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Lilley revisited: or science and society in the twentieth century

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Abstract. In the 1940s the Marxist mathematician and historian of science Samuel Lilley (1914–87) made a substantial contribution to British history of science both intellectually and institutionally. His role, however, has largely gone unnoticed. Lilley is otherwise portrayed either as exemplifying the immaturity of Marxism, most famously by Rupert Hall in 'Merton revisited' (1963), or as a tragic figure marginalized during the Cold War because of his communist commitment. But both themes of exclusion and victimization keep Lilley's legacy hidden. By revisiting Lilley and his long-standing commitment to developing our discipline, this essay challenges the notion of radical discontinuity with respect to Lilley's legacy and argues for a more sustained contribution by Marxist historiography of science. This, in turn, requires a more appreciative understanding of the moderate Marxist model developed by Lilley in his popular, political and professional publications on the history of the social relations of science.

In 1963 Rupert Hall published 'Merton revisited – or science and society in the seventeenth century'. In this paper he rejected the socio-economic analysis of Robert K. Merton in *Science, Technology and Society in Seventeenth Century England* (1938). At the same time Hall heralded the intellectualist approach of Alexandre Koyré in *Études galiléennes* (1939) in a frequently quoted remark: 'Merton summed up one epoch, that of the socio-economic historian, Koyré opened another, that of the intellectual historian.'[1] But discussions of 'Merton revisited' seldom attend to Hall's full argument. There was another explicit target in the text. For instance, when he triumphantly concluded that 'at the present time it is clear that the trend towards intellectual history is strong and universal', Merton was not mentioned at all. When Hall explained how no single article with a sociological interpretation of science had appeared in any of the leading journals since *Centaurus* in 1953 had published 'a special group of articles on the social relations of science', the explicit target was the British historian of science

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1 A. R. Hall, 'Merton revisited – or science and society in the seventeenth century', *History of Science* (1963), **2**, 1–16, 10.

Sam Lilley, editor of the *Centaurus* volume.[2] Thus Hall had a twofold agenda, criticizing both Merton in the United States and Lilley in the United Kingdom.

The relationship between Hall and Lilley is even more interesting. Lilley was one of the three candidates shortlisted for the first formal position in the history of science at Cambridge. Hall eventually got the job, but in retrospect has pointed out that both of his competitors, Crombie and Lilley, 'had progressed a good deal further'.[3] Politics clearly played a pivotal part in that priority. Lilley was a long-standing member of the Communist Party. As Anna-K. Mayer has shown, Joseph Needham reported indignantly in a letter to Charles Singer that 'Lilley was eschewed on account of his being a well-known Marxist'.[4]

If Lilley and his collection of *Essays on the Social History of Science* (1953) was a summing up of one epoch, that of the Marxist tradition, then Hall and *The Scientific Revolution 1500–1800* (1954) could be regarded as consolidating another, that of intellectual history.[5] Against such an interpretation, I will challenge Hall's retrospective claim in 'Merton revisited' of the existence of a radical discontinuity with regard to the Marxist tradition in the history of science. Through a reflexive application of Lilley's 'contribution model', I will argue that Lilley's legacy to some extent represents a Marxist continuity within the history of science. This requires a much broader understanding of our own disciplinary history, including both the popular and the political press as well as wider institutional and international aspects. By revisiting Lilley, I will also challenge the usual dismissal of the early Marxist contribution to the history of science as a form of social reductionism or extreme externalism. Consequently, both the assumed discontinuity and the alleged reductionism usually ascribed to Marxism must be modified.

The man, the machines and the history of science

Samuel Lilley was born in Belfast on 25 June 1914. His father, William Edwin Campbell Lilley, who married Lizzie Shaw in September 1913, worked in the linen industry. He began as apprentice and worked his way up, first as supervisor at the

2 Hall, op. cit. (1), 13. Lilley is also criticized on 6 and 14.

3 A. R. Hall, 'The first decade of the Whipple Museum', in *The Whipple Museum of the History of Science: Instruments and Interpretations, to Celebrate the 60th Anniversary of R. S. Whipple's Gift to the University of Cambridge* (ed. L. Taub and F. Willmoth), Cambridge, 2006, 57–68, 57–8.

4 Joseph Needham to Charles Singer, 7 June 1948, quoted from A.-K. Mayer, 'Setting up a discipline, II: British history of science and the "end of ideology" (1931–1948)', *Studies in the History and Philosophy of Science* (2004), **35**, 41–72, 59. The letter was later reprinted in L. Taub and F. Willmoth (eds.), *The Whipple Museum of the History of Science: Instruments and Interpretations, to Celebrate the 60th Anniversary of R. S. Whipple's Gift to the University of Cambridge*, Cambridge, 2006, 33–4.

5 A. R. Hall, *The Scientific Revolution, 1500–1800: The Formation of the Modern Scientific Attitude*, London, 1954; S. Lilley (ed.), *Essays in the Social History of Science*, *Centaurus* (1953), **3**. Lilley had a separate contribution to the volume, 'Cause and effect in the history of science', *Centaurus* (1953), **3**, 58–72. This paper was reprinted in 2008 in a special issue of *Centaurus*, celebrating the fiftieth volume of the journal, with short commentaries by H. Kragh, T. Arabatzis, H. Chang and J. Schickore, *Centaurus* (2008), **50**, 19–20, 32–45. The other well-known publication by Lilley is 'Social aspects of the history of science', *Archives internationales d'historie des sciences* (1949), **2**, 376–433.

handkerchief manufacturer Robert McBride & Son and then from the early 1930s as a factory manager at Lurgan Weaving Company. The family attended a Methodist church: the values of independent thought and education throughout life as a means for social betterment were characteristic of Lilley's family background.[6] Sam studied at the Royal Belfast Academic Institution from 1926 to 1932 and in 1935 graduated from the Queen's University of Belfast with first-class honours in science. One year later he gained an M.Sc. for research in algebraic geometry at Queen's.[7] Revealing evidence of Sam's early orientation towards the politics of science is a speech made to the Literary and Debating Society at the Academic Institution in 1931 on 'The function of science in modern life'. It is our duty, he argued, 'to see that every boy and girl in the future is given training in scientific method and in the application of science to life before his mind has become too conservative to appreciate the idea of progress'. This relationship between scientific progress and social progress would be a red thread running through his later writings in the history of science. Even at this stage his political position was quite outspoken: 'In a word some form of communism is now more than ever a necessity to mankind.'[8]

Lilley's teacher at Queen's, John Semple, was very much impressed by his work in mathematics and supported his wish to study at Cambridge by introducing him to James Wordie in 1935: 'I have a student called Lilley who wants to go up to St. John's next year as a research student in Geometry.'[9] From October 1936 Lilley studied at Cambridge and his Ph.D. thesis, supervised by William Hodge, was submitted in August 1938.[10] He was then employed as assistant lecturer in mathematics at King's College London, where Semple now held the chair of pure mathematics. So Lilley moved to London and began teaching in October 1938. When the thesis was approved in January 1939, he received very good recommendations. According to Professor Hodge it was a 'thesis of an unusually high standard' and 'a remarkable achievement for a man of his standing'.[11] In May 1940 Lilley was awarded a three-year fellowship in mathematics at St John's College, Cambridge.

The fellowship was suspended, however, while Lilley was on war service from 1940 to 1945. Crucially, it was during this period that he became committed to the pursuit

6 My thanks to Brenda Collins (Sam's niece in Belfast) for details on the family history. Communication on 28 January 2008.

7 M. D. Stephens, 'In memoriam: Samuel Lilley (1914–1987)', *Archives internationales d'historie des sciences* (1988), **38**, 291–2.

8 S. Lilley, 'The function of science in modern life', 14 September 1931, Lilley's notebook from Royal Belfast Academic Institution, possession of Lilley's family in Wilberfoss (subsequently Wilberfoss).

9 John Semple to James Wordie, 3 October 1935, St John's College Archives, Cambridge (subsequently SJC), TUF*/LILLEY, Samuel 1936.

10 S. Lilley, 'The isolated double points of an algebraic surface. The isolated unit points of a cyclic involution of prime order of an algebraic surface', thesis presented for the Degree of D.Phil., Cambridge University, 1938, CUL, Ph.D. dissertation 1002. Parts of the thesis were published as S. Lilley, 'On the isolated united points of a cyclic involution on an algebraic surface', *Proceedings of the London Mathematical Society* (1940), **46**, 312–60; *idem*, 'The characteristic exponents of a pair of power series', *Proceedings of the Cambridge Philosophical Society* (1940), **37**, 105–8; and *idem*, 'On the construction of algebraic curve branches of a given composition', *Journal of the London Mathematical Society* (1947), **22**, 67–74.

11 Statements by William Hodge, SJC, D93, 62.

of a professional career in history of science. When he returned to St John's on 8 September 1945 Lilley asked that the fellowship be changed: 'During the war my interest was gradually transferred to the history of science, and on my return to Cambridge in 1945, the College Council gave me permission to change my subject for research accordingly.'[12] Consequently, Lilley had a formal fellowship in the history of science in Cambridge three years before the first formal position at the Whipple Museum was given to Hall. In what follows I will try to explain Lilley's change of trajectory during the war by focusing on the professional, private and political aspects of his life.

When the London Blitz started, King's College was evacuated and the staff suspended, and Lilley lost his job. Back in Cambridge he got a temporary position at the Divisional Petroleum Office, and from 5 February 1940 he began working as an experimental officer in the Armaments Research Department of the Ministry of Supply.[13] His immediate superior was John Lennard-Jones, Plummer Professor of Theoretical Chemistry, who was central in establishing both theoretical chemistry and early computer science in the university.[14] According to Mary Croarken, his theoretical work on molecular wave functions required such huge and complex calculations that by 1935 'the work of the group had reached a point where the amount of computations necessary, primarily the solution of differential equations, was becoming too large and too complex to be carried out by hand'.[15] In the meantime, Vannevar Bush at MIT had solved the main problems concerning force and friction with the invention of the torque amplifier and the construction of the first differential analyser in 1930.[16] By 1934 Douglas Hartree at Manchester University had a small-scale model built in Meccano. An improved model was built in Cambridge in 1935 under the direction of Lennard-Jones, who in 1936 appointed Maurice Wilkes as technical assistant. Together they convinced the University Board of Mathematics to set up a central computing laboratory, including a full-scale differential analyser from Metropolitan Vickers in Manchester to be used by all scientists at Cambridge. This was approved in May 1937 and the Cambridge Mathematical Laboratory was officially opened in October with Lennard-Jones as director and Wilkes as demonstrator, yet the differential analyser was not delivered and installed until October 1939.[17]

In early September 1939 the Mathematical Laboratory was taken over by the Ministry of Supply. Lennard-Jones was charged with transforming it into an External Ballistics Department. According to Croarken, he built up a research team to work on ballistic calculations, experimental work on guns and rockets, high explosives research

12 Sam Lilley to William Mansfield Cooper (the Registrar, University of Manchester), 20 July 1948, Niels Bohr Archive, Copenhagen (subsequently NBA), History of Science, Correspondence Generale, History of Science 1940–51.

13 Sam Lilley to H. S. Harley (accountant, King's College London), 9 February 1940, King's College London (subsequently KCL), personnel file on Sam Lilley, ref. 1941 KA/FPA.

14 Sam Lilley to S. T. Shovelton (secretary, King's College London), 9 March 1941, KCL, op. cit. (13).

15 M. Croarken, *Early Scientific Computing in Britain*, Oxford, 1990, 54.

16 Cf. J. Huxley, 'Labour-saving in mathematics', *Listener* (1933), **9**, 711–12.

17 Croarken, op. cit. (15), 57–8.

and sound ranging.[18] This research group at the External Ballistics Department in Cambridge also included the promising mathematician Sam Lilley. In Lennard-Jones's notebooks of the early 1940s there are numerous references to Lilley and his work on trajectories and range tables at the External Ballistics Department in Cambridge.[19]

A valuable source for Lilley's experience with early computers is his first publication on 'Mathematical machines' in *Nature* in 1942.[20] Here he outlined the history of calculating machines from Pascal, Leibniz, Babbage and Kelvin to Bush and the Second World War. He also displayed his hands-on knowledge of technological devices such as the Brunsviga calculating machine, the Hollerith punch-card machine, the electrical Mallock machine and the differential analyser or Bush machine. It this sense, Lilley's earliest contribution to the history of science was derived from his practice and experience with mathematical machines.[21] During the war Lilley gradually broadened his approach to the history of science and published widely through various channels in the popular, political and professional press. I will first outline his contribution to popular science or, to be more exact, popularized history of science, before turning to his more political and professional publications.

Lilley's main channel for popularizing both science and the history of science was *Discovery: A Journal for Science and Progress*. The journal was recently discussed by Peter Bowler in his 2005 BSHS presidential address. While researching experts and publishers, Bowler soon discovered that renowned writers such as Huxley, Eddington, Crowther, Haldane, Hogben, Bernal and Waddington 'were only the most visible fraction of a regular industry among professional scientists writing for a non-specialist readership'.[22] Bowler does not mention Lilley, but the description fits. Lilley wrote more than twenty-five articles and book reviews in *Discovery* from 1943 to 1955. Similarly, as the new editor of *Discovery* in 1938, one of the first actions of C. P. Snow was to introduce a series of lectures on the history of science originally presented in a seminar by the Cambridge History of Science Committee organized by Joseph Needham and Walter Pagel.[23] The initiatives of scientists such as Lilley, Snow, Needham and Pagel were aimed at popularizing both science and the history of science, especially since their efforts coincided with the establishment of history of science in

18 M. Croarken, 'The emergence of computing science research and teaching at Cambridge, 1936–1949', *IEEE Annals of the History of Computing* (1992), **14**, 10–15, 11.

19 There is no mention of Lilley either in Croarken, op. cit. (15) or in *idem*, op. cit. (18). The notebooks are kept at the Lennard-Jones Papers, Churchill College, Cambridge (subsequently CCC), LEJO 24-25.

20 S. Lilley, 'Mathematical machines', *Nature* (1942), **149**, 462–5. Lennard-Jones was fully informed about this publication. CCC, LEJO 24.2 (notes on both 28 November and 28 December 1942).

21 S. Lilley, 'Machinery in mathematics I', *Discovery* (1945), **6**, 150–6; *idem*, 'Machinery in mathematics II', *Discovery* (1945), **6**, 182–5; *idem*, 'ENIAC, ASCC and ACE: machines that solve complex mathematical problems', *Discovery* (1947), **8**, 23–7 (cont. 32); *idem*, 'Review of Hartree (1947) *Calculating Machines*', *Discovery* (1947), **8**, 249; *idem*, 'The work of a century – in a few minutes: calculating machines, and the revolution in science which they are helping to bring about', *UNESCO Courier* (1952), **5**, 8–10.

22 P. Bowler, 'Experts and publishers: writing popular science in early twentieth-century Britain, writing popular history of science today', *BJHS* (2006), **39**, 159–87, 163. For his comments on the journal *Discovery* see 179.

23 A.-K. Mayer, 'Setting up a discipline, I: conflicting agendas of the Cambridge History of Science Committee, 1936–1950', *Studies in the History and Philosophy of Science* (2000), **31**, 665–89, 673.

Cambridge.[24] For instance, Lilley informed the readers of *Discovery* about the latest institutional efforts towards developing history of science as a discipline, such as the opening of the Whipple Museum in 1944, the establishment of the British Society for the History of Science (BSHS) in 1947 and the Fifth International Congress for the History of Science in Lausanne in October 1947.[25]

Lilley also had a much broader audience than the readers of *Discovery*. From 1946 he wrote a regular monthly article for the newspaper *Unity: The Worker's Voice* published by Unity Press in Northern Ireland. In the early 1950s he was general editor of the popular science series *Science in Action*.[26] Lilley was also a popular broadcaster on BBC radio: as a computer pioneer he presented the latest news on mathematical machines and artificial intelligence to a broader audience. According to Allan Jones, 'one of the earliest speakers to broadcast on computers was the Cambridge mathematician Sam Lilley, who had worked in ballistics during the Second World War. He gave two radio talks in 1945, but no material from them survives.'[27] In September 1949 Lilley also gave a talk on 'Electronic calculating machines' for the Overseas Service in a series introduced by Charles Darwin and followed by such notable speakers as Douglas Hartree, Maurice Wilkes, Norbert Wiener and Alan Turing.

Lilley's major contribution to the popularization of history of science was doubtless his first book *Men, Machines and History* (1948). According to Lilley the work had to be done 'almost entirely in spare time while carrying on a war job'.[28] As indicated by the subtitle, 'A short history of tools and machines in relation to social progress', the book outlined the role of tools and technology from 'The first industrial revolution' (3000 BC) to the 'The second industrial revolution', followed through its embryonic state (500–1440), childhood (1440–1660) and youth (1660–1815) to its maturity (1815–1918). For Lilley there was no such thing as a 'scientific revolution' in the seventeenth century. Having explained the role of skilled craftsmen in the Middle Ages, exemplified by various treatises on machines and applied mechanics in mining and

24 S. Lilley, 'Science for freedom: 1543–1943, I', *Discovery* (1943), **4**, 113–7; *idem*, 'Science for freedom: 1543–1943, II', *Discovery* (1943), **4**, 147–51; *idem*, 'Review of Lyons (1944) *The Royal Society*', *Discovery* (1945), **6**, 62; *idem*, 'Review of Conant (1947) *On Understanding Science*', *Discovery* (1948), **9**, 266.

25 S. Lilley, 'History of Science Museum at Cambridge', *Discovery* (1944), **5**, 352; *idem*, 'History of science', *Discovery* (1947), **8**, 197–8; *idem*, 'International History of Science Congress', *Discovery* (1947), **8**, 381. It is also worth mentioning that among Lilley's first academic papers was a study of an outlet for popular science in the early nineteenth century; *idem*, '*Nicholson's Journal* (1797–1813)', *Annals of Science* (1948), **6**, 78–101.

26 The *Unity* series was introduced with S. Lilley, 'Science and socialism', *Unity: The Worker's Voice*, 23 February 1946, 2. The first two books in the Science in Action series were A. F. Parker-Rhodes, *Fungi, Friends and Foes*, London, 1950, and H. S. W. Massey, *Atoms and Energy*, London, 1953.

27 A. Jones, 'Pioneers on the air: BBC radio broadcasts on computers and A.I., 1946–56', *Actes du sixième Colloque sur l'Histoire de l'Informatique et des Réseaux, Éditions ACONIT*, Grenoble, 2002, 14–28 (available at http://oro.open.ac.uk/5614). See also P. Partner, *Arab Voices: The BBC Arabic Service 1938–88*, London, 1988, 70: 'The first talk on computers seems to have been that on "Calculating machines" by a Cambridge don, Sam Lilley, in 1945.'

28 S. Lilley, *Men, Machines and History: A Short Story of Tools and Machines in Relation to Social Progress*, London, 1948, Preface. See also the related articles, *idem*, 'Technology in a changing world', *World Review* (1944), 32–9; *idem*, 'The story of alum', *Discovery* (1949), **10**, 350–4; *idem*, 'From sickle to combine', *Farming: The Journal of Agricultural Progress* (1950), **4**, 19–22.

metallurgy by Agricola, Ramelli, Zonca and Leonardo da Vinci, he rather emphasized the revolutionary implications of iron and printing before outlining how the 'commercial revolution' led to new inventions related to navigation, manufacture, mining and military technology.[29] The main focus in the book was not the so-called scientific revolution of the seventeenth century but rather the fully matured Industrial Revolution of the nineteenth. Finally, having classified and graded some two thousand important technological innovations throughout history, Lilley produced a series of graphs illustrating the relationship between social structure and technical invention, or the 'relative invention rate'.[30] These curves also indicated the more recent discrepancy between developments in the USSR and the dysfunctions of capitalist organization in the West, where social progress was being frustrated and restricted by patenting and monopoly. This explicitly political turn towards the end of the book was indicated by the reviewer in *Discovery*: 'Unfortunately, however, the author has not kept polemics out of it, and increasingly towards the end of the book one is actually aware of a Marxist bias in argument and choice of quotations.'[31] Denis Segaller concluded that the book was unsuited for young people in schools and at universities who would probably be unable to discriminate between Lilley's presentation of facts and opinions. What is striking, however, is how many of Lilley's implicit Marxist perspectives in *Men, Machines and History* (1948) were considered appropriate.

The invisible colleague

Before arriving in Cambridge in the mid-1930s Lilley had not been politically active. Yet in 1935 he had been in Germany and seen the Nazis march in Heidelberg and Munich. He also visited Italy in 1937, 'to find out something of the political atmosphere of the country, a subject in which I am particularly interested (as an opponent of the present regime)'.[32] He joined the Communist Party the same year. The reason, he later reported, was a 'pretty general political conviction of the correctness of Cumminist [*sic*] attitude, though in a rather abstract sort of way'.[33] A more specific reason for Lilley's political radicalization was his girlfriend Pearl Brammar, whom he married on 12 August 1939. Born in Cambridge in 1915, she began working at the Cambridge Co-operative Company at the age of fifteen. She was active in both the Labour League of Youth and the Shop Assistants' Union. In 1934 she became a member of the Communist Party. During the war she was responsible for the Cambridge Housewife Committee and a member of the Anglo-Soviet Committee. After the war, in the municipal elections of 1946 and 1947, Pearl Lilley was adopted as prospective candidate

29 Lilley, *Men, Machines and History*, op. cit. (28), 47: 'This was a revolution comparable to that brought about by the introduction of iron. Iron democratized physical tools; printing did the same for the tools of thought.'

30 Lilley, *Men, Machines and History*, op. cit. (28), 180 ff.

31 Lilley, *Men, Machines and History*, op. cit. (28), 204–5. D. Segaller, 'Review: *Men, Machines and History*. By S. Lilley', *Discovery* (1948), **9**, 385.

32 Sam Lilley to James Wordie, 9 April 1937, SJC, TUF*, LILLEY, Samuel 1936.

33 Personnel file 'Samuel Lilley', Papers of the Communist Party of Great Britain, Labour History Archive and Study Centre, Manchester (subsequently PCP).

by the Cambridge Communist Party.[34] She remained active in the peace movement and from 1968 worked as general secretary of the British–Soviet Friendship Society. In December 1975 she was awarded the Order of Friendship of Peoples by the Supreme Soviet of the USSR, presented to her by Ambassador Nikolai Lunkov at a ceremony at the Soviet Embassy.[35]

Pearl's father, Charles Brammar, had been working for the Cambridge Scientific Instrument Company since 1907. Unlike Robert Stewart Whipple, who was the managing director from 1905 to 1935 and whose collection of more than one thousand scientific instruments and antiquarian scientific books later formed the basis of the Whipple Museum of the History of Science and subsequently the Department for the History and Philosophy of Science in Cambridge, Brammar was a man of the shop floor, testing laboratories and trades unions.[36] He was also a well-trained and skilful craftsman. Educated at Shelton National School in the early 1890s, he was specializing in model drawing, magnetism, electricity and practical chemistry by 1895, before moving on to the advanced stage of mathematics and model drawing by 1897. When Whipple retired in 1935, Brammar received a long-service certificate and a clock presented to the twenty-six of the 350 employees present who had served the company for twenty-five years or more.[37] Brammar remained at the Instrument Company until his retirement in 1947. Both Sam and Pearl shared his long-standing interest in scientific instruments and the social aspects of science and technology. They were both invited to attend the formal presentation of the Whipple collection at Regent House on Saturday 4 November 1944.[38] In a sense, Sam Lilley represented an alternative association between the Cambridge Scientific Instrument Company and the establishment of history of science in Cambridge, not from Robert Whipple in the board room, but from Charles Brammar on the shop floor.

Obviously, Pearl and Charles Brammar were not the sole inspiration for Sam's political radicalization. Cambridge in the late 1930s was much influenced by the social responsibility of science movement, as portrayed by Gary Werskey in *The Visible College* (1978). Werskey's book was a collective biography of five famous scientists and socialists in the 1930s: Bernal, Haldane, Hogben, Needham and Levy.[39] More recently, Werskey has also added Blackett as the 'sixth man' in this group, yet Lilley has rather remained an 'invisible colleague' in Werskey's writings on *The Visible*

34 Cambridge Branch of the Communist Party, *Cambridge Today and Tomorrow: The Policy of the Cambridge Communist Party*, Cambridge, 1946.

35 Transcript from TASS, 15 December 1975, Wilberfoss.

36 Conversations with Pearl Lilley, 26–7 February and 13–14 September 2007.

37 There is no mention of Brammar in the official history of the company by M. J. G. Cattermole and A. F. Wolfe, *Horace Darwin's Shop: A History of the Cambridge Scientific Instrument Company 1878 to 1968*, Bristol and Boston, 1987. However, the event was reported under the heading 'Mr. Robert Whipple Retires', in *Cambridge Standard*, 24 January 1936, 9. A paper clip is to be found among the research material for the book. 'Cambridge Scientific Instruments Co. Ltd', CUL, Box 4, 'Minute Book No 3: Cambridge Instrument Co's Sports and Social Club'.

38 Letter accepting the invitation from S. Lilley ('signed by Pearl Lilley'), Whipple Museum of the History of Science, Department for the History and Philosophy of Science, Cambridge, B I 37.

39 G. Werskey, *The Visible College: A Collective Biography of British Scientists and Socialists of the 1930s*, London, 1978.

College.[40] This is especially unfortunate because, unlike the others in the group, Lilley most thoroughly transformed his political radicalism into a professional career within the emerging discipline of history of science.

During the war Lilley had developed a fully Marxist approach to the history of science in relation to this group of radical scientists. In 1942, for example, when *Nature* invited a group of academic sociologists to comment on the 'science and society' debate, Lilley criticized this dilution of the debate as a 'grand demonstration of the poverty of bourgeois sociology. In 17 pages, only two refs. to Marx, one to Lenin, none to the class struggle!'[41] Lilley also wrote material for the Communist Party. His first booklet on *Science and Progress* (1944) appeared in the Story of Science Series projected by the Young Communist League. Here Lilley focused on more recent developments: the initial progressive elements of capitalism had grown old, thus perverting the progress of science through patenting, monopoly capitalism and imperialism. Yet the critique of science under Fascism was even stronger: 'Fascism is the very antithesis of science'.[42] The only hope for Britain was to follow the USSR in developing a science for the people. It is worth emphasizing that Lilley's approach was not simply descriptive but also imperative. British scientists should join the fight against Fascism because the 'chaotic unplanned science of peace-time capitalism is no use for war'.[43] Thus the final section of the booklet was devoted to the growing political mobilization among scientists, particularly in the Association of Scientific Workers (ASW), which in the spring of 1942 had affiliated to the Trades Union Congress.[44]

Crucially, Lilley's first lectures in the history of science were transmitted through the ASW as presented by the *Scientific Worker* in July 1942: 'We have received from the Secretary for the Cambridge Branch some notes on a series of lectures recently given by Dr. S. Lilley. The notes form an excellent framework for a lecture series or discussion group.'[45] It is also noteworthy that as early as 1942 Lilley was a well-known lecturer in the history of science even outside the ASW. For instance, in October he was invited by Margaret Hammerton, the secretary of the Bedford College Scientific Association: 'I have heard of the successful & interesting talks which you gave on this subject last year, and feel that we would profit greatly if you would speak to us.'[46] From the subsequent correspondence it appears that Lilley gave two talks in November 1942,

40 G. Werskey, 'The visible college revisited: Second opinions on the red scientists of the 1930s', *Minerva* (2007), **45**, 305–20.

41 Unpublished notes, Papers of Dr Sam Lilley (1914–87), University of Nottingham, Manuscripts and special collections, GB 159 MS 200, 189. See the section on 'science and society' in *Nature* (1942), **149**, 85–102.

42 S. Lilley, *Science and Progress*, London, 1944, 30.

43 Lilley, op. cit. (42), 46.

44 R. MacLeod and K. MacLeod, 'The contradictions of professionalism: scientists, trade unionism and the First World War', *Social Studies of Science* (1975), **9**, 1–32. D. Horner, 'The Cold War and the politics of scientific internationalism: the post-war formation and development of the World Federation of Scientific Workers 1946–1956', in *Internationalism and Science* (ed. A. Elzinga and C. Landström), London, 1996, 132–61.

45 'History of science lectures', *Scientific Worker: Journal for the Association of Scientific Workers* (1942), **14**, 30.

46 Margaret Hammerton to Sam Lilley, 15 October 1942, Wilberfoss.

entitled 'Whence modern science?' and 'Science and society through the ages'. Lilley was giving popular lectures in history of science in Cambridge years before any formal positions existed.

Why was the history of science so important for the radical scientists within the ASW? According to Lilley the answer was quite simple, as outlined in *Science and Progress*: 'Since we build the future on the experience of the past, a lot can be learned from the history of science considered in relation to society.'[47] He suggested members of the Young Communist League should study books by Crowther, Childe, Bernal and Needham, as well as the American journal *Science and Society*, and he recommended a series of relevant publications by the ASW, such as their 1943 pamphlet on *Planning in Science*. In fact, Lilley also co-authored another ASW memorandum in 1943 on the *Development of Science*. His contribution here explicitly addressed the crucial question 'Why study the history of science?' His conclusion is worth quoting in full:

> In fact, if the moral to be drawn from a study of the history of science had to be stated in a sentence it would be this: science is so closely linked with other aspects of social life that any scientist who cares for the progress of his subject must necessarily consider it in its relations to the whole development of society and must take action, even in extra-scientific spheres, in conformity with that consideration.[48]

According to Lilley, the history of the social relations of science was crucial for working scientists, not primarily for humanists or citizens. And his argument was normative, not simply descriptive. Because progress in science was intimately interwoven with social progress, scientists had an obligation to be socially responsible. The practical implication of the idea of progress was for scientists to take action even in extra-scientific spheres. In this context, a broader historical understanding of the relationship between science and society was a necessary precondition for politically engaged scientists.

During the war the ASW had emerged as a strong trade union for about 18,000 scientific workers. According to Werskey this rapid assimilation of scientists into the labour movement was one reason why the Communist Party paid increasing attention to the claims of science during the 1940s, as in Haldane's party pamphlet *Why Professional Workers Should Be Communists* (1945). However, this concern also included an increased discussion of the history of science. Between 1945 and 1946 the Communist Party launched a series of extended discussions where, for the first time, economists, historians and philosophers inside the party seriously debated a range of scientific issues. According to Werskey, 'Echoes from these debates reverberated through the pages of *Modern Quarterly*. But the main forum here was the Engels Society.'[49] Not surprisingly, Lilley was associated with both these groups, though he has again remained in the shadow of the giants in *The Visible College*.

47 Lilley, op. cit. (42), 68.

48 S. Lilley, 'Why study the history of science?', in *An AScW Memorandum: The Development of Science*, London, 8 November 1943, 2–5, 5. For Lilley's further engagement in the ASW, see his conference reports: *idem*, 'Science in peace', *Nature* (1945), **155**, 260–2; *idem*, 'Publication and classification of scientific knowledge', *Nature* (1947), **160**, 649.

49 Werskey, op. cit. (39), 269.

Lilley was in fact central in launching the Engels Society, an association of Marxist scientists formed immediately after the war and devoted to the history and philosophy of science. The society is perhaps best known for the heated discussions on the Lysenko affair published from 1949 in the first volumes of the *Transactions of the Engels Society*. However, there previously existed an older Engels Society with a separate *Bulletin*, and this initial Engels Society was formally established at a meeting on 23 February 1946 in Hyman Levy's office at Imperial College London. At this first meeting a paper was presented by Sam Lilley on 'Causality and determinism'.[50] The initial Engels Society also planned to publish a book called *Scientific Materialism: The Scientific Picture of the World*, and here Lilley would contribute with a paper on mechanics. But in 1948 the Communist Party decided 'to liquidate this committee' and instead organize the scientists along the same lines as the history group of the Communist Party.[51] As a result, the scientists were divided into separate groups of biologists, physicists, chemists and psychologists, in addition to the medical Sigerist Society. It seems as if both Lilley and history of science as a topic had disappeared from the Engels Society by the time of the Lysenko affair and the publication of the *Transactions* in 1949. On the other hand, Lilley's contribution to history of science was acknowledged by members of the history group of the Communist Party: 'Here the value of Bernal's *Science in History* was stressed; as also, throughout, the importance of Lilley's *Men, Machines and History*.'[52]

Lilley was also associated with the Marxist journal *Modern Quarterly*, established in 1938 with Bernal, Haldane, Levy, Needham, Childe and Farrington on the Editorial Council. It came to an end in 1939, but was revitalized after the war. The first volume of December 1945 had contributions by Bernal and Haldane, while the back cover listed forthcoming articles by Levy, Zuckerman, Farrington, Childe, Hill and – of course – Lilley. Unfortunately this article by Lilley was never published in *Modern Quarterly* or elsewhere. This unpublished paper on 'The emancipation of geology' is a remarkable source. In a forty-four-page draft Lilley identified a distinct correlation between political progressiveness and geological uniformitarianism from James Hutton's *Theory of the Earth* (1795) to Charles Lyell's *Principles of Geology* (1830–3). Throughout this period, Lilley argued, the progress of factual discovery was gradually breaking the bonds which had formerly tied geology to Revelation: 'These things can only be fully explained when political, social and theological factors are added to these arising from the internal progress of scientific research.'[53] In addition to such methodological considerations of the interplay between the internal and external

50 Report and invitation sent to John Desmond Bernal by the secretary, Mrs Kay Cornforth, 18 March 1946, Bernal Papers (Add. 8287), CUL, I.16.1, 'Engels Society, 1946–48'. The unpublished paper by Sam Lilley on 'Indeterminacy and Causality' is located in the same folder.

51 M. Cornforth, 'Report on Engels Society, June, 1949', unpublished manuscript, Bernal Papers (Add. 8287), CUL, I.16.2, 'Engels Society, 1949'.

52 The Communist Party History Group, 'The teaching of history', *Marxism Today* (1959), **3**, 29–31, 31. E. Hobsbawm, 'The History Group of the Communist Party', in *Rebels and Their Causes: Essays in Honour of A. L. Morton* (ed. M. Cornforth), London, 1978, 21–48. R. Samuel, 'British Marxist Historians, 1880–1980', *New Left Review* (1980), **120**, 21–96.

53 S. Lilley, 'The emancipation of geology – an historical essay', unpublished draft, Wilberfoss.

aspects of science, the paper is valuable for understanding where Lilley himself drew the line between political polemic and professional history of science. Lilley wrote to Leon Rosenfeld explaining why he would not be submitting this geology paper when applying for academic positions: 'The point is that in this article I have been writing explicitly from a Marxist point of view (the article was intended for the *Modern Quarterly*, though it now seems that it may not be published there).'[54] The most obvious difference between this article and Lilley's other academic publications in this period is the explicit references to Marx and Engels in relation to the revolutionary ideology implicit in the new geology.[55] Consequently, the distinction was not so much between internalism and externalism, but whether political perspectives were implicit or explicit.

Lilley's turn towards history of science must be understood in relation to his political radicalization. Particularly through the ASW and the initial Engels Society, he was promoting the study of the history of the social relations of science from a political perspective, not simply as a methodology for socio-economic historians. At the same time, Lilley seemed well aware that although the social relations of science might be acceptable in the popularization of history of science, Marxism might not be equally appreciated within the professional community.

Cambridge at the crossroads

In the post-war period, history of science was not yet established as a discipline in Cambridge and, according to Mayer, 'the real founders of the first forum for history of science at Cambridge were scientists, *not* historians'.[56] The first initiative came from Joseph Needham and Walter Pagel, who in 1936 set up the Cambridge History of Science Committee. These scientists opted for a broad approach including both social and economic aspects. Yet when during the war Needham became involved in setting up UNESCO, the committee was taken over by Butterfield and the programme turned towards the history of thought.[57] Upon his return to Cambridge in April 1948 Needham complained about the situation in a letter to Singer: 'I feel that the committee which I myself founded has gone off the rails.'[58] Mayer further emphasizes the importance of academic appointments as key events in the institutionalization of a field. She has elaborated the 'Cold War perspectives' involved when Lilley was bypassed through Herbert Butterfield's appointment of Hall.[59] Lilley was in fact lecturing on behalf of the Cambridge History of Science Committee during autumn 1948 in a series on Classics in

54 Sam Lilley to Leon Rosenfeld, 29 June 1948, Leon Rosenfeld Papers, NBA, Box 1. History of Science, Correspondence Generale, Histoire des Sciences etc. 1952–58 (Manchester), Lilley (1948–59). For details on Rosenfeld in relation to both Hall and Bernal see A. S. Jacobsen, 'The complementarity between the collective and the individual', *Minerva* (2008), **46**, 195–214.

55 Lilley, op. cit. (53), 43. For a comparison see *idem*, 'Attitudes to the nature of heat about the beginning of the nineteenth century', *Archives internationales d'histoire des sciences* (1948), **2**, 630–9.

56 Mayer, op. cit. (23), 667.

57 Mayer, op. cit. (4).

58 Quoted from Mayer, op. cit. (4), 56.

59 Mayer, op. cit. (4), 55 ff.

the History of Science, together with A. C. Crombie and Miss R. H. Syfret. Thus while Hall was writing up his thesis, to be submitted in April 1949, his two main competitors for the first established position were both formally teaching history of science in Cambridge.[60] However, Butterfield had made it clear from the beginning that Lilley was not even being seriously considered:

> I think that Crombie of Jesus and Lilley of St. John's would regard themselves as candidates for any lectureship that may be offered. Although they ought to be considered by the Committee if the policy of trying-out lectures is discussed … I should hesitate about giving hopes to people where my mind was made up.[61]

Later, in the discussions concerning the subsequent appointment, Butterfield would simply dismiss Lilley as 'not a good personality'.[62]

'Cold War perspectives' should nonetheless be treated with caution. In the case of Lilley, at least, he was not completely excluded or stigmatized in Cambridge. On the contrary, he collaborated with both Butterfield and Hall on various occasions in popularizing the history of science through radio broadcasts and in publications. Lilley participated in a series of talks for the BBC during 1949 and 1950 together with Butterfield, Dingle, McKie and Sherwood Taylor, addressing the question 'what is the history of science?' These talks were later published as *The History of Science: Origins and Results of the Scientific Revolution* (1951). Lilley contributed his perspectives on 'The development of scientific instruments in the seventeenth century', focusing on Boyle and the air pump in relation to mining and industry. He also emphasized the progressive role of craftsmen and instrument-makers in facilitating the scientific revolution.[63] Finally, Lilley noted how the important role of people who actually knew, used and invented instruments was not always equally appreciated; 'in fact various forms of snobbery prevented the scientist from learning anything from humble workmen'.[64] This approach to the history of science certainly stood out in comparison with the other contributions in the volume, as pointed out by Crombie in his otherwise positive review: 'Dr S. Lilley follows with a sociological thesis about the development of scientific instruments in the seventeenth century, supposing that before that time scholars never used their hands, a thesis contradicted by many examples from the

60 'Lectures on the history of science', *Cambridge University Reporter* (1948), **79**, 171.

61 Herbert Butterfield to members of History of Science Committee (marked 'Confidential'), 28 May 1948, Needham Papers, CUL, B.309. Later, according to Needham, 'The general criticism of Lilley is that he is too Marxist, and of Crombie that he is too Thomist. Hall here in Cambridge has never much impressed me; he is wafted on by the Butterfield circle, to which I do not adhere.' Joseph Needham to Frank Sherwood Taylor, 2 October 1950, Needham Research Institute, Cambridge (subsequently NRI), SSC2, 16, 96. My thanks to Anna-K. Mayer for informing me about this letter.

62 Butterfield quoted in Needham's informal notes from the meeting of the History of Science Committee held on 3 June 1948. Needham Papers, CUL, B.309. This dismissive discussion of Crombie and Lilley was not included in the transcript of this document in the celebratory volume for the Whipple Museum of the History of Science. Taub and Willmoth, op. cit. (4), 33.

63 S. Lilley, 'The development of scientific instruments in the seventeenth century', in *The History of Science: Origins and Results of the Scientific Revolution* (ed. J. Lindsay), London, 1951, 65–75, 74.

64 Lilley, op. cit. (63), 74.

preceding five hundred years.'[65] Ten years later, both Hall and Lilley took part in another series of talks on history and philosophy of science at the BBC between May and July 1959. These contributions were edited and published in *The Making of Modern Science* (1960), with a foreword by Hall. Lilley's article on 'The nature of the physical world' focused on the rise of the science of mechanics in relation to economic and political theory.[66] On this occasion, Lilley received a more favourable review, by the young Jerome Ravetz.[67] Although controversial, Lilley's contributions were clearly included and regarded as acceptable.

Being suitable for the work of popularization was one thing, but a professional position was something quite different. Having been rejected for the position at the Whipple Museum, Lilley continued to apply for new funding opportunities. His St John's fellowship (£450 in total) would run out at the end of August 1948. The Leverhulme Trust had awarded him a grant of £200 (plus £50 for expenses for one year) from 1 September 1948 and in addition he would earn £180 a year writing editorial matter for *Discovery*. Yet with a wife and a small daughter to support, a regular income was now paramount:

> If I am driven to it, I think I can make a living by writing and broadcasting, but naturally this would reduce my research almost to zero, which I am anxious to avoid. My ultimate hope is that I shall eventually be able to pick up one of the very few jobs of teaching the history of science in the universities.[68]

At one point, Lilley was considered for a post in Oxford, but according to Mayer, McKie 'was torn between Lilley's competence, his know-how, his seeming "a good fellow" even, and his intellectual deviance ("I wish his ideas were different")'.[69] In April 1947 he also applied for a lectureship in the history of science at University College, Leicester, but with no success. By the time Lilley applied for a job at the Science Museum in 1949, the situation was rather desperate: 'So unless the job at the Science Museum comes off, I shall be completely on the rocks – or rather shall have to look for a school teaching job or something like that.'[70] Lilley did not get this job either. He started to apply for positions in a series of extra-mural departments in Bristol, Newcastle upon Tyne and Cardiff. He finally secured a position from January 1950 as resident organizing tutor at an initial salary of £700 per annum at the Birmingham University Extra-mural Department with responsibility for adult education in south-west Birmingham.[71] Here, Lilley gave a series of courses in the history of science for the

65 A. C. Crombie, 'Review: *The History of Science: Origins and Results of the Scientific Revolution. A Symposium*', *British Journal for the Philosophy of Science* (1952), **3**, 279.

66 S. Lilley, 'The nature of the physical world', *Listener* (1959), **62**, 1065–9, reprinted in *The Making of Modern Science* (ed. A. R. Hall), Leicester, 1960, 39–46. The other contributors were G. Buchdahl, M. Hoskin, M. Boas Hall and C. Raven.

67 J. Ravetz, 'Review: *The Edge of Objectivity*. By Charles Coulston Gillispie, and *The Making of Modern Science*. Edited by A. Rupert Hall', *British Journal for the Philosophy of Science* (1961), **12**, 250–5, 254.

68 Sam Lilley to Leon Rosenfeld, 29 June 1948, op. cit. (54).

69 Mayer, op. cit. (4), 59.

70 Sam Lilley to Leon Rosenfeld, 11 May 1949, Leon Rosenfeld Papers, op. cit. (54).

71 Sam Lilley to Leon Rosenfeld, 4 November 1949, NBA, Box 1. History of Science, Union internationales d'historie des sciences, Commission for the History of the Social Relations of Science (1946–56).

Workers' Educational Association. The teaching obligations were massive and by 1954 a total of 1,510 people had passed through his classes; twelve had attended classes for the whole period, and 184 for at least three successive years.[72]

The period in Birmingham from 1950 to 1956 was nevertheless very productive. He was working on three books at the time. First, as secretary of the Commission for the History of the Social Relations of Science, he was editing their collection of *Essays on the Social History of Science* finally published by *Centaurus* in 1953. Lilley worked hard to tone down the political issues involved. He explained in a letter to Bernal that there would 'be an opportunity to hint at political morals, though it would not be possible to go beyond hints, since the book will have to maintain the appearance of being non-political'.[73]

Second, Lilley was very much fascinated by the emergence of new automation technologies in various West Midlands factories and office spaces. He wrote a series of papers on the topic, eventually turned into *Automation and Social Progress* (1957) based partly on his journey to the USSR in 1955 to study the use of automation.[74] Despite dystopic references to Chaplin's *Modern Times* (1936) and Čapek's *R.U.R.* (1921), Lilley was remarkably enthusiastic about the new automatic factories. In particular, he explained how the use of automation in Russia was proceeding far better than in the West because of the planned socialist economy, negotiation with labour unions and a breakdown of the division of labour such that worker, thinker and creative artist could become one. Here, Lilley did not bother to conceal his own political engagement, as he explained in a letter to Charles Singer: 'I believe that to do nothing about this question of automation at the present time is to invite disaster. This is why I have felt that I could not keep up in this book any pretence of academic detachment.'[75]

Lilley's third project while in Birmingham concerned the history of the Lunar Society. At a dinner in January 1954 chaired by Sam Lilley on behalf of the Birmingham People's Peace Committee to commemorate the 150th anniversary of the death of Joseph Priestley, he suggested a revival of the Lunar Society by asking those at the dinner to contact him if they felt an active interest in re-forming the society. Again, the approach to the history of science was both descriptive and normative. His initiative was immediately supported at the meeting by the Nobel Prize-winner R. L. M. Synge, vice-president of the British Peace Committee, whose talk on Priestley had inspired Lilley to

72 Birmingham University Extra-mural Department, *Annual Report 1953–54*, 6. It is emphasized in the *Annual Report* for 1953–4 that Lilley, as the only resident tutor who was a mathematician, had indeed produced 'something special in the way of statistics' and that the department knew much more about these students than any others.

73 Sam Lilley to John Desmond Bernal, 26 March 1949, Bernal Papers (Add. 8287), CUL, B.3.207.

74 S. Lilley, 'Automatic factories', *Discovery* (1955), **16**, 147–52; *idem*, 'Technical aspects of automation', *Marxist Quarterly* (1956) **3**, 84–98; *idem*, 'Soviet progress in automation', *Process Control and Automation* (1956), **3**, 162–9; *idem*, *Automation and Social Progress*, London, 1957.

75 Lilley, *Automation and Social Progress*, op. cit. (74), 13. See also Sam Lilley to Charles Singer, 27 February 1957, Charles and Dorothea Singer Collection, Wellcome Library, London (subsequently CDS), A.11 Correspondence L, 7: 'I fear the book will do my reputation more harm than good in History of Science circles – it is very outspoken politically, but I have been feeling very strongly that this is one of those questions on which one just cannot keep neutral.'

suggest the revival.[76] Lilley's principal approach to the history of the Lunar Society was set out a year later in an article published in *Chemistry and Industry* on the occasion of the seventy-fourth annual meeting of the Society for Chemical Industry in Birmingham in July 1955.[77] Lilley was planning to finish a book on Erasmus Darwin by the summer of 1960, as he wrote to Leon Rosenfeld: 'He is, of course, a most useful peg on which to hang discussion of many "social relations" themes of the later 18th century.'[78] This book was never finished; we can only trace Lilley's approach to Erasmus Darwin and the Lunar Society in a series of pamphlets and articles.[79] But in 1966 Lilley was quite naturally a member of the steering committee for the celebration of the bicentenary of the foundation of the Lunar Society.[80]

While at the Birmingham Extra-mural Department Lilley continued to apply for positions in the history of science. For instance, in 1953 he applied for a fellowship at King's College, Cambridge. In his letter of recommendation Charles Singer described Lilley as 'a man whom I really much admire and always back', and further emphasized that he had 'some good ideas and his main thesis is thoroughly sound and needs properly working out'. Nevertheless, Singer would not recommend Lilley because of his political engagement:

> In spite of all this, I somehow do not see him as a Fellow of King's! The use of his time on political matters has distressed me greatly, and that apart from his particular views. I think he has learned his lesson. Were I a Fellow of King's I think I should probably vote for him. What a very good thing for King's that I am not![81]

Meanwhile, in 1956 Lilley moved with his family from Birmingham to a position in the Extra-mural Department at the University of Nottingham.[82] Even there he kept seeking positions in the history of science, both a lectureship in Leeds in 1958 and another one in Leicester in 1959, without success.[83] Lilley's final attempt to obtain a university position in the history of science arose when Hall resigned from his position in Cambridge with effect from 30 September 1959. Lilley again applied for the position he had failed to secure ten years earlier – Michael Hoskin was successful in obtaining

76 *Birmingham Post*, 19 January 1954: 'Revival of Lunar Society Suggested. Need Mentioned at Birmingham Dinner.'

77 S. Lilley, 'The Lunar Society', *Chemistry and Industry* (1955), **28**, 787–9.

78 Sam Lilley to Leon Rosenfeld, 13 June 1959, Leon Rosenfeld Papers, op. cit. (54). S. Lilley, 'Joseph Priestley: man of many parts', published lecture, Saltley Grammar School, 1962.

79 S. Lilley, 'Dr. Erasmus Darwin of Lichfield', *Transactions of the Johnson Society* (1956), 32–49; *idem*, 'The origin and fate of Erasmus Darwin's theory of organic evolution', *Actes du XIe Congrès International d'Historie des Sciences* (1968), **5**, 70–5; *idem*, 'La scienza alle origini della rivoluzione industriale', in *Studi Storici*, Instituto Gramsci Editore, Anno II, N. 3–4 (1961), 465–95, especially 474–80.

80 Lunar Society Anniversary Steering Committee, Report from Meeting, 25 March 1966, and the invitation folder to the 'Lunar Society Bicentenary Celebrations', both at Wilberfoss. See R. E. Schofield, *The Lunar Society: A Social History of Provincial Science and Industry in Eighteenth-Century England*, Oxford, 1963, 131, 473.

81 Charles Singer to G. T. Sheppard (provost at King's College), 12 November 1953, CDS, A.11, 2.

82 Stephens, op. cit. (7).

83 Sam Lilley to Leon Rosenfeld, 27 February 1958 and 13 June 1959, Leon Rosenfeld Papers, op. cit. (54).

the post.[84] Perhaps more remarkably, Derek John Price was among the other rejected applicants. Charles Singer had been asked by both Lilley and Price to write letters of recommendation: 'Lilley has the advantage of Price as a fully trained mathematician and perhaps as a writer and a philosopher. He is also strong on bibliography. He has, of course, good experience in teaching and I know him to be a good speaker.' Yet Singer considered Price much the better, 'both absolutely and for this position'. The enthusiasm for Price was shared by his other referees, Robert Oppenheimer and Otto Neugebauer, as well as Alexandre Koyré, who 'wholeheartedly recommend the appointment of Dr Derek Price'.[85] Later, in a letter to Needham, Price described the whole experience as chastening: 'to have offered myself in all humility for the post in Cambridge – a post far inferior in status to any that my friends here felt I could consider with dignity – and to have been rejected in favour of a man who has yet to prove his ability'.[86] Needham was, of course, distressed. He criticized the History of Science Committee for 'seeing the better and deliberately choosing the worse'. In fact, Needham had 'at no time felt worse about the development of the subject in Cambridge'.[87] The main target for Needham was of course his old nemesis Herbert Butterfield: 'Add to this that from October next Butterfield will be Vice-Chancellor for two years', he lamented, '"The saints under the altar cry, O Lord how long? how long?"'[88]

Having again been rejected at Cambridge by Butterfield and his allies Lilley did not, to the best of my knowledge, apply for other positions in the history of science. He remained at the Extra-mural Department in Nottingham until his death in 1987. Nevertheless, Lilley's double failure in gaining a Cambridge post should not overshadow his continuous commitment to the development of the discipline. Portraying Lilley's career as a tragedy would conceal his real contribution. Not only did he promote the history of science both in politics and through popularization, he was also very much involved in the institutionalization of professional history of science both nationally and internationally. For instance, Lilley was active in setting up the British Society for the History of Science (BSHS) from the very first meeting at the Science Museum on 22 November 1946 and was among those nominated to serve on the committee to draw up the rules and constitution. The society was formally established at the first Annual General Meeting on Monday 5 May 1947 and Lilley was elected to the council, where he served from 1947 to 1950.[89] At the second formal meeting of the

84 There is no mention of Lilley's contribution to the history of science in Cambridge in M. Hoskin, 'History and philosophy of science in Cambridge', *Cambridge: The Magazine of the Cambridge Society* (1990), **26**, 46–50.

85 Statements by Charles Singer to Holmes (registrar), 8 May 1959, Robert Oppenheimer to Holmes (registrar), 15 May 1959, Otto Neugebauer to Holmes (registrar), 15 May 1959, and finally Alexandre Koyré to Holmes (registrar), 18 May 1959, CUL, UA GB 100.

86 Derek John Price to Joseph Needham, 29 June 1959, Needham Papers, CUL, B.330.

87 Joseph Needham to Derek John Price, 14 June 1959, Needham Papers, CUL, B.330.

88 Needham, op. cit. (87).

89 G. Cantor, 'Charles Singer and the early years of the British Society for the History of Science' *BJHS* (1997), **30**, 5–23. J. Browne, 'Officers and council members of the British Society for the History of Science, 1947–97', *BJHS* (1997), **30**, 77–89, 81. Other foundation members included Bernal, Crowther, Needham and Rosenfeld, yet on the list of members from 1948 Hall and Butterfield were absent. 'List of members on

BSHS on 9 December 1947 Lilley presented a paper under the title 'Apology for caloric'. When the BSHS elected two delegates to represent the society at the Fifth International Congress for the History for the Science at Lausanne in October 1947 they voted for Singer and Lilley.[90] The Lausanne congress was also important as a setting for the establishment of the International Union for the History of Science (IUHS): Singer was elected president and Lilley was elected assessor for the council.[91]

Furthermore, Lilley was the first official editor of the *Bulletin for the British Society for the History of Science*, the official forerunner to what in 1962 became the *British Journal for the History of Science*. By the late 1940s, however, Lilley was desperately seeking a full-time job. At the council meeting on 26 October 1948 Lilley stated that 'he found difficulty in giving time to cope with the work of editing the Bulletin and felt that in view of this he ought to offer to resign'. The council unanimously agreed that the sum of £25 'should be allocated to Dr. Lilley as editorial expenses for the current financial year'. This support kept Lilley going and he thanked the committee for the help 'which would enable him to continue as editor of the Bulletin'.[92] One year later, however, Lilley had to let go of the *Bulletin*. At the council meeting on 24 October 1949 Lilley explained that he had 'great difficulty in finding sufficient time to do justice to the Editorship of the *Bulletin* and that he felt that he must submit his resignation to the Council though he did so with great reluctance'. The chairman, James Riddick Partington, expressed the sincere regret of the council and thanked Lilley warmly 'for the excellent service he had rendered to the Society in launching its first publication. Both numbers were very satisfactory and he hoped Dr. Lilley would be willing to see No. 3 through the press. To this Dr. Lilley agreed'.[93] Janet Browne indicates, correctly, that Lilley was the editor from 1948 to 1949. One can, however, add that in the very first volume of January 1949 Lilley effectively compiled the whole history and activity of the society from 1946 and also prepared Volume Three, published in April 1950.[94] Thus his contribution as first editor of the *Bulletin* was substantial and certainly acknowledged.

Despite the fact that Lilley was twice rejected at Cambridge, he was remarkably well regarded within both the BSHS and the IUHS. Despite his political commitments, he was acceptable to the wider professional community both nationally and internationally. This external appreciation of Lilley's externalism within the emerging

1st June, 1948', *Bulletin for the British Society for the History of Science* (1949), **1**, 19–24. In the subsequent list from 1951, Butterfield has been added, yet Hall has not. 'List of members on 1st January, 1951', *Bulletin for the British Society for the History of Science* (1951), **1**, 119–25.

90 'Reports of meetings', *Bulletin for the British Society for the History of Science* (1949), **1**, 6–10.

91 'Union internationales d'historie des sciences', *Archives internationales d'historie des sciences* (1948), **27**, 317–19. At the subsequent Sixth International Congress for the History of Science in Amsterdam in 1950, Lilley and Singer were still among the official delegates, but now joined by Crombie, Dingle, Feyer and Sherwood Taylor. 'Proceedings of meetings', *Bulletin for the British Society for the History of Science* (1951), **1**, 112–4, 113.

92 'Report from Council Meeting 26th October 1948', CDS, A.55.

93 Both quotations from 'Report from Council Meeting 24th October 1949', CDS, A.55.

94 Browne, op. cit. (89), 80.

discipline of history of science is a striking contrast to the internal devaluation of his work by the internalists within the University of Cambridge.

Standing on the shoulders of Zilsel

Lilley was clearly judged controversial by leading figures within British history of science. This tension became even more evident in the early 1950s through Lilley's criticism of and confrontation with Crombie in Oxford and Butterfield in Cambridge. Crombie had been Lilley's competitor for the position at the Whipple Museum and later criticized the sociological thesis of Lilley in his review of *The History of Science* (1951). In the early 1950s Crombie was about to launch his continuity thesis, outlined both in *From Augustine to Galileo* (1952) and in *Robert Grosseteste and the Origins of Experimental Science* (1953).[95] Lilley gave the 1952 book a devastating critique in the journal *Discovery*. In particular, he singled out the unsatisfactory leap in Crombie's alleged continuity between the fourteenth and sixteenth centuries and emphasized the fundamental difference between the abstract methodologies of the medievals and the application of experimental practices by the moderns. Further, Lilley insisted upon the importance of this intervening period, especially that the 'habitual use of experiment grew up in the late 15th and 16th centuries among superior craftsmen given new opportunities by social conditions of the times'.[96] By bringing in these other social strata, the superior craftsmen, Lilley was able to develop a continuity thesis where Crombie had failed to do so. Crombie was unsurprisingly furious and posted a letter to the editor of *Discovery* criticizing Lilley's review for being 'in some respects so perverse that I feel bound to correct the false impression'. In particular he attacked Lilley's externalism, which, according to Crombie, consisted of 'nothing but the play of sociological forces'.[97] The Lilley–Crombie controversy in 1953 continued in the next volume of *Discovery*. Lilley replied to Crombie's criticism by clarifying his own rather careful claim combining internalism and externalism. Both the craftsmen's role and the scholars' methodological theory had to be taken into account: 'I believe that the history of science can only be understood by considering the *inter*play of both internal scientific development and the influence of ambient social change.'[98] On the very next page of *Discovery*, Lilley delivered an equally ruthless critique of *Robert Grosseteste and the Origins of Experimental Science* (1953) by pointing at Crombie's unsatisfactory treatment of 'the practice of progressive Renaissance artists and engineers' and the 'social conditions unique to that period'.[99]

95 B. S. Eastwood, 'On the continuity of western science from the Middle Ages: A. C. Crombie's *Augustine to Galileo*', *Isis* (1992), **83**, 84–99.

96 S. Lilley, 'Review: *Augustine to Galileo: The History of Science A.D. 400–1640* by A. C. Crombie', *Discovery* (1953), **14**, 163, cont. 165.

97 A. C. Crombie, 'Letter to the editor: a book review criticised', *Discovery* (1953), **14**, 195–6.

98 S. Lilley, 'Letter to the editor: Dr. Lilley's reply to Dr. Crombie's criticisms', *Discovery* (1953), **14**, 228.

99 S. Lilley, 'Review: *Robert Grosseteste and the Origins of Experimental Science 1100–1700*, *Discovery* (1953), **14**, 229.

Perhaps even more devastating was Lilley's review of Butterfield's *The Origins of Modern Science* (1949) published in the Marxist journal *Modern Quarterly* in 1952. Here Lilley mocked Butterfield's description of the scientific revolution as 'putting on a different kind of thinking cap' by asking rhetorically where this new attitude, this new thinking cap, came from. According to Lilley the answer was easily available: 'A coming together of craftsman and scholar, leading to a combination of the craftsman's empirical attitude with the scholar's theoretical way of thinking, was one of the chief sources of the method of modern science.'[100] Butterfield, on the other hand, had simply invented a 'new idealist theory of history', Lilley argued, and the 'extreme danger' of this theory consisted in concealing the important role played by the bourgeois revolution in launching the so-called scientific revolution:

> But enough has been said to show that there is really nothing mysterious about the new 'thinking cap'. It was spun, woven, cut and stitched by the bourgeoisie and their allies. It is only when one looks for origins exclusively in the field of pure thought that the 'thinking cap' appears to be without antecedents.[101]

In Lilley's critique of Crombie and Butterfield it became apparent that his analytical approach was derived not so much from Hessen or Merton, as rather from Edgar Zilsel. During the 1940s Zilsel had published a series of essays on the sociological roots of science, focusing in particular on superior craftsmen and artist–engineers such as Brunelleschi, Ghiberti, Alberti, Dürer and Leonardo da Vinci.[102] According to Lilley, Zilsel's work was perhaps the most important contribution in recent years to the understanding of the origins of modern science: 'It is remarkable that Butterfield makes no reference to it and, so far as I can see, makes no use of it, though it was all published by 1945.'[103]

Zilsel was the point of departure for Lilley's paper on Leonardo da Vinci presented at a four-hundredth anniversary conference in Italy in 1953.[104] Yet his most detailed discussion of the scholar–craftsman thesis is to be found in his 1958 paper on Robert Recorde (1510–58). Here Lilley was not so much concerned with skills or craftsmanship as rather with the origin of the notion of scientific progress as outlined by Zilsel in his

100 S. Lilley, 'Review: *The Origins of Modern Science: 1300–1800* by Herbert Butterfield', *Modern Quarterly* (1952), **7**, 101–11, 105, 108.

101 Lilley, op. cit. (100), 110.

102 E. Zilsel, 'The sociological roots of science', *American Journal of Sociology* (1942), **47**, 544–62. S. Shapin, 'Zilsel thesis', in *Dictionary of the History of Science* (ed. W. F. Bynum, E. J. Browne and R. Porter), London, 1981, 450. N. Jardine, 'Zilsel's dilemma', *Annals of Science* (2003), **60**, 85–94.

103 Lilley, op. cit. (100), 109, n. 1.

104 S. Lilley, 'Leonardo da Vinci and the experimental method', in *Atti del Convegno di Studi Vinciani* (ed. L. S. Olschki), Academia Toscana di Scienze e lettere *La Colombaria*, Florence, 1953, 401–20. See also Charles Singer to Sam Lilley, 27 February 1956, CDS, A.11, 5: 'I like your article very much and I think you have treated the medieval enthusiasts very fairly. The situation seems to me fairly plain and I cannot understand why Crombie cannot be made to understand it. Neither in the 13th century nor in the 20th can discussion of the nature of science be equated with science. You cannot get science until you have men who devote their lives to observation and/or experiment. When the medievalists have succeeded in finding such a one in the Middle Ages, I will accept their claim. But you have put all these matters much more efficiently than I.'

1945 paper on 'The genesis of the concept of scientific progress'.[105] According to Zilsel, the idea of progress was first developed among the artist–engineers and only later transmitted from the crafts to scholars such as Gilbert, Galileo and Bacon. This new outlook – described by Lilley as 'the contribution model' – was characterized by 'the recognition that knowledge is a cumulative store which is built up by a succession of contributions by individuals'.[106]

Nonetheless, Lilley criticized Zilsel on two points. First, Zilsel's sociological approach did not give sufficient credit to 'certain largely internal scholastic trends'. Lilley thus presented his own modified version of the Zilsel thesis, attempting 'seriously to synthesize the two views and to show in what ways the empiricism of the practical men interacted with the methodological strivings of the scholars'.[107] Second, he criticized Zilsel for failing to take into account the practical implications of the idea of progress. The idea itself, Lilley acknowledged, could very well be found elsewhere and even much earlier, as in the work of Bodin and Le Roy. However, their concern was merely theoretical, as opposed to the practical concerns in the writings of Dürer or Paré. Here the general tone was imperative, not merely descriptive. Lilley explained: 'Progress is not a theory to be proved or disproved, but a duty to be carried out.'[108]

Lilley then put his own modified version of the Zilsel thesis to the test in the case of Robert Recorde, a representative of a group of practically minded Elizabethan mathematicians including John Dee, Thomas Digges, Thomas Hood and Edward Wright.[109] Recorde published a series of books in English introducing science and mathematics, such as *The Grounde of Artes* (1540), *The Pathway to Knowledge* (1551) and *The Whetstone of Whitte* (1557). Lilley was attempting to locate the first emergence in Recorde's writings of the idea of progress. He found it in the preface of *The Pathway to Knowledge*. Crucially, this book was written at the time when Recorde had left the universities and was engaged in the practical problems of craftsmen. Only when moving outside the colleges, Lilley explained, was Recorde able to absorb a craftsman's attitude towards the advancement of his art.[110] Lilley concluded that the prediction was strikingly confirmed: 'To that extent Zilsel's hypothesis is also verified, though not, of course, proved with mathematical certainty.'[111]

In his study of Recorde from 1958 Lilley was also *en passant* contributing to what Robert K. Merton has eloquently elaborated as 'otsogian historiography' in his book on the history of the aphorism, *On the Shoulders of Giants* (1965): 'Truly Recorde was a solid support in the bottom rank of that pyramid of giants on whose shoulders

105 E. Zilsel, 'The genesis of the concept of scientific progress', *Journal for the History of Ideas* (1945), **6**, 325–49.

106 S. Lilley, 'Robert Recorde and the idea of progress: a hypothesis and a verification', *Renaissance and Modern Studies* (1958) **2**, 3–37, 6.

107 For Lilley's careful modification of the Zilsel thesis see Lilley, op. cit. (106), 4–19.

108 Lilley, op. cit. (106), 15.

109 For further details on Recorde, yet with no reference to Lilley, see J. Williams, 'Mathematics and the alloying of coinage 1202–1700: Part II', *Annals of Science* (1995), **52**, 213–63, 240; and S. Johnston, 'Recorde, Robert (c.1512–58)', *ODNB*, Oxford, 2004.

110 Lilley, op. cit. (106), 26.

111 Lilley, op. cit. (106), 29.

Newton knew himself to be standing.'[112] Yet there is nothing in Lilley's paper suggesting that Recorde actually presented his own explicit version of the aphorism. Rather, his metaphor for the contribution model in the *The Pathway to Knowledge* was embodied in the self-imposed task of making mathematics available to practical men, an invitation to future giants to continue the work by doing better in the future: 'I drew this platte [plan] rudelie, whereon thei maie builde, whom god hath induced with learning and livelihood.'[113]

My claim is, nevertheless, that Lilley anticipated Merton and the whole idea of writing a historiography of the 'on-the-shoulders-of-giants' (OTSOG) aphorism in his 1958 paper on Robert Recorde. Merton's 1965 book was at best 'an essentially independent, though somewhat belated, discovery'.[114] Everyone knows, of course, that fascination with this saying goes back to George Sarton, Merton's mentor in the Widener Library at Harvard back in the 1930s. Everybody knows this because Merton referred to Sarton's 1935 remark when he himself first mentioned the aphorism in 1942 in formulating the ethos of science: 'See *Isis*, 1935, 24, 107–9; 1938, 25, 451–2.'[115] In Sarton's 'Query no. 53. – "Standing on the shoulders of giants"' from 1935 we further learn how the Belgian historian first referred to the use of the aphorism by Bernhard of Chartres and John of Salisbury in *The History of Science and the New Humanism* (1931).[116] And later, according to Merton, his first reference to Sarton's 1935 formulation was reprinted without further comment in the 1949 version of the 1942 paper. In fact, it remains unchanged both in later versions of the ethos of science and also in the 'Post-Italianate Edition' of *On the Shoulders of Giants* of 1993.[117]

The sad truth is that Merton's full citation is painfully wrong. This could easily have been spotted by a simple comparison of the respective volumes of *Isis* ('1935, 24' and '1938, 25'). Since no work on the aphorism is present in the 1938 volume of *Isis* it is obvious that the correct reference should have been to Volume 25 from 1936, where a note by R. E. Ockenden on the aphorism is printed on pages 451–2. We also know Merton was wrong because Lilley independently got it right when referring to these *Isis* notes in his 1958 paper on Recorde and the idea of progress: '1935, XXIV, 107–9; 1936, XXV, 451–2', before, ironically, making a similar mistake in his reference to Klibansky's 1936 paper, '1937, XXVI, 147–9' (the problem being, of course, that these

112 Lilley, op. cit. (106), 31.

113 From the introduction 'To the gentle reader', in Recorde's *The Pathway to Knowledge*, as quoted in Lilley, op. cit. (106), 28.

114 R. K. Merton, *On the Shoulders of Giants: A Shandean Postscript. The Post-Italianate Edition* (1965), Chicago, 1993, 25.

115 R. K. Merton, 'A note on science and democracy', *Journal of Legal and Political Sociology* (1942), **1**, 115–26, 123; *idem*, op. cit. (114), 1.

116 G. Sarton, 'Query no. 53. – "Standing on the shoulders of giants"', *Isis* (1935), **24**, 107–9; *idem*, *The History of Science and the New Humanism*, New York, 1931, 30. See also Sarton on the use of aphorism by Bernhard of Chartres and John of Salisbury, in *Introduction to the History of Science*, *Vol. 2: From Rabbi Ben Ezra to Roger Bacon*, Baltimore, 1931, 195–6.

117 Merton, op. cit. (114), 2.

earlier volumes of *Isis* did not always appear annually, so Volume 26 also appeared in 1936).[118]

In a footnote to his 1958 paper on Recorde, Lilley formulated the historiographical task in question: 'A continuous history of this idea is yet to be written.'[119] From the first pages his analysis of Recorde is intimately interwoven with both the history of the OTSOG aphorism and the 'Battle of Ancient and Moderns'. Lilley even outlined the history from Sarton, beginning with Bernard of Chartres and John of Salisbury, yet he moved rapidly towards more modern formulations in the fourteenth century by the French surgeons and practitioners such as Henry de Mondeville and Guy de Chauliac. We learn from Lilley that Chauliac was first mentioned by Ockenden in 1936, while Mondeville's version of the aphorism first appeared in the long-awaited third volume of Sarton's *Introduction to the History of Science* (1947), reviewed by Lilley in *Nature* in 1950.[120] Later, in Merton's *On the Shoulders of Giants* both Mondeville and Chauliac become central to the history of the aphorism, based on exactly the same citations as Lilley had used, both to Ockenden in 1936 and to Sarton in 1947. Only here Merton got the reference to Ockenden right, yet without correcting the other citation that was still embarrassingly present in the book.[121] 'Why does Sarton take no notice of Guy's use of the Aphorism?' Merton asked rather cryptically. The simple answer is that Lilley did. In fact, Guy de Chauliac was at the core of his whole argument and crucial in his careful modification of Zilsel's thesis: 'Guy de Chauliac, writing about 1363, changed the metaphor from "dwarfs" to "children" – with a subtle implication about future developments [that we shall grow up] – and combined it with some approach to the "contribution" idea.'[122]

This does not prove 'with mathematical certainty' that Merton actually read Lilley's 1958 paper on Recorde, although it was widely known, as it had been listed in *Isis* in 1959.[123] It might just be that Merton's book on the OTSOG aphorism from 1965 was simply an example of serendipity.[124] Nevertheless, Merton had missed Lilley's major sociological point in locating the continual use of the aphorism, with knowledge being passed on and accumulated from master to apprentice, among the social strata of surgeons like Mondeville, Chauliac and Ambroise Paré. More precisely, Lilley first located the progressive and practically oriented imperative of the contribution model embodied in the aphorism in the writings of Paré, in an added passage to the preface 'Au Lecteur' in the second edition of *Oeuvres complètes d'Ambroise Paré* (1579) published only four years after the first edition: 'We have learned from the good father Guidon [Lat.: Guy de Chauliac] that we are like a child on the neck of a giant: that is to say, that by their

118 Lilley, op. cit. (106), 32, n. 11. Cf. R. Kilbansky, 'Answer to Query no 53. – Standing on the shoulders of giants (*Isis* 24, 107–9, 1935)', *Isis* (1936), **26**, 147–9.

119 Lilley, op. cit. (106), 32, n. 11.

120 S. Lilley, 'Review of Sarton (1948) *Science and Learning in the Fourteenth Century*', *Nature* (1950), **165**, 128.

121 Merton, op. cit. (114), 223–8.

122 Lilley, op. cit. (106), 8, 15.

123 I. B. Cohen, H. Woolf and P. B. Bosson, 'Eighty-fourth critical bibliography of the history of science and its cultural influences (to 1 January 1959)', *Isis* (1959), **50**, 289–407, 347.

124 R. K. Merton and E. Barber, *The Travels and Adventures of Serendipity*, Princeton, 2004.

writings we see what they saw, and can moreover see and understand more.'[125] With this crucial version of the aphorism in Paré, sometime between 1575 and 1579, Lilley could conclude his quest by noting how 'the craftsman's approach to the idea of progress had virtually reached completion'.[126]

In Merton's 1965 version of the history of the OTSOG aphorism the contribution of Paré is completely missing. This is even more remarkable since Paré was a contemporary of Didacus Stella (Diego de Estella), the crucial character in otsogian historiography to whom both Sarton and Merton persistently referred both as the start of the whole inquiry (based on Robert Burton's cryptic citation: 'Didactus Stella, in Luc. 10, tom.2') and as the first to present the modern version of the aphorism later ascribed to Newton.[127] Contrary to the innovation ascribed to Burton by both Sarton and Merton, Lilley suggested that Paré and the practitioners within the surgical tradition, 'carrying a constant battle against those who wanted to separate the head and the hand', represented a real continuity in the promotion of the idea of progress as embodied in the modern version of the aphorism: 'It may be that when this separation [of head and hand] took place the "shoulders of giants" metaphor was forgotten among scholars, but carried forward among the surgeons.'[128] Between the fourteenth and the sixteenth centuries, Lilley concluded, the practical precept of the cumulative contribution model first emerged among craftsmen. Thus Lilley suggested two sociologically distinct lines of otsogian decent: one, among the scholars within the universities, of radical discontinuity; the other, continuously cultivated among surgeons and skilled craftsmen outside in the wider society. What in theory appeared as a gap was in fact bridged and promoted in practice. Yet when it comes to the historiography of the aphorism usually ascribed to Merton, Lilley has been bypassed and overlooked. His contribution to otsogian historiography could be summed up in yet another version of the aphorism formulated by Marchamont Needham, Joseph Needham's predecessor, in 1665: 'others have walked afoot … or have been laid by the heels'.[129]

Hessen revisited

At the Second International Congress for the History of Science and Technology at the Science Museum in London in 1931, Nikolai Bukharin presented his version of the aphorism, tracing it back to Friedrich Engels and *The Peasant War in Germany* (1874): 'Just as German theoretical socialism will never forget that it rests on the shoulders of Saint-Simon, Fourier and Owen … so the practical workers' movement in Germany

125 Lilley, op. cit. (106), 15.

126 Lilley, op. cit. (106), 15.

127 Merton's subtle point – contrary to Lilley's reading of Diego di Estella as introducing his own generation as 'dwarfs' – was the following: 'Robert Burton quoted out of context and, in a most literal sense, actually misquoted Didactus' version of the Aphorism.' According to Merton, 'Didactus aligns himself on the side of the moderns'. Cf. Lilley, op. cit. (106), 33–4, n. 31; Merton, op. cit. (114), 223–8, 254, 257.

128 Lilley, op. cit. (106), 33, n. 31.

129 Quoted from Merton, op. cit. (114), 86. On Needham's genealogy see G. Werskey, 'The visible college: a study of left-wing scientists in britain, 1918–1939', Ph.D. in history, Harvard University, Cambridge, MA (HU 90.10680.10), 1973, 74–5.

must never forget that it has developed on the shoulders of the English and French movements.'[130] Yet the 1931 congress is best known for the presentation by Boris Hessen on 'The social and economical roots of Newton's *Principia*', published together with the rest of the Russian delegation in *Science at the Cross Roads* (1931). Their contribution has been described as the 'single event that most shaped the study of the history of science in the 20th century'. The congress is regarded by Werskey as the precise moment when the political vision of *The Visible College* was dramatically improved: 'It was at the Science Museum, South Kensington, on Saturday morning, 4 July 1931.'[131] Interestingly, Lilley's first ever publication, in *Nature*, on 'Mathematical machines', was a direct application of Hessen's Marxist methodology to emerging computing technologies.[132] However, this Marxist tradition was allegedly withering away by the time Hall concluded that 'the trend towards intellectual history is strong and universal'.

Hall's argument for radical discontinuity in 'Merton revisited' in 1963 is seriously challenged by the fact that Lilley fought back with his version of 'Hessen revisited' in 1964. His counterblast was launched on 6 March 1964, most probably at Maurice Dobb's rooms in Trinity College where the senior branch of the Cambridge University Communist Party used to gather. To the best of my knowledge it was never published, but luckily Needham wrote eight pages of notes from Lilley's talk under the heading 'Revaluation of the Hessen thesis'.[133] Here Lilley outlined the history of the Marxist tradition within the history of science since the 1931 congress, including Hessen's analysis of Newton, the subsequent critique by Clark and the alternative approach developed both by Merton and by Zilsel. When revisiting Hessen some thirty-three years later, Lilley criticized him for insufficient appreciation of the intrinsic logic of science and also pointed out that Hessen 'made no use of previous science'. Against this, Lilley sketched out his own modified version of the continuity thesis, emphasizing how early capitalism brought scholars and craftsmen together, thereby bridging the mental–manual antagonism. After the subsequent discussion, in his ten-minute closing remarks, Lilley finally addressed the more recent critique proposed by Hall in 'Merton revisited'. He explicitly referred to the above-mentioned passages: how the trend towards intellectual history was overwhelming and that no externalist papers had

130 As quoted in Merton, op. cit. (114), 264–5.

131 R. Olwell, '"Condemned to Footnotes": Marxist scholarship in the history of science', *Science and Society* (1996), **60**, 7–26, 10. Werskey, op. cit. (39), 138. For more recent revaluations of Hessen see G. Freudenthal, 'The Hessen–Grossman thesis: an attempt at rehabilitation', *Perspectives on Science* (2005), **13**, 166–93. C. A. J. Chilvers, 'La Signification historique de Boris Hessen', in *Le Racines sociales et économiques de* Principia *de Newton* (ed. S. Guerout), Paris, 2006, 179–206. *idem*, 'The tragedy of Comrade Hessen: biography as historical discourse', in *The History and Poetics of Scientific Biography* (ed. T. Söderquist), London, 2007, 105–20.

132 Lilley, op. cit. (20), 462.

133 My thanks to Anna-K. Mayer for informing me about this important source at NRI SCC2, 347, 1, 16. Needham's notes are marked 'chez M Dobb' – most probably referring to Maurice Dobb, the Marxist economist at Trinity College in Cambridge whom Lilley had known since the 1930s. According to Brian Pollitt, who was the literary executor of Dobb's papers and who still possesses Dobb's diaries, there is indeed a single entry in Dobb's Cambridge University Pocket Diary, 1963–4, on 6 March 1964, stating 'Sam Lilley'. Communication with B. Pollitt, 28 March 2007.

appeared in the leading journals in history of science since Lilley's 1953 volume of *Centaurus*. Then he pointed out that 'this is a purely political suppression'. Lilley's concluding comment was nevertheless more optimistic: 'there'll be a comeback'.[134]

Lilley did not have to wait long for a more organized response. The initiative came from Robert S. Cohen at Harvard. In 1955 he had published a historiographical article deploring 'the extraordinary lack of detailed studies in the history of the social relations of science' while at the same time referring to Lilley's *Centaurus* volume as a rare exception.[135] Cohen also remarked on the tragic death of Edgar Zilsel and pointed out that it would be helpful 'if Zilsel's essays were brought together in one volume, but yet no publisher has been found for such a project'.[136] Almost ten years later Cohen still regarded Lilley as a possible contributor to a collection of Zilsel essays. He wrote to Needham in June 1964 that he would 'like to offer a critical commentary on Edgar Zilsel's work. There must be many more who could be invited. Perhaps we could ask those who organized that fine issue of *Centaurus* to help us? It was Lilley who did that issue, wasn't it?'[137] By this time, however, the possible revival of Zilsel was being readdressed as a direct response to Hall and 'Merton revisited'. A few months earlier, in March 1964, Cohen had written to Needham about the declining status of historical studies of the social relations of science and urged Needham to participate in a critical symposium at the forthcoming Eleventh International Congress for the History of Science in Warsaw and Cracow in 1965.[138] Needham responded quickly, stressing the need for such a symposium: 'Nothing could be more useful than a set-to on the historical sociology of science. Perhaps you have seen A. R. Hall's "Merton Revisited" in *History of Science*, vol. 2. It is a cannonade against the sociological interpretation of the origins of modern science.' In this context Needham was curious about Cohen's earlier commitment to Zilsel: 'What happened to the collected edition of all Zilsel's papers? Weren't you much occupied with that at one time?' And Cohen's revival of Zilsel was directly related to Lilley's recent revaluation of Hessen: 'It is greatly needed, indeed now more than ever. Just a day or two ago Sam Lilley was lecturing on "Hessen Revisited".'[139]

Cohen's symposium, designed to confront Hall's denunciation of social history of science, was gradually coming together for summer 1964. By June the list of tentative participants included the following names in addition to Robert S. Cohen: D. Struik, J. Needham, M. Minnaert, S. Lilley, B. Farrington, E. Kolman, L. Rosenfeld,

134 S. Lilley, as quoted in Needham's notes, op. cit. (133).

135 R. S. Cohen, 'Alternative interpretations of the history of science', *Scientific Monthly* (1955), **80**, 111–16, 112.

136 Cohen, op. cit. (135), 115, n. 2.

137 Robert S. Cohen to Joseph Needham, 15 June 1964, NRI, SCC2, 347, 1, 15. More recently, the collection and critical commentary appeared as E. Zilsel, *The Social Origins of Modern Science* (ed. D. Raven, W. Krohn and R. S. Cohen), Boston Studies in the Philosophy of Science 200, Boston, 2000. In Cohen's introduction to the volume he explains how his early and extensive knowledge of Zilsel was due to the fact that Zilsel's son Paul was Cohen's friend and fellow physics graduate student at Yale where they both received doctorates in 1948.

138 Robert S. Cohen to Joseph Needham, 3 March 1964, NRI, SCC2, 347, 1, 15.

139 Joseph Needham to Robert S. Cohen, 8 March 1964, NRI, SCC2, 347, 1, 15.

J. D. Bernal, J. Fayet, R. K. Merton, S. F. Mason, L. Mumford, L. White, L. L. White, H. Lay, B. Rosenfeld and D. D. Kosambi.[140] Cohen also corresponded directly with Lilley, who suggested he might present a paper on the theory of evolution of Erasmus Darwin: 'This is a topic I can document well – in fact, at much greater length than a Congress contribution would permit.'[141] To the best of my knowledge, this symposium never materialized, and at the Congress in Poland Lilley presented his paper on Erasmus Darwin in an entirely different context.[142]

Contrary to Hall's claim in 'Merton revisited', the social history of science was certainly vital and active in 1965. Cohen was already revisiting Zilsel, while Lilley quickly responded to Hall's revision with 'Hessen revisited'. A few years later, in 1971, Hessen certainly experienced a revival of interest on the occasion of the fortieth anniversary of the 1931 conference and *Science at the Cross Roads*. Cohen wrote the new introduction to the American re-publication of Hessen's essay on 'The social and economic roots of Newton's *Principia*' in 1971, while Werskey wrote the new introduction to *Science at the Cross Roads*, also published in 1971. Interestingly, it was Cohen, with Everett Mendelsohn and Stuart Hughes, who aided Werskey in his study of *The Visible College*.[143] In his introduction to *Science at the Cross Roads*, Werskey listed Lilley among the 'key figures', along with Bernal, Childe, Crowther, Farrington, Hill, Hobsbawm, Hogben and Needham, who in the aftermath of the 1931 congress attempted to develop the Marxist approach to the history of science.[144] Although Lilley has usually been omitted from discussions of *The Visible College* since 1978, he was in fact included by Werskey in his 1971 introduction to *Science at the Cross Roads*.

Lilley's legacy

By revisiting Lilley I argue for a much more continuous interpretation of the Marxist tradition within history of science. The argument hinges reflexively on Lilley's contribution model, suggesting the existence of a real continuity within the practical

140 Robert S. Cohen to Joseph Needham, 15 June 1964, NRI, SCC2, 347, 1, 15.

141 Sam Lilley to Robert S. Cohen, 1 August 1964, NRI, SCC2, 347, 1, 24.

142 Cf. *Actes du XIe Congrès International d'Historie des Sciences* (1968), op. cit. (79).

143 Werskey, op. cit. (129), p. vi. Cohen wrote the letter of introduction to Needham in 1968 presenting Werskey as a doctorate in the history of ideas at Harvard, 'interested in the development of Marxist and other related or similar interpretations and movements about the social relativity of science, which came about during the 1930's'. Robert S. Cohen to Joseph Needham, 25 March 1968, Needham Papers, CUL, A.700.

144 G. Werskey, 'New introduction', *Science at the Cross Roads* (ed. N. Bukharin *et al.*), London, 1931 (2nd edn 1971), pp. xi–xxix, xxiii. See also H. Sheehan, *Marxism and the Philosophy of Science: A Critical History. The First Hundred Years*, Atlantic Highlands, NJ, 1993, 303: 'Others [adding to Bernal and Haldane] who formed a part of the radical science movement of the 1930s were P. M. S. Blackett, E. H. S. Burhop, J. G. Crowther, Hyman Levy, Sam Lilley, Joseph Needham, N. W. Pirie, C. H. Waddington, W. A. Wooster, and Lancelot Hogben.' Similarly in P. Rossi, 'Craftsman-and-scholar thesis', in *Encyclopedia of the Scientific Revolution from Copernicus to Newton* (ed. W. Applebaum), New York and London, 2000, 174–7, 175: 'To J. D. Bernal, J. S. Haldane, L. Hogben, J. Needham, V. G. Childe, B. Farrington, S. Lilley, and C. Hill, the book [*Science at the Crossroads*] appeared as the starting point for a new interpretation of the history of science.'

tradition, despite the alleged discontinuity inside the scholarly tradition.[145] This Marxist continuity was clearly not to be transmitted in any of the leading journals, nor within the established centres of learning, be it in Oxford or in Cambridge, but was instead nurtured among other groups and organized commissions and conferences such as the international congresses for the history of science.[146]

At the same time, the early 1960s was a turbulent period for Lilley personally, because he was divorced and left by his wife and children. He also apparently gave up applications for formal positions in the history of science. Few professional papers appeared after 1960, with the obvious exception of his substantial contribution on the Industrial Revolution in *The Fontana Economic History of Europe* (1973).[147] By 1965 he had completed a revised and enlarged edition of *Men, Machines and History*, also published in the United States in 1966, and later he wrote a widely read introduction to relativity theory called *Discovering Relativity for Yourself* (1981).[148] Nevertheless, from the early 1960s Lilley's main contribution to the social history of science was in the popular and political press.

Lilley remained a member of the Communist Party until his death and was actively engaged in the Science Sub-committee throughout the 1960s and 1970s. He kept publishing material on history of science, as in the journal *Marxism Today* and in the communist weekly review *Comment*.[149] Lilley was also central in launching the *Science Bulletin*, a communist journal of Marxism and science which appeared in the spring of 1972. At the first national aggregate meeting of communist scientists and technologists at Marx House in London in June 1972, Lilley gave the opening lecture on 'Science and society and the struggle of socialism'. This time, however, he spoke mainly about the environmental problem, deploring the fact that Barry Commoner, author of the eco-socialist *The Closing Circle: Nature, Man, and Technology* (1971), had already formulated a political platform long before the Communist Party had even begun to address the problem.[150]

Lilley's loyalty to the Old Left raises an interesting question concerning the marginalization of Lilley not only by Hall, but also within the Left and especially among

145 I owe this reflexive application of the continuity thesis to Simon Schaffer.

146 This argument will be further elaborated in a separate publication on Lilley's role as secretary for the Commission for the History of the Social Relations of Science. Cf. R. MacLeod, 'The historical context of the International Council for Science Policy Studies', *Archives internationales d'historie des sciences* (1975), **25**, 314–28. Also P. Petitjean, 'The joint establishment of the World Federation of Scientific Workers and of UNESCO after World War II', *Minerva* (2008), **46**, 247–70.

147 S. Lilley, 'Technological progress and the Industrial Revolution 1700–1914', in *The Fontana Economic History of Europe*, Vol 3: *The Industrial Revolution* (ed. C. M. Cipolla), London, 1973, 187–254.

148 S. Lilley, *Men, Machines and History*, London, 1965; *idem*, *Men, Machines and History*, New York, 1966; *idem*, *Discovering Relativity for Yourself*, Cambridge, 1981.

149 S. Lilley, 'Higher education in the nuclear age', *Marxism Today* (1959), **3**, 1–10; *idem*, 'Galileo and the scientific method', *Marxism Today* (1965), **9**, 276–82; *idem*, 'Science, technology and socialism', *Marxism Today* (1966), **10**, 230–6; *idem*, 'Marxism and science', *Marxism Today* (1969), **13**, 347–51; *idem*, 'The technological revolution that faces us', *Comment: A Communist Weekly Review* (1965), **3**, 599–602; *idem*, 'Science, society and socialism', *Comment: A Communist Weekly Review* (1972), **10**, 235–8.

150 The event was reported in *Science Bulletin*, 3 (1972), 3–4. Lilley's talk was published in *Comment* (1972) **10** and discussed in subsequent volumes by R. Press, R. M. West and F. Stewart.

the New Left. It has been pointed out by Jerry Ravetz that the main weakness of Bernal's monumental *Science in History* (1954) was his systematic neglect of post-war Marxist contributions in the history of science developed for example by Mason and by Lilley.[151] Clearly, Lilley was not only marginalized by the internalists, but also to some extent made invisible or irrelevant within leftist historiography and history of science.[152] A poignant example is Bob Young and his contribution to the Needham Festschrift *Changing Perspectives in the History of Science* (1973). Here Young presents a rather sympathetic reading of 'Merton revisited', while pointing out that both Merton and Lilley were in fact being criticized by Hall: '[Hall's] analysis of Merton's work is interrupted by comments on the Marxist historian S. Lilley, without any indication that there is a fundamental distinction between their approaches.'[153] Yet I must disagree with Young's dismissal of earlier attempts to develop a Marxist approach to the history of science as being reducible to 'vulgar Marxism' or to the reductionism of the Marxist 'base–superstructure' model. By too easily accepting Hall's attack on Lilley and the Old Left as extreme externalists, Young in his defence of the New Left as the only valid point of departure for developing a radical historiography of science clearly reproduced clichés about Lilley and the previous generation of Marxist historians of science. In fact, Young's plea to set aside the internalist–externalist dichotomy and transcend the reductionism of the Marxist base–superstructure model is much better as a description of Lilley's project than as a criticism of it. For instance, in his contribution to *Centaurus* in 1953 Lilley concluded that the time had 'come therefore to stop arguing about the relative importance of internal and external influences, and instead to approach the causation of scientific development as a unity in which both aspects are studied, and studied in their mutual interaction'.[154] Crucially, when arguing for the continuity of Lilley's legacy, I am not referring to the cliché of Marxist reductionism, or to the extreme externalism attributed to Lilley both by Hall and by Young, but rather to a more moderate Marxism insisting on the mutual interaction of internal and external influences on science. This alternative Marxist model was elaborated, for instance, in Lilley's controversy with Crombie in *Discovery* in 1953 and even more interestingly in his critique of the sociology of Zilsel and the Marxism of Hessen.

Lilley's Marxist approach was remarkably well received in the United States, especially among the few participants in the emerging sociology of science. For instance, in the foreword to Bernard Barber's book *Science and the Social Order* (1952), Robert K. Merton began by giving credit to the British radicals such as Bernal, Crowther, Farrington, Pledge and Hogben who had all made important contributions to the debate on science and society, before emphasizing Lilley's particular position: 'But these, with the important exception of Lilley's "Social Aspects of the History of

151 J. Ravetz, 'Bernal's Marxist vision of history', *Isis* (1981), **72**, 393–402, 396, n. 12.

152 For a similar argument see the recent paper by H. Sheenan, 'Marxism and science studies: a sweep through the decades', *International Studies in the Philosophy of Science* (2007), **21**, 197–210, 201: 'There was residual anti-communism, as well as generational rebellion in the US new left's attitude to the old left.'

153 R. Young, 'The historiographic and ideological contexts of the nineteenth-century debate on man's place in nature', in *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham* (ed. M. Teich and R. Young), London, 1973, 344–438, 418.

154 Lilley, 'Cause and effect', op. cit. (5), 71.

Science,'' have not examined the linkage between science and social structure by means of a conceptual framework that has proved effective in other branches of sociology.' Merton further pointed to the role of the Commission for the History of the Social Relations of Science and how 'its important first report, prepared by S. Lilley, gives ample proofs of its sociological orientation'.[155] Similarly, Barber makes numerous references to Lilley, and his description of the historical development of science was to a large extent based on *Men, Machines and History*.[156] Barber also singled out Lilley in his trend report of the sociology of science from 1956. Here the other British radicals were written off as important but insufficient predecessors, while Lilley's work was accepted as 'academic sociology':

> Lilley being still a Marxist in his analysis of the social relations of science, published *Men, Machines and History*, which contained much good material. A year later, he had abandoned his moderate Marxist inclinations in his excellent essay, ''Social aspects of the history of science'', where he combined original historical work with a critical survey of other men's writings in the sociology of science.[157]

This close affiliation between Lilley in the United Kingdom and both Merton and Barber in the United States might explain why Hall wrote as he did in 'Merton revisited'.

With the emergence of the new history and sociology of science in the early 1970s, there are further traces of Lilley's continuous legacy.[158] For instance, in their paper on prosopography from 1974, Steven Shapin and Arnold Thackray referred to Lilley's *Centaurus* paper as an early expression of how 'dissatisfaction with the [internal/external] dichotomy has been building for some time'.[159] In 1976 Ron Johnston pointed towards both Lilley and Thackray, 'who both argue for a combination of the two approaches which will avoid the extremes of both pure history of ideas and vulgar Marxism'.[160] And more importantly, Shapin highlighted the tension between Hall and Lilley in his 1992 article on the historiography of the internalism–externalism debate: 'From 1948 to 1953 the English Marxist engineer Sam Lilley used a popular science brief from UNESCO to publicize his scientific externalism as a search for the universal causal laws of scientific development.'[161] Shapin correctly pointed out that 'the reputed externalist Sam Lilley echoed Mertonian sentiments', yet contrary to Hall's critique of the alleged extreme externalism Shapin acknowledged the eclecticism of both Merton

155 R. K. Merton, 'Foreword', in B. Barber, *Science and Social Order*, Glencoe, IL, 1952, pp. xi–xxiii, xi and xxi–xxii.

156 Barber, op. cit. (155), 264–6. For further references to Lilley, cf. notes 6, 7, 8, 10, 11, 23, 33, 44, 45.

157 B. Barber, 'Sociology of science: a trend report and bibliography', *Current Sociology* (1956), **5**, 91–153, 99.

158 S. Dedijer, 'The sociology of science: a program and a plea', *Minerva* (1966), **4**, 489–504. R. G. A. Dolby, 'Sociology of knowledge in natural science', *Science Studies* (1971), **1**, 3–21.

159 S. Shapin and A. Thackray, 'Prosopography as a research tool in history of science: the British scientific community 1700–1900', *History of Science* (1974), **12**, 1–28, 22, n. 9.

160 R. Johnston, 'Contextual knowledge', *Australian and New Zealand Journal of Sociology* (1976), **12**, 193–203, 201, n. 5.

161 S. Shapin, 'Discipline and bounding: the history and sociology of science as seen through the externalism–internalism debate', *History of Science* (1992), **30**, 333–69, 340.

and Lilley. Lately, Gary Werskey has summarized the way that Lilley's tortuous efforts 'to avoid the "c-word" (capitalism) and the "M-word" (Marx) by adopting the external/internal distinction are a poignant example of what proved to be a pointless effort to demonstrate what a reasonable chap he was'.[162] This enthusiastic evaluation of Lilley is echoed by Hasok Chang in a recent volume of *Centaurus*: 'He is a most under-appreciated historian of science. Perhaps his work had too much plain good sense to make him famous!'[163]

Lilley was not only trying to bring society into the history of science. He also had a strong commitment towards bringing history of science into society. When he began popularizing history of science in *Discovery* or through the ASW from around 1942, there was no such thing as history of science in a professional sense. Although Lilley remained an outsider with regard to the disciplinary developments in Cambridge, his initial interest always sought to relate history of science to the wider society outside the university. While he failed twice to get a formal position in Cambridge, he was still regarded as acceptable within the BSHS and the IUHS. I have thus argued for the continuity of Lilley's contribution, while at the same time emphasizing how his moderate Marxism was radically misinterpreted both by Hall and by Young. Finally, by tracing Lilley's legacy through the 1960s, I have suggested that the supposedly new contextualist perspectives of the 1970s not only have a striking similarity to Lilley's moderate methodology, but also to some extent exist in direct continuity with Lilley's contribution. Lilley was a solid supporter at the basic level of what eventually became professional history of science, inviting future giants to continue the work by doing better in the future. One crucial difference is, of course, Lilley's explicit political commitment present in some of his writings. Furthermore, his approach to the social history of science was initially developed *by* a scientist *for* the scientists so that they could take action even in extra-scientific spheres.

162 G. Werskey, 'The Marxist critique of capitalist science: a history in three movements?', *Science as Culture* (2007), **16**, 397–461, 455, n. 112.

163 H. Chang, 'The historian of science: painter, guide, or connoisseur?', *Centaurus* (2008), **50**, 37–42, 37.