life, see Marshall Clagett, 'Lynn Thorndike (1882–1965)', *Isis*, LVII (1), (1966), 85–89, and Pearl Kibre, 'Lynn Thorndike (1882–1965)', *Archives internationales d'histoire des sciences*, XX^e année (80), (1967), 285–288.

²⁴ On the founding of the society, see A. Frewer and Y. Steif, 'Personen, Netzwerke und Institutionen: Zur Gründung der Deutschen Gesellschaft für Geschichte der Medizin und Naturwissenschaften', *Sudhoffs Archiv*, LXXXVII, Heft 2 (2003), 180–194.

²⁵ G. Sarton, 'Acta atque agenda', in *Actes du VI^e Congrès international d'histoire des sciences. Amsterdam (14–21 août 1950)*, 2 vols (Paris: Académie Internationale d'Histoire des Sciences and Hermann, 1951–53), vol. 1, 45–78 (54).

²⁶ *Archeion*, XV (1), (1933), 168–170.

²⁷ *Archeion*, XV (2), (1933), 249–252.

Sciences had urged the cancellation of the Berlin congress for political reasons.²⁸ With much of the administrative structure already in place,²⁹ this sudden turn of events called for quick action. The first alternative was a congress divided between Spain (with sessions in both Barcelona and Madrid) and Portugal (in Porto, Coimbra, and then Lisbon).³⁰ But disagreements soon led to the abandonment of the venues in Spain, and to a decision to hold the entire congress at the three centres in Portugal.³¹

In the end, the 1934 Congress was a success for the discipline in Europe, or at least for its standing in Portugal.³² It drew strength from an active national group of historians (one that had close links with *Archeion* and the Academy) and from a wide resurgence of interest in Portugal's scientific tradition. Certainly, the discipline in Portugal benefited from the recognition the Congress bestowed. The quarterly journal *Petrus Nonius* began publication in 1937 under the editorship of Mieli's main Portuguese collaborator, Arlindo Camilo Monteiro.³³ But the sequence of events that led from the abandonment of Berlin to the adoption of Portugal reflected the vulnerability of internationalist ideals. This was the era that saw

²⁸ Chris Chilvers has observed that Mieli came round to Singer's view, after initially arguing that the congress should go ahead in Berlin.

²⁹ The preparations included discussion of financial support by industrial and public bodies. See A. Kleinert and C. J. Scriba, 'Der Nachlass von Hans Schimank (1888–1979): Bericht über ein Seminar', *Acta historica leopoldina*, no. 27 (1997), 287–314, where a copy of the notepaper for the congress is reproduced (302). I am grateful to Andreas Kleinert for drawing my attention to this source.

³⁰ *Archeion*, XV (2), (1933), 249–252, and XV (3), (1933), 446–448.

³¹ *Archeion*, XVI (1), (1934), 100–101, 114–116, and XVI (3), 337–372.

³² The proceedings were published as *III^e Congrès international d'histoire des sciences: Tenu au Portugal du 30 septembre au 6 octobre 1934, sous le haut patronage de S.E. le Président de la République portugaise. Actes, conférences et communications* (Lisbon, 1936). The report in *Archeion*, XVI (3), (1934), 337–372, conveyed Mieli's satisfaction. Sarton also looked back on the congress with evident pleasure; see his 'Lusitanian Memories', *Isis*, XXII (2), (1934–35), 440–455. Of about a hundred participants, almost two-thirds were from Portugal.

³³ The Portuguese group that Monteiro animated was one of the most successful of the groups that Mieli urged national communities to establish under the aegis of the Academy. For the political and cultural context of the discipline's development in Portugal, see Maria de Fátima Nunes, 'The History of Science in Portugal (1930–1940): The Sphere of Action of a Scientific Community', *e-JPH [e-journal of Portuguese History]*, no. 2 (2004). I am grateful to Ana Simoes for information about the 1934 Congress, echoes of which are evident in early issues of *Petrus Nonius*. See Arlindo Camilo Monteiro, 'Esclarecimentos sobre a actuação do "Grupo Português de História das Ciências"', *Petrus Nonius*, I (1), (1937), v–xi.

the Heidelberg physicist Philipp Lenard condemn Einstein's 'Jewish arrogance', and that saw him urge German scientists to fight against the 'Jewish spirit', and describe 'Aryan' 'deutsche Physik' as 'the physics of truth'.³⁴

The official accounts of the Academy mask its difficult relations with colleagues whose proximity to totalitarian regimes made them uncomfortable bedfellows. Animosities, however, ran deep. The disagreements that led to the abandonment of Spain as a venue for the 1934 Congress, for example, were represented as merely a matter of dates; the reality was that political differences between Catalan historians of science and their peers in Madrid so compounded Mieli's bad relations with the Madrid-based Spanish national group, that constructive collaboration was impossible.³⁵ With the international situation deteriorating, it became increasingly difficult to maintain the norms of academic life, and as mounting tension gave way to war, problems multiplied. One casualty was the Congress planned for Lausanne in 1940. Another, at the personal level, was Mieli, whose move from Paris to Argentina resulted in his virtual disappearance from the international community he had done so much to build. Yet another was Hélène Metzger's deportation and death at Auschwitz in 1944.

In a discipline whose leaders abhorred racism and the excesses of nationalism, such blows were hard. Once the war was over, however, hopes revived. Even the spat that divided Marxists and non-Marxists in 1931 was momentarily suppressed, and soon a circle of figures, with commitments across the political spectrum, found itself united in promoting the history of science as a discipline encapsulating the goals of post-war reconstruction and reconciliation. Of the

³⁴ A.D. Beyerchen, *Scientists under Hitler: Politics and the Physics Community in the Third Reich* (New Haven: Yale University Press, 1977), especially 79–102 and 123–167. On the Academy's resistance to anti-Semitism, see Cantor, *op. cit.* note 19, 9–15.

³⁵ The difficulties within the Spanish community culminated in the dissolution of the Spanish group of historians of science by Mieli on 24 February 1934. Thereafter, Mieli dealt only with Catalan colleagues, a number of whom attended the 1934 congress (in marked contrast with their peers from Madrid and elsewhere in Spain, none of whom were present). For the politically fractured Spanish context, see A. Roca-Rosell, 'El caso del congreso internacional de 1934: "Guerra" entre historiadores de la ciencia', in M. Valera and C. López Fernández (eds.), *Actas del V Congreso de la Sociedad Española de Historia de las Ciencias y de las Técnicas: Tomo II. Ciencia y técnica en la España contemporánea. Murcia, 18–21 de diciembre de 1989* (Murcia and Barcelona: PPU, 1991), 1066–1084. I thank Antoni Roca-Rosell for drawing my attention to this source and for providing me with a copy.

older generation, Sarton and Singer were still there. So, too, was Joseph Needham, along with his friend the biologist Julian Huxley, at UNESCO.³⁶ Among the new names were those of the Portuguese diplomat and historian of cartography, Armando Cortesão, the French philosopher Pierre Brunet (who carried the mantle of Henri Berr and Aldo Mieli, in particular as Mieli's successor in the History of Science section of the Centre de Synthèse in Paris),³⁷ and the remarkable Pierre Sergescu, a refugee from Romanian Communism, who had been director of the Polytechnic School in Bucharest until his definitive departure for Paris in 1948.³⁸ This reconstituted circle of disciplinary champions was as close-knit as ever, international in its vision, and united by a belief in the universalism of science. It shared to the letter Singer's description of the man of science as 'a citizen of the world', someone who 'speaks a language that can be understood by all who call themselves men',³⁹ just as it shared the conviction that scientific evidence compelled the assent of all human beings, irrespective of nation or culture.

Science emerged from this vision as axiomatically beneficent and civilizing: it did the work of evil only when it was distorted from its true purpose. In Sarton's 'new humanism', real understanding,

³⁶ J. Needham, 'UNESCO and the history of science', *Archives internationales d'histoire des sciences*, no. 1 (October 1947), 3–4. This text is followed, on pages 5–8, by a preface by Aldo Mieli to what was the first issue of the new journal (which, after an interval of four years, had taken the place of *Archeion*).

³⁷ H. Berr, 'In memoriam: Pierre Brunet', *Revue d'histoire des sciences et de leurs applications*, IV (1), (1951), 5–12, and P. Sergescu, 'Pierre Brunet', *Archives internationales d'histoire des sciences*, IV^e année (15), (1951), 480–482.

³⁸ M. Katerska-Sergescu *et al.*, contributions to a special issue of *Janus* devoted to the life and work of Pierre Sergescu: *Janus*, LV (1), (1955); G. Sarton, 'P. Sergescu 1893–1954', *Journal of the History of Medicine and Allied Sciences*, X (4), (1955), 421–425; F. S. Bodenheimer, 'Petre Sergescu (1893–1954)', *Archives internationales d'histoire des sciences*, VIII^e année (30), (1955), 3–4; followed by the 'Discours prononcé aux funérailles de Pierre Sergescu', on pages 5–6; R. Taton, 'Pierre Sergescu (1893–1954)', *Revue d'histoire des sciences et de leurs applications*, VIII (1), (1955), 77–80; and R. Taton, 'Pierre Sergescu (1893–1954): Son oeuvre en histoire des sciences et son action pour la renaissance des *Archives internationales d'histoire des sciences*', *Archives internationales d'histoire des sciences*, XXXVII^e année (118), (1987), 104–119, reprinted in Taton, *Études d'histoire des sciences recueillies pour son 85^e anniversaire par Danielle Fauque, Myriana Ilic et Robert Halleux* (Turnhout: Brepols, 2000), 453–466.

³⁹ C. Singer, 'The Role of the History of Science', the first presidential address to the British Society for the History of Science, delivered at the Society's annual general meeting on 4 May 1948. The text is in the *Bulletin of the British Society for the History of Science*, I (1), (1949), 16–18, and is reprinted in *The British Journal for the History of Science*, XXX (1), (1997), 71–73.

achieved through scientific method, would bring about consensus and, in the best Enlightenment tradition, ensure progress. Yet, even at its height, the post-war optimism was not wholly free from the shadows of 1931. At the first ordinary meeting of the British Society for the History of Science in October 1947, when all should have been sweetness and light, the classicist and Francis Bacon scholar Benjamin Farrington, turned his Marxist artillery on the physicist, historian, and philosopher Herbert Dingle. For Farrington, Dingle in his recent professorial inaugural lecture had considered only the 'thought aspect' of science, and had failed to see science 'as an integral part of human history in general'.⁴⁰ Where was society in Dingle's abstract scheme of things? Where was the driving force of the quest for the 'practical mastery of nature'? These were predictable questions coming from Farrington, who was at home neither in the British Society for the History of Science nor in the International Union of History of Science—which, from 1947, took over from the Academy the role of organizing the international congresses, beginning with Lausanne. While Farrington flirted with both the BSHS and the IUHS, he soon invested his main energies in the left-leaning UNESCO Commission for the History of the Social Relations of Science.⁴¹

As a body that owed much, in both spirit and leadership, to the Academy, the new International Union laid claim to the internationalist ideals that surfaced with renewed vigour after the war. It was less than all-embracing, however, in its conception of the discipline of the history of science. For while the style of

⁴⁰ A report on Farrington's address, 'What must we include in the history of science?', and Dingle's rather brusque response, is to be found in the *Bulletin of the British Society for the History of Science*, I (1), (1949), 6–7. See Cantor, *op. cit.* note 19, 21.

⁴¹ On the founding of the Commission, also of the Union and its relations with UNESCO, see A. Cortesão, 'L'UNESCO: Sa tâche et son but concernant les sciences et leur développement historique', in *Actes du V^e Congrès international d'histoire des sciences. Lausanne (30 septembre–6 octobre 1947)* (Paris: Académie internationale d'histoire des sciences and Hermann, 1948), 25–35. Also in this volume are texts of historical interest by the Swiss historian Arnold Reymond and Mieli. For a later account of the Commission and its latter-day descendants, see R.M. MacLeod, 'The International Commission on Science Policy Studies: Its Historical Context', in Jean-Jacques Salomon and Ina Spiegel-Rösing (eds.), *Contributions in Science Policy Studies*, for the XIVth Congress, International Union of History and Philosophy of Science (Tokyo, 1974); reprinted in *Zagadnienia Naukoznawstwa*, II (42), (1975), 316–322; and (with revisions) in *Archives internationales d'histoire des sciences*, XXV^e année (97), (1975) 314–323.

scholarship that dominated the congresses of 1934, 1937 (Prague), and 1947 (Lausanne) serenely perpetuated the vision that Singer had sought to defend in 1931, it now sat uneasily with the approaches of Farrington or Needham, or the UNESCO Commission. On the other hand, the 'International Union style' might have been expected to commend itself to philosophers, many of whom, especially in North America, began to establish new alliances with historians.⁴²

Some of these alliances led to the new departments of the history and philosophy of science. But in a number of national communities, as at the international level, the corresponding relations were not always close. There was some talk of integrating philosophy in the International Union of History of Science and in the title of the *Archives internationales d'histoire des sciences*, the successor (in 1947) to *Archeion*. In the short term, however, nothing came of it. What Cortesão referred to as 'jealousy' surrounding the word philosophy killed the idea,⁴³ and in 1949, the philosophers went their own way and formed the International Union of Philosophy of Science. It was only in 1955 that historians and philosophers came together institutionally in the International Union of History and Philosophy of Science, although even then they chose to work (as they still do) in distinct divisions: the Division of History of Science (History of Science *and Technology* since 2001) and the Division of Logic, Methodology and Philosophy of Science.

First-hand testimony of these uneasy relations comes from René Taton, then a newcomer to the international community, following his training as a teacher of mathematics and the presentation of his doctoral thesis on Gaspard Monge (with a shorter 'complementary' thesis on Girard Desargues).⁴⁴ In Paris, as Taton recalled many

⁴² *Catching up with the Vision: Essays on the Occasion of the 75th Anniversary of the Founding of the History of Science Society*, an independently paginated supplement to *Isis*, XC (1999), edited by Margaret W. Rossiter. This contains almost twenty contributions, mainly on the discipline in the United States of America, and celebrates the foundation of the HSS in 1924. In addition to studies of the post-war development of the subject at Harvard (by Joy Harvey) and Indiana (by Kevin T. Grau), see the eye-witness accounts of I. Bernard Cohen, Marie Boas Hall, and Charles C. Gillispie.

⁴³ Cortesão, 'L'UNESCO', *op. cit.* note 41, 34.

⁴⁴ D. Fauque, 'In memoriam: René Taton (1915–2004)', *Revue d'histoire des sciences*, LVIII (2), (2005), 267–303. Taton's recollections were well expressed in an interview published in 1997; see J. Peiffer, 'Entretien avec René Taton', *NTM. Internationale Zeitschrift für Geschichte und Technik der Naturwissenschaften, Technik und Medizin*, new ser. V (2), (1997), 65–89.

years later, philosophers, historians, writers, mathematicians, and others (a number of them refugees) met regularly at Pierre Serge-scu's frugal Saturday evening *soirées* in his flat in the Rue Daubenton.⁴⁵ But as Taton succeeded to the mantles of Brunet (who died prematurely in 1950), Sergescu, and eventually Koyré (becoming in due course director of what has been known since 1966 as the Centre Alexandre Koyré), Taton found relations between historians and philosophers difficult. This was especially the case within the CNRS, and most conspicuously so where new appointments were concerned.⁴⁶

In reviewing this period, it should be remembered that the philosopher Gaston Bachelard supervised Taton's thesis, and that Bachelard supported the history of science in France. But Bachelard was never involved actively with historians—at least with those who practised what Taton characterized as his own 'technical' style, founded on a detailed mastery of the scientific or mathematical content of rigorously established texts. The fact that such work was respected and from time to time drawn upon by Bachelard, as it was by Georges Canguilhem and his Parisian colleagues in the philosophically oriented Institut d'Histoire et Philosophie des Sciences et Techniques,⁴⁷ cannot be taken to imply that there was a sustained meeting of minds or a commonality of objectives. Among the discipline's leaders, only Koyré, it seems, managed to walk easily with both historians and philosophers, as he did throughout his career, from the early 1920s until his death in 1964.⁴⁸

Divergences of the kind to which Cortesão and Taton referred were not, however, matters of public debate, and to a beginning graduate student in Oxford in the early and mid 1960s, things appeared calm enough. The history and philosophy of science, as practised in Oxford, and elsewhere in Britain (notably Cambridge, University College, London, Leeds, Aberdeen, Leicester, and Durham), looked like a well enshrined entity that reflected the natural

⁴⁵ Peiffer, 'Entretien', *op. cit.* note 44, 68.

⁴⁶ *Ibid.*, 87. See also Fauque, 'In memoriam', *op. cit.* note 44, 297 and 300–301.

⁴⁷ The Institut was established in its present premises in the rue du Four in Paris in 1935, following its creation in 1932, at the instigation of the philosopher Abel Rey, as the Institut d'Histoire des Sciences. On the founding of the Institut, see the documents reproduced in *Archeion*, XIV(1), (1932), 103–105.

⁴⁸ On the work and influence of Alexandre Koyré, see the contributions to P. Redondi (ed.), 'Science: The Renaissance of a History', a special issue of *History and Technology*, IV (1–4), (1987).

order of things. It is only thanks to recent researches that we can sense the continuing tensions that rumbled on beneath the surface. Anna Mayer argues, for example, that during the early years of the Cold War, reservations about social and economic explanations made their mark in the History of Science Committee in Cambridge, with decisions about appointments and the direction the subject should take becoming bound up with anti-Marxist sentiments, despite the powerful presence of Needham.⁴⁹ If she is correct, it is not surprising that an early site of controversy emerged in the Department of History and Philosophy of Science in Cambridge, a group (given departmental status in 1972) in which historians and philosophers had worked together since the early 1950s.⁵⁰ It was there, from the early 1960s, that Robert Young, who was at the time head of the Cambridge Wellcome Unit for the History of Medicine and a Fellow of King's College, voiced his misgivings. In a discussion informed by a wide range of sources, Young asked whether the alliance between history and philosophy, and the exclusive character of the resulting HPS amalgam, could or should survive in isolation from other facets of historical enquiry: social, political, economic, or cultural. In answer, he delivered an unequivocal 'no'.

This 'no', of course, reflected Young's personal intellectual position, and under the leadership of Gerd Buchdahl, Mary Hesse, and Michael Hoskin. History and Philosophy of Science continued as a strong core element in the Cambridge department's offering, as it still does today. But by the time Young recalled these events in an autobiographical memoir in 1972, the climate in the discipline, as in society at large, had changed significantly.⁵¹ Crucially, there had been two decades of accumulating doubts about the benevolence of science, something that Sarton and Singer had taken as given.

⁴⁹ Mayer, 'Setting up a Discipline', *op. cit.* note 6, 55–65.

⁵⁰ G. Buchdahl, 'Twenty-five Years of History and Philosophy of Science at Cambridge', *The Cambridge Review*, XCI (1989), 167–71; M.A. Hoskin, 'History and Philosophy of Science in Cambridge', *Cambridge. The Magazine of the Cambridge Society*, no. 26 (1990), 46–50; and J.A. Bennett, 'Museums and the Establishment of the History of Science at Oxford and Cambridge', *The British Journal for the History of Science*, XXX (1), (1997), 29–46 (42–44).

⁵¹ R.M. Young, 'The Historiographic and Ideological Contexts of the Nineteenth-Century Debate on Man's Place in Nature', in M. Teich and R.M. Young (eds.), *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham* (London: Heinemann, 1973), 344–438 (esp. 348–361). I am grateful to Simon Schaffer for his helpful comments on the debates of the 1960s and 1970s about the kinds of philosophy that could or should become part of the historian's armoury.

There had also been two ‘tipping points’ in the history of the profession, focused on major conferences: ‘Critical Problems in the History of Science’, organized by Marshall Clagett at Madison, Wisconsin, in 1957, and ‘The Structure of Scientific Change’, organized by Alistair Crombie at Oxford in 1961.⁵²

A decade of discussion of Kuhn’s *Structure of Scientific Revolutions* had made its mark as well, although in ways that were still open to discussion. At the time, there were broadly two ways of reading Kuhn. One interpreted Kuhn’s normal science and revolutionary episodes as carried along by logic and the rational scrutiny of arguments and experimental evidence. But social scientists and historians with a contextualist agenda saw in *Structure* the way towards a sociological analysis of scientific change. This inspired new work in the social studies of science, which came to fruition in the 1970s in the Science Studies Unit at the University of Edinburgh, the Liberal Studies in Science Department at the University of Manchester, and the History and Social Studies of Science Division at the University of Sussex.

As scholars in these groups recognized, Kuhn’s work left many questions unanswered. But it had the merit of offering a promising alternative to the cumulative, progress-oriented visions of past science with which they wanted to break. While few believed that Kuhn had effected a revolution in historiography, many who focused on the social dimensions of science saw him as the architect of a revolution that remained to be carried through. This spirit informed work in the social history and sociology of scientific knowledge that characterized Project PAREX (Paris–Sussex), and that later inspired the European Association for the Social Studies of Science and Technology (EASSST).⁵³

It is the challenge inherent in Kuhn’s unfinished business that the present generation of students (of whom I wrote at the start) tend to see as the mainspring of the vigour of our discipline over the past four decades. To state that my students’ perception is

⁵² Papers from these conferences were published in M. Clagett (ed.), *Critical Problems in the History of Science: Proceedings of the Institute for the History of Science at the University of Wisconsin, September 1–11, 1957* (Madison: University of Wisconsin Press, 1959), and in A.C. Crombie (ed.), *Scientific Change: Historical Studies in the Intellectual, Social and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present* (London: Heinemann, 1963).

⁵³ For further details of EASSST, including its current programme of work under the presidency of Dr. Christine Hine (University of Surrey), see the Association’s website, <http://www.easst.net>.

understandable is not to say that it is wholly correct. Most importantly, their perception seems to overstate the influence that *Structure* has had on the practices of today's historians (whatever its influence on the social studies of science more broadly). And it carries with it the risk of undervaluing currents that not only throw light on Kuhn's questions, but also pose new ones. During the 1970s, the works of Michel Foucault, Jürgen Habermas, and Ludwig Wittgenstein (especially his *Philosophical Investigations*) had a huge following, and, by the 1980s, their influence was reflected in ground-breaking work on the social construction of knowledge. Traditional questions about the determining factors in scientific change, and the acceptance of new ideas were seen in a new light, as Steven Shapin and Simon Schaffer demonstrated in their *Leviathan and the Air Pump* (1985)—a book whose brilliance earned its authors the exceptional accolade of the Erasmus Prize in 2005.⁵⁴

I unreservedly applaud these new departures. Yet we should not be unmindful of the dangers they may bring. In particular, it must be a matter for regret that at least in Britain (and I can only stress again my personal perspective) the very proper interest in Pestre's 'new history of science' has had the effect, certainly not intended by Pestre himself, of diverting attention from even the best and, in its day, most influential writing of the historians of Mieli's and Taton's generations. What saddens me is the neglect of these traditions, not only as a model, but also as a resource. We all know how difficult it is to keep up with even a modest range of the current secondary literature. But we also know the excitement that comes with re-reading older authors, such as Metzger, or, in a more distant register, the work of that extraordinary polymath John Theodore Merz, whose four-volume *History of European Thought in the Nineteenth Century* (1896–1914) was the starting point for my own immersion in the subject.⁵⁵

Most strikingly, we seem to have distanced ourselves from the kind of encompassing vision that encouraged earlier historians. A.O. Lovejoy's *The Great Chain of Being* (1936), E.J. Dijksterhuis' *The Mechanization of the World Picture* (1956; English translation, 1961), and Alexandre Koyré's *From the Closed World to the Infinite Universe* (1957) treated major changes in mankind's conception of the universe, and did so over extended periods, using sources that

⁵⁴ S. Shapin and S.J. Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985).

⁵⁵ J.T. Merz, *History of European Thought in the Nineteenth Century*, 4 vols (Edinburgh: Blackwood, 1896–1914).

transcended national and linguistic boundaries.⁵⁶ If my students of today show less interest in such works than those of my own generation, it cannot be because Lovejoy, Dijksterhuis, and Koyré neglected the context of the ideas they studied; for they manifestly did not. The fact remains, however, that their view of the world stands poles apart from much current writing, with its emphasis on the fine structure of locality and time. This is not to say that any one approach is better than another. Still less is it to overlook the continued strength and fertility of the approaches that still bind the history of science to philosophy in many countries of continental Europe more closely than is generally the case in Britain. I simply highlight the dangers inherent in any closing off of an established, and highly distinguished, scholarly tradition.

All this said, I see not just dangers but also challenges and opportunities in what has occurred. The ‘geographical turn’—towards finely focused studies of the kind exemplified by Carlo Ginzburg’s *The Cheese and the Worms*,⁵⁷ Shapin and Schaffer’s *Leviathan and the Air Pump*, and Secord’s *Victorian Sensation*—has substantially extended our menu of methodological options. It has also forced us to reflect on the best strategies for writing not only for one another within the discipline but also in ways that will interest readers beyond our immediate professional community. Does it follow, for example, that as we delve ever more deeply into the geographically and temporally rooted world of Ginzburg’s 16th-century Friuli miller, we inevitably turn away from the kind of ‘big picture’ of history that might be expected to interest the general reader? If, as our model for such history, we take *The Edge of Objectivity* (1960), Charles Gillispie’s study of science since the Renaissance, it would be hard to deny that such a turning away has occurred.⁵⁸ For few of us now would feel equipped with the range, erudition, and perceptiveness that enabled Gillispie to unravel a connecting thread from science through half a millennium

⁵⁶ A.O. Lovejoy, *The Great Chain of Being: A Study of the History of an Idea* (Cambridge, MA: Harvard University Press, 1936); E.J. Dijksterhuis, *The Mechanization of the World Picture*, trans. by C. Dikshoorn (Oxford: Clarendon Press, 1961); and A. Koyré, *From the Closed World to the Infinite Universe* (Baltimore: Johns Hopkins University Press, 1957).

⁵⁷ C. Ginzburg, *The Cheese and the Worms: The Cosmos of a Sixteenth-Century Miller*, trans. by J.A. and A. Tedeschi (London: Routledge & Kegan Paul, 1980). The original Italian edition appeared as *Il formaggio e i vermi* in 1976.

⁵⁸ C.C. Gillispie, *The Edge of Objectivity: An Essay in the History of Scientific Ideas* (Princeton: Princeton University Press, and London: Oxford University Press, 1960).

of Western culture and to do so in a way that combined a strong thesis and formidable erudition with accessibility.

It must be said right away that the challenge of following in Gilispie's footsteps has not gone unanswered. John Pickstone's *Ways of Knowing*, for example, is a bold attempt to analyse the history of science, technology, and medicine from the Renaissance to the present in a 'new' way that respects chronology while breaking with the traditional structure of a single temporal sequence.⁵⁹ The fact remains, however, that the writing of histories of science covering vast periods and the whole range of the sciences has been taken up predominantly by authors working outside the community of academic historians. A recent book in this genre, *Science: A History 1543–2001*, by the distinguished science writer John Gribbin, makes the point. The book has had an understandable success, both with the reading public and with a supportive publisher, Penguin Books, which presumably sees Gribbin's history as less arcane than what might have been commissioned from an academic historian of science.⁶⁰ As a glowing review in the *Economist* put it, the merits of Gribbin's 'splendid' book lay precisely in its being at once free from 'trendy' postmodernism, unmarked by 'faddish' notions of scientific theories as social constructions, and founded on the view that 'science progresses by incremental steps rather than by revolutions or paradigm shifts'.⁶¹ The implication of the review was clear: *Science: A History* was a model of the lucid writing about past science that the reviewer saw as no longer emerging from the academic world.⁶²

Are we to assume, then, that the methodological trends in some of the more 'progressive' quarters of academic history of science have created a barrier for the wider audience that the generation of Mieli and the other inter-war pioneers of our discipline sought so resolutely to address? The danger is undoubtedly there, especially with regard to readers in the scientific community, who seem readier to look to their fellow scientists than to us for their history of science. In the light of this, it is all the more important to stress that the 'new history of science', rightly deployed, offers inviting

⁵⁹ J.V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Manchester: Manchester University Press, 2000).

⁶⁰ J.R. Gribbin, *Science: A History, 1543–2001* (London: Allen Lane, 2002).

⁶¹ *The Economist*, CCCLXIV (28 September 2002), 108.

⁶² For comment in a similar vein, see Luciano Boschiero, 'Stories about the Birth of Modern Science', *Minerva*, 43 (3), (2005), 311–318, and Patricia Fara, 'Newton, Industry and Empire', *Minerva*, 43 (4), (2005), 435–439.

vistas as well as snares. Secord's *Victorian Sensation*, for instance, is firmly in the 'Big Picture' tradition. But it is 'big' not in chronological span or in the range of the sciences it treats. Its bigness lies instead in its multiplicity of interfaces with literary theory, the economics of the book trade, Scottish church history, and the nature of working-class culture as well as with the author's knowledge of evolutionary theory.

The conclusion must be that filling the broad canvas of history, however we choose to do it, is still a worthy objective, just as earlier generations believed it to be—and as virtually all the contributors to the British Society for the History of Science's meeting of 1991 on 'Getting the Big Picture' agreed.⁶³ If the ways of filling the canvas and the tools for doing so have multiplied, so much the better. The challenge is to make sure that we develop an armoury that is as diverse as possible, so that we can become eclectic, and not prescriptive, in our choice of research methods and literary styles.

Which brings me to a concluding reflection. If a large international society, such as the European Society for the History of Science, has any single role to fulfil, it must surely be to help to open minds and build bridges, especially between countries that have been for too long separated by military and political conflicts. Of course, such a role has been played by other bodies, notably by the International Union of History and Philosophy of Science. But in the 1990s, the ending of the Cold War created new avenues for the academic communities of Europe. The founding of the European Association for the History of Medicine and Health in Strasbourg in 1991 was an early response, one that in turn encouraged colleagues from more than a dozen countries to gather in Paris in 2003 to found our own European Society for the History of Science (ESHS).

My overriding hopes for the new society are two-fold. First, that it will do everything it can to avoid exclusive allegiances to one or other school or style of work: in the history of science, as in all things, Procrustean beds are notoriously uncomfortable and in the historian's world they have no place. Second, I should wish to see its conferences and symposia serve as settings in which scholars from all the countries of Europe can at last work together on a

⁶³ *The Big Picture*, a special issue of the *British Journal for the History of Science*, XXVI (4), (1993), edited by James A. Secord. This issue contains historiographical articles by Secord, J.R.R. Christie, Andrew Cunningham and Perry Williams, John V. Pickstone, Andrew Barry, and Ludmilla Jordanova.

regular basis. One thing that will surely emerge will be a recognition of the variety of ways in which the history of science is pursued in different communities. With this recognition, I hope, will come toleration and respect, and a constructive pluralism that will exploit the diversity of Europe's intellectual traditions and not see it as a barrier.

In the history of science, we live by alliances, whether between individual groups, national communities, or whole disciplines. Quite apart from the intellectual benefits of solidarity, we have to recognize that many of us work in small groups that are marginal to the priorities of our parent institutions. As a result, places of higher education and research all too often see us as tempting targets when cuts are called for. Whilst the ESHS cannot on its own reverse such decisions, its broad membership should enable it to argue persuasively for the importance of endangered groups or individuals. I like to believe that our predecessors of the 1920s, 1930s, and 1940s would have warmed to these aims and the methodological tolerance they imply. Even Singer might eventually have seen the potential of Marxist perspectives, and perhaps the 'new history of science' as well. Like all those who strove with him, against heavy odds, for international understanding, he would certainly have applauded the creation of a society committed to fostering unity among scholars of a continent so cruelly scarred for most of the twentieth century by prejudice and war. The significance of our holding the Society's first conference in Maastricht, a city at the heart of the Continent and redolent of the European ideal, would not have escaped him.

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