History of Science
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The discipline of the history of science (henceforth 'the history of science') concerns the history of the way nature has been manipulated, modelled and understood by different societies. Because the sciences deal with what are taken to be true statements about a natural world that exists independently of human activity, the subject of the history of science seems to resist being *historicised* in the same way as other topics. This alone makes the history of the sciences different from other species of historical enquiry. In addition to providing us with true statements about nature, science has been held to have a unique capacity to progress. As such, it seems to be exemplary of the potential of human reason. Belief in scientific rationality and scientific progress was almost unanimous in academic history of science until the 1960s, but such views have been heavily criticised in the last three decades.

Perhaps the fundamental question for the history of science has been why, and in what sense science is a different sort of activity from any other. The assumption that the method and object of scientific practice demarcates it from all other human activities drew history of science and philosophy of science closely together. The opposite point of view, according to which scientific practice is fundamentally similar to other forms of human endeavour, has led to a marked separation between the two fields. Instead, the discipline has allied itself more closely with developments in sociology of science and other historical disciplines, such as imperial history, economic history and global history.

In the university curriculum the subject has sat squarely between the humanities and the sciences, only occasionally being granted the status of a department. Otherwise, it has been housed in a wide range of faculties and departments including anthropology, sociology, science, and philosophy, and only rarely has the subject been located in a school or department of history. Moreover, while historians of science have published in the major journals in the field, they have been conspicuously unsuccessful in placing their work in mainstream history journals. As a consequence of this, the topic has been marginalised and there is a lamentable ignorance in the wider historical profession of basic facts about the historical development of science.

In the twentieth century, the history of science has been lauded by many of its exponents both as a uniquely interdisciplinary activity, and at various periods commentators and academics have argued that the history of science has an unrivalled capacity to appeal to students of both scientific and humanities subjects, tempering the narrow specialism of one group while opening the eyes of the other to the great achievements of science and technology. Many have argued that science's rationality is peculiarly universal, and its ability to inform and improve technical practice makes it paramount in both forging and defining the central features of the modern age. It stands above national or religious interest and represents unparalleled international cooperation. Being morally neutral has also allowed it to make a uniquely important contribution to human civilization and well-being. Historians of science have therefore had to balance the widely held view that science lacks any intrinsic or historically bounded moral values, with the view that particular forms of scientific endeavour can and must be placed in their historical contexts.

Acres of woodland have been destroyed so that historians of science can debate whether it denigrates scientists to show that even their most successful theories are informed by contemporary religious and other ‘non-scientific’ values. Whatever position one takes on the capacity of science to escape its local contexts of production, the practice of the history of science has been affected by numerous external forces, most notably by the two world wars and, perhaps most powerfully, by the Cold War. In times of crisis, science has appeared to democrats as exemplary of a self-critical and meritocratic society. While in some sense neutral, it has been lauded as a form of knowledge that could only have arisen in the West, where there were unprecedented opportunities to pursue and publish natural knowledge conducted for its own sake while at the same time engaging in correspondence with other researchers. To Marxists, science is legitimated by its application to the outside world. It has appeared as a paradigmatic example of how a number of human beings -- technicians, engineers and scientists -- can all work together for the benefit of the whole.

The history of science in the twentieth century has passed through a number of phases. The first was characterised by great individual contributions from authors such as Pierre Duhem, J.E. Dreyer and others, with major contributions to the philosophy of science coming from scientists such as Duhem, Ernst Mach and Henri Poincaré. Secondly, in the wake of the Great War, the history of science seemed to epitomise what George Sarton called the ‘new humanism’. It seemed to offer an account of how civilized people all over the world had contributed to the one great project that could elevate them above their petty nationalistic and religious differences.

However, in the 1930s, the arrival of Marxist-inspired socio-economic approaches to the history of science forced liberal humanists to stress the contribution to science made by individuals, theories and ‘reason’. While Marxists emphasised the socio-economic determinants and social consequences of science, the liberal humanists extolled the capacity of great geniuses to rise above the obstacles placed by these same surroundings. Increasingly, they identified elements of the Anglo-British culture as a bulwark against Marxist determinism and German obscurantism. Consequently, like those histories that examined the more occultist and less acceptable interests of scientific heroes, Marxist histories of science barely registered as serious undertakings in the academy for many years after the end of the war. However, in the 1960s and 1970s historians turned away from purified, intellectualist accounts of the exact sciences of the Scientific Revolution, to social histories of the eighteenth and in particular, of the nineteenth century life and earth sciences.

The intellectual history of science has remained a powerful force within the discipline as a whole. This approach has remained balanced between an examination of the religious and metaphysical commitments of individuals, and a more narrowly focussed attention on their technical accomplishments. However, following the advent of a ‘social’ history of science, a fully fledged materialist account of the history of science became possible when historians integrated the history of scientific instruments and their use into more mainstream history of science. In the 1970s and 80s, the discipline borrowed approaches in the sociology of science in order to discuss historically the skilful use of instruments and machines, without which almost no scientific work would be possible. At the end of the century, history of science has addressed the formation of the global (and extra-global) reach of science, and historians have linked the expansion of science to large-scale processes such as industrialisation, colonialism and imperialism.

Throughout this essay I integrate an account of developments in historiography with the story of its institutional development in British universities. British historians of science have forged their own idiosyncratic traditions and approaches, especially from the 1960s onwards, but they have closely followed developments in Europe and particularly in the United States. For that reason, it is impossible to tell the story of the intellectual developments within the discipline without situating them in an international context. Finally, although there is a strong case to be made for believing that articles have been the primary units of innovation and influence in the field, for ease of reference I have tended to concentrate on book-length works.

1. Early History of Science

1.1 Eighteenth and Nineteenth Century Progenitors

As a professional academic discipline, the history of science came into being soon after the First World War, but scholars, usually practising scientists, had written historical accounts of their own disciplines for over a century before this. As Rachel Laudan has pointed out, apart from demonstrating that their subject enjoyed the status of a bona fide science, one early motivation for writing histories of particular sciences was to show that knowledge (and thus humanity) was capable of progress, and indeed had progressed up to the present day. Depending on various contexts, histories of the sciences were used to display the unfolding of a new enlightened human understanding (in which science and mathematics had the leading roles); to offer a way for a general audience to grasp the basics of a specific subject; or to gain legitimacy for a particular specialism by showing that it had some founding philosopher or principle from which everything subsequent to it had developed (Laudan, 1993).

In 1758 Jean-Étienne Montucla published his *Histoire des Mathématiques*, a scholarly analysis of the history of mathematics which equated cultural progress with the development of mathematics and which was intended as an attack on the barrenness of the humanities. Closely allied to one of the two progenitors of the *Encylopédie*, Jean le Rond d'Alembert, Montucla offered the first of a number of accounts of the rise of the mathematical sciences that appeared in the rest of the eighteenth century. He was followed in this by the scholarly multi-volume histories of astronomy by Jean Bailly and Jean-Baptiste Delambre in 1779-82 and 1817-27 respectively (Laudan, 1993: 1-2, 6-9).

From the start of the nineteenth century, German scholars working in the tradition of *naturphilosophie* produced a number of grand histories, or philosophies of history. These involved unifying themes concerning the development of Reason, *Geist* and *Kultur*, or the merging of natural history with overarching histories of the Earth and life on it. Scientific knowledge played increasingly influential roles in histories of human culture, while Darwinism became the dominant *historical* narrative capable of linking a range of new sciences. Figures such as Adam Smith, Joseph Priestley, Georges Cuvier and Alexander Brongniart, composed histories of astronomy, electricity, vision, zoology, botany and geology, most of which were designed to celebrate the existence and legitimacy of new disciplines. In various subjects, both general and particular histories thrived (Jardine, 1999, 478-83).

The two most influential nineteenth century histories of science also offered a general account of what today we would call philosophy of science. As such, the historical examples were intended to corroborate a general theory of scientific progress as much as the theory was invoked to organise the history. Following the vogue for grand Hegelian-style accounts of the progress of Reason, Auguste Comte's *Cours de Philosophie Positive* (1830-42) presented a three-stage account of the development of science. These phases were the theological, the metaphysical and the scientific (or positive), the last of which left behind the futile search for causes of phenomena in favour of the uncovering of general laws. Comte argued that all sciences were capable of reaching the final stage of development, although only the mathematical sciences (not including chemistry or experimental physics) had done so. He believed that scientific progress began when knowledge became more abstract and freed itself from its craft origins and then from any unnecessary remaining metaphysical elements. This process had been of long duration but a "general and continuous revolution" had occurred at the end of the sixteenth century. Comte's 'Positivism' was taken up by social reformers, especially in the France that followed the revolution of 1830, who believed that society could be organised along more rational lines. Comte's work was significant in a number of other areas, most notably in Henry Thomas Buckle's *History of Civilization in England* of 1857-61, in which the history of science was integrated into wider social and intellectual developments (Laudan, 1993: 12-15 and Guerlac, 1963: 806-7).

William Whewell's *History of the Inductive Sciences* of 1837 and his *Philosophy of the Inductive sciences* of 1840 were deliberately composed in opposition to the Comtean thesis. Whewell had been a member of the so-called Analytical Society, a body of Cambridge undergraduates that had set out to incorporate Continental analytical mechanics into the Cambridge mathematics curriculum, but later he regretted this move away from geometrical solutions to problems in mechanics. Whewell, who coined the term 'scientist' in 1833, wrote on an exceptionally wide range of subjects, including mathematics, mineralogy, archaeology and tidology, but it was his rich and original works on the history and philosophy of science that exerted the most influence outside the scientific community.

In the *History* Whewell put forward a general three stage history of science in which a period of fact gathering (‘the Prelude’) was followed by the bringing together of disparate facts through great discoveries (‘the Inductive epoch’). Finally, these discoveries were evaluated and accepted by the wider community (‘the Sequel’). If this resembled Baconian inductivism, in his *Philosophy* he emphasised the ways in which scientists had made use of organising principles or ‘ideas’ that served to ‘colligate’ various facts. For this he was accused of relying too heavily on idealist Kantian philosophies but he also cautioned that the organising mind was reliant on expert fact-gathering. Against Comte, Whewell argued that science had not progressed by eschewing the search for causes, but in fact its onward march was linked to the creation of increasingly refined methods of discovering novel types of causes (Laudan, 1993: 13-15; Fisch, 1991).

History of science in Britain was the preserve of a few committed writers, many of whom were based at Trinity College Cambridge. Notable here were the mathematician W.W. Rouse Ball’s *Short History of Mathematics* of 1888 and his *Essay on Newton’s Principia* of 1893, the latter made possible thanks to the generous decision by the Earl of Portsmouth to donate Newton’s scientific and mathematical papers to Cambridge University. Thomas Heath combined a position as a civil servant in the Treasury with the role of a college fellow and historian of ancient Greek mathematics. He published major histories and translations of the work of Diophantus, Apollonius, Archimedes, Aristarchus and most notably, Euclid, in 1885 (revised 1910), 1896, 1912, 1913 and 1908 (new edition 1926) respectively. He synthesised these researches in his *History of Greek Mathematics* of 1921. William Whetham (Dampier after 1931), physicist (and also fellow of Trinity), wrote the best-selling *Recent Developments in Physical Science* in 1904, and in subsequent years published on a wide range of topics such as heredity and agriculture. His *History of Science and its relations with Philosophy and Religion* of 1929 also went through numerous editions, and was to be a strong supporter of the introduction of the subject into Cambridge in the 1930s.

Otherwise, there were few examples of work that matched the standards of European scholarship. The books on William Herschel and Tycho Brahe written by the Dane John Louis Emil Dreyer, were examples of excellent and thorough historical scholarship. Dreyer had become the astronomer at the Earl of Rosse’s observatory in Parsonstown in 1874, and he composed these works despite moving to larger observatories in Ireland and cataloguing all known nebulae (which he accomplished in 1888 after just two years, adding over 5000 more in the following two decades). His *History of Planetary Systems from Thales to Kepler* of 1906 was an original history of astronomy, but his lasting achievements were extraordinary editions of the works of Herschel and Tycho (in 1912 and 1913-26 respectively) (New DNB).

1.2 Mach, Poincaré, Duhem

In Europe, a number of scientists turned to history to bolster their theories of scientific method (what we would call philosophy of science). The tradition of offering overarching accounts of scientific progress through the use of historical examples was continued at the turn of the century by Ernst Mach, Pierre Henri Poincaré and Pierre Duhem. Working in an era of extraordinary innovation in physics, each used historical examples to illuminate highly original philosophical positions. Mach spent most of his career as a professor of experimental physics at the University of Prague (from 1867 to 1895), where he worked on a number of different areas including the physiology of sense-perception, optics and supersonic ballistics. In his writings on the history of mechanics and the philosophy of science, and especially in his *The Science of Mechanics* of 1893, he argued that the assumption that there were real theoretical entities such as atoms was an unwarranted leap of inductive faith. Only scientific experiences, or more basically, physical sensations were real, and the goal of science was to formulate the simplest laws that accounted for the known facts, the latter being the disciplined experiences consisting of experiments and observations. For that reason, all theories, including those of Newton and Einstein, were provisional, to be revised when a new formulation or new facts called existing theories into question.

Poincaré made a number of discoveries in the fields of applied mathematics and celestial mechanics, and did pioneering research on non-Euclidean geometry and the three-body problem. He also did both practical and theoretical work on clock synchronisation that was important in paving the way for Einstein's Special Theory of Relativity. In the 1870s he studied at the École Polytechnique and the École des Mines, and from 1881 he taught at the Sorbonne, where he remained for the rest of his career. In his *Science and Hypothesis* of 1902, he argued that scientists adopted those theories that were most useful for solving problems in physics. Newton's first law of motion (concerning the fact that a body will remain in its current state of rest or motion unless acted upon by another body) was a *convention* that was assumed to be true for simplifying mechanical problems, but it described a situation that could – with equal accuracy – be couched quite differently. Experiments by themselves could not determine which of two systems to adopt; all the terms of the Newtonian system are conventions that hang together but Newtonian dynamics – and indeed Euclidean geometry -- are not the only frameworks through which scientists can experience the external world (Giedymin, 1982 and Galison, 2003).

Pierre Duhem was a brilliant physics student at the Ecole Normale Supérieure in the early 1880s and in the following decades went on to publish numerous works in theoretical physics, the most innovative aspects being in statistical thermodynamics. Alongside a lifelong commitment to the Catholic faith, and a belief that all physical theory could reduced to thermodymanical principles (which he and contemporaries termed 'energetics'), he completed a three volume work on Leonardo da Vinci not long before his death. In 1906 and 1908 he published two major works in the history and philosophy of science, *The Aim and Structure of Physical Theory* (2^{nd} edition in 1914) and *To Save the Phenomena*, and a ten-volume work on the history of cosmological doctrines up to Copernicus, begun in 1913, was completed posthumously in 1959.

In these works Duhem argued from a positivist assumption in that he separated physical theory from metaphysics, but unlike Comte he argued that metaphysics dealt with more significant forms of knowledge. Moreover, he had a much more sophisticated view of the relations between fact and theory than Comte, or for that matter, Whewell. His most important doctrine, made famous by W.V.O. Quine over half a century later (and later known as the Duhem-Quine thesis), was the anti-empiricist argument that because there was a web of interrelated beliefs and theories underlying any experiment or observation, data or facts *by themselves, or in isolation* could never determine fully the adoption of one theory rather than another. A seemingly disconfirmed theory could be saved according to a number of strategems, or a different theory could be invoked or formulated to account for the new facts. A related view was that science ought to aim at creating accurate accounts of the world (i.e. 'saving the phenomena'), without being committed to the view that these views are true or in some sense corresponding representations of reality. Despite Duhem's notion that theories were 'undetermined' by data, he believed that Poincaré's conventionalism made theory-choice far too arbitrary.

Duhem was one of the first scholars to take seriously the study of natural philosophy and mechanics in the medieval period. In works published in the last decade of his life, he argued that there was a continuity between medieval practices and approaches and the better known heroic endeavours of the early modern period. For Duhem, Galileo was not so much a modern as a practitioner of the fourteenth century Parisian tradition of mechanics. Galileo's theory of inertia was in effect merely an extension of the concept of impetus developed by Jean Buridan and others over two centuries earlier. Scientific advance came through continuous accretion effected by numerous workers over centuries rather than revolutionary overthrow of past theories by individuals.

Along with a deep-seated nationalism that pervaded his historical work, Duhem's Catholicism was central to the way he analysed the past. In taking a positive view of the medieval Church, he arrived at conclusions diametrically opposed to the conventional views about the Church's hostility towards science. For example, when the Archbishop of Paris Etienne Tempier issued a decree against a number of Aristotelian positions in 1277 that restricted God's power, this allegedly made it possible to consider the existence of other worlds beyond earth, and to consider that the heavenly spheres could have rectilinear motion. Indeed, Duhem went so far as to say that the decree was the birth of modern science. His religious bias and his commitment to the underdetermination thesis made him sympathetic to the more cautious attitude of the Catholic Church when faced by Galileo's claim that new data conclusively proved the truth of the Copernican theory. Although the following generation of scholars, including Anneliese Maier, E. J. Dijksterhuis and Alexandre Koyré would disagree with his emphasis on the primacy of medieval scientific innovation, Duhem's work served as an inspiration and foil for historians seeking to understand how, if at all, a 'Scientific Revolution' had occurred in the seventeenth century (Jaki, 1984; Cohen, 1994, 54-76).

2. History of Science in the Academy

2.1 History of Science: The Formation of a Discipline

The founding father of history of science as an academic discipline was the Belgian scholar George Sarton. Sarton founded the journal *Isis* in 1912 while in Ghent, and edited it for four decades. At the outbreak of the First World War he went to England but could find no employment is a teacher or independent scholar in the history of science, and he travelled to the United States. After the war he was funded by the Carnegie Institution, and succeeded in revitalising *Isis*, which had ceased publication when he had left Belgium. Sarton devoted most of his research to producing a gigantic *Introduction to the History of Science*, which would survey the place of science and individual sciences within the great civilizations from Ancient Greece onwards. However, when the third volume of a projected 26 appeared in 1948, he had only reached the end of the fourteenth century. He believed that the sciences represented human greatness at its broadest and deepest form, and they each contained some unifying features that made it possible to speak of science as a whole (rather than as a group of sciences). Despite having a far richer conception of the place of science in its local milieux than Comte, Sarton agreed with his predecessor that all the sciences exhibited a uniquely rational and progressive quality. Accordingly, in examining past achievements in science he concentrated on the acceptable, positive scientific attainments that had acted as building blocks for discoveries by later generations. His compilation of bibliographies, a key function of *Isis*, was explicitly designed to serve the same supporting role for the future development of the field (Thackray and Merton, 1972: 479-80 and 490-1).

Sarton believed that science provided a way of effacing the differences that existed between different nations and cultures. His commitment to the unity of science was closely allied to his belief in the unity of mankind, and the titles of his works show how closely he linked the practice of science to the attainment of *civilization*. Indeed, with typical vigour, and in times that were hardly auspicious, he tried to create an Institute for the History of Science and Civilization as early as 1917. In promoting the history of science as an ideal and as a reality -- particularly at Harvard, where he had been given a lecturing position in 1916 -- he aimed at founding a new, synthetic discipline that would span the divide between the sciences and the humanities. He called this new area of study ‘new humanism’, and believed that exposure to the new field would make scientists into citizens. Sarton demanded exceptionally high and unattainable standard for teachers in the field: they had to be au fait with current science, possess a number of languages, and be conversant with the scientific achievements and philosophical currents of the period they were studying. As Arnold Thackray and Robert Merton have noted, Sarton’s drive and vision was crucial in shaping the discipline in the United States and indeed in creating an international community of historians of science. Yet despite showing what a rigorous scholarly history of science might be like, his own histories were turgid, and when he died in 1956 he left no disciples to carry on his approach (Thackray and Merton, 1972: 482, 487).

The first institution to formally teach the history of science in Britain was University College London, where a central rationale for creating the subject was that it should be aimed at science teachers. Abraham Wolf, who was the first head of the Department of the History and Method of Science when it was created in 1921, had a remarkable career.

He joined UCL as an assistant professor of philosophy in 1906 while still carrying out his duties as a rabbi in the Reform synagogue in Manchester but soon afterwards, his social radicalism and his penchant for the rationalist philosophising of Maimonides and Spinoza forced him to turn wholly towards the secular studies offered by the university setting (see Haberman, 1991, esp. 285-90). Wolf published on a wide variety of subjects including Nietzsche's philosophy (in 1915) and scientific method (in 1925), and having earlier written an account of (and translated) Spinoza's 'Short Account of God, Man and his Well-being' (in 1910), he edited the correspondence of his hero in 1928. His best known work, with some chapters contributed by colleagues, was an accomplished history of science and technology from the sixteenth to the eighteenth centuries.

In 1919-20 Wolf offered 20 lectures on the history of science from the ancient period to the middle of the nineteenth century. He invited a number of other scholars from a wide range of departments to give lectures in various topics in the history of science, and the success of these lecture series led to the formation of the new department. The presence of history of science within UCL was also helped by the appointment to the Faculty of Medicine of Charles Singer as a historian of medicine. Singer must have been an interesting colleague. The son of a rabbi who helped found the Liberal movement in British Jewry, he became arguably the most dominant presence in British history of science and medicine between 1920 and 1950. Singer moved to Oxford in 1914 to work with the Regius Professor of Medicine, Sir William Osler (himself a keen historian of medicine), taking up a position that explicitly allowed time for historical research. During this period Singer published a number of papers on the history of medicine despite spending much of his time in work connected with the war.

Following the success of the first of his two volume *Studies in the History and Method of Science* (1917-21), Singer came to the attention of the Australian neuroanatomist Grafton Elliot Smith, and he moved to UCL in 1920. He taught the history of medicine and biology at UCL throughout the 1920s and remained exceptionally productive, publishing a number of works including the *Evolution of Anatomy* in 1925 and the *Short History of Biology* in 1931. He was president of the Académie Internationale d'Historie des Sciences from 1928 to 1931, and was president of the famous Second International Congress in the History of Science and Technology that met in London in the summer of 1931 (see below). With no teaching duties after 1934, he moved to Cornwall and led efforts to fight anti-Semitism and to support refugees from Nazi Germany (See more generally Cantor, 1997).

Elliot Smith makes a fascinating study in his own right, for outside his medical work, he was a phenomenally successful author on all things Egyptian, arguing in a number of works that ancient civilization had originated in Egypt and had then diffused outwards in a series of migrations. Long interested in mummification, Elliot Smith was not slow to capitalise on the public fascination with the discovery of the tomb of Tutankhamun in November 1922 and he published a book on the subject in the following year. He was consulted on a number of occasions by the chemical industrialist Sir Henry Wellcome, the world's leading collector of medical books and curiosities. Wellcome's will, which created the Wellcome Trust, would be of exceptional importance in promoting the teaching and research of history of medicine after the Second World War.

2.2 Douglas McKie, *Ambix* and *Annals of Science*

From the mid-1920s Douglas McKie and Angus Armitage became full-time members of staff, giving lectures for practising scientists in the history of chemistry and astronomy respectively. Armitage specialised in various aspects of the life and career of Copernicus, while McKie was a historian of chemistry who was also central to the founding of two major British journals in the field. Wounded at Paschendaele in the First World War, McKie joined the history and philosophy of science department at UCL in 1925, not yet having completed his PhD on the adsorption of gases (see Robinson, 1968). He worked on the history of the theory of combustion, writing two major works on Lavoisier in 1935 and 1952. With the curator of the Oxford Museum for the History of Science, Frank Sherwood Taylor, McKie helped set up the Society for the Study of Alchemy and early Chemistry (later named the History of Alchemy and Chemistry) in 1937, and the society's journal, *Ambix,* was founded in the same year. From 1933 the study of the 'occult' sciences was also helped greatly by the arrival at the Courtauld Institute in London of the Warburg Library and along with it a number of scholars such as Ernst Gombrich, Edgar Wind and Fritz Saxl. As we shall see, Walter Pagel, another refugee interested in the intellectual contexts of early modern chemistry, was to have a significant impact at Cambridge.

Given the positivist assumptions inherent in a great deal of history of science at the time, alchemy was still viewed by many as an unfortunate and even ludicrous practice that had had to be overcome before modern chemistry could flourish. Although constrained by the outbreak of war, McKie, Taylor and J.R. Partington helped make the study of pre-Lavoisierian chemistry a serious scholarly activity. In the 1930s McKie co-authored with Partington a major four-part history of the notion of phlogiston and contributed chapters on the history of chemistry to Wolf's volumes on the history of science and technology. He had a major hand in the publication of a new edition of these works in 1950 and 1952. As Eric Robinson notes in his obituary, McKie was a keen bibliophile and an early defender of the idea that historians of science should above all be trained as historians rather than as scientists (Robinson, 1968: 320-1).

In 1936 McKie had also played a central role in the formation of the first major British journal for history of science, *Annals of Science*. The American historian Harcourt Brown later recalled that while he doing research on the foreign correspondence of Henry Oldenburg at the Royal Society Library in 1934, he had discussed with Henry Robinson (the Society's librarian and co-editor of the diary of Robert Hooke) the possibility of founding a new outlet for publishable papers in the history of science. Unlike *Isis*, the journal would publish relatively quickly and would concentrate on the modern period, "bringing to light new documents of special interest, and illuminating new aspects of social and economic history, [leading] to informed discussion of the place science has come to occupy in the modern world." In fact Robinson had already been in contact with

McKie and the publishers Taylor & Francis about the possibility of creating a new journal, and with the support of Brown, *Annals* appeared for the first time in 1936, already festooned in its distinctive orange cover. McKie was its main editor for three decades and except for a number of years where its appearance was prevented by war, the journal was marked by an eclectic mix of articles of exceptional quality, including papers by Grant McColley and Marjorie Hope Nicolson on science and literature, and McKie's and Partington's history of the theory of phlogiston (Brown, 1970, iii-v).

3. The Social Contexts of Science

3.1 The Social and Economic Roots of Science

Academic history of science was carried out in the contexts of vast events and narratives that affected intellectual culture and the wider society. In the 1920s and 30s, new ideological differences would begin to colour the way historians wrote about science and technology in the past. Professional historians of science such as those at UCL were careful to avoid any ideological taints being read into their work. They emphasised the heroic nature of many early modern natural philosophers, whose brilliance and power of thought had allowed science to escape the superstitious obstacles placed in its way by organised religion. In the 1920s, this could (as it did for Singer) serve the function of demonstrating the unrivalled capacity of science to speak truth to tyrannical institutional power. In the 1930s, this belief in the separateness of science from its societal surroundings was shaped by its opposition to Marxist socio-economic explanations, and there grew a concomitant suspicion of any historical links between science and its technological or economic relations.

In very different ways, both these approaches placed an immensely high value on science. For others, particularly those writing in various parts of Germany, the untrammelled pursuit of scientific knowledge and technological prowess was held responsible not merely for the worst atrocities of the war but also more generally, for a pervasive and soulless malaise in the western world. While some religious leaders in Britain called for a moratorium on scientific research in the late 1920s, a hostility to the perceived irrationality of modern science (especially the supposed 'Jewish'physics of Einstein) characterised Nazi ideology.

As the political situation lurched towards crisis at the end of the 1920s, so the emancipatory potential of applied science increasingly appealed to British writers and scientists on the left. Those in the 'Red Decade' of the 30s who wrote from a socialist or even Communist perspective were likely to adopt a broadly Marxist approach to the role of science, viewing the latter as formally dependent upon the productive forces that existed at any period. They were therefore primed to see science in its applied form in terms of the key role it played in enhancing the nation's productive forces, and as a central means of in liberating workers from capitalist oppression.

In June 1931, during a period of global political and financial turmoil, a delegation of Soviet scholars and politicians arrived in London in connection with the Second International Congress in the History of Science and Technology. As Gary Werskey has noted, this was only possible because a fortnight earlier, Stalin had made peace with the bourgeois intelligentsia after an intense period of harassment. The Soviet group was headed by Nikolai Bukharin, once a confidant of Lenin and now head of the section on history of science within the Soviet Academy of Sciences. With time short because of the late decision to attend, and delegates therefore unable to give their talks in their entirety, the papers were printed off by the Soviet Embassy and made available to the audience on the date that the papers were given (on July 4th). They appeared as a book, *Science at the Cross Roads*, on the 7th. At a number of points throughout the conference – and to the chagrin of Singer, who was chairing the meetings -- members of the Soviet delegation often intervened in discussions to make the case that the focus on Great Men distracted attention from the much greater social and economic forces that had moulded them (Werskey, 1988: 138-41).

Bukharin's paper predictably extolled the achievements of Soviet science and condemned bourgeois science in which those who practised science 'for its own sake" were artificially separated from engineers and applied scientists. Before this, the physicist Boris Hessen gave a now famous paper on the social and economic roots of Newton's *Principia Mathematica* of 1687, arguing that this great work was the product of the nascent capitalist and industrial base that characterised the 'English' mode of production. He began with the relatively unobjectionable claim, parts of which could be found in the *Communist Manifesto*, that military, economic and imperialist 'needs' had stimulated advances in knowledge and techniques associated with mining, gunnery and navigation. Hessen added that there were various other 'superstructural' activities, such as political interests, philosophical theories and religious beliefs, that gave rise to the form of the work of individual scientists, but he cautioned that these too were determined by the deeper economic base. Science had thrived historically, and would do so again, in those countries most positively disposed towards developing their productive forces (Hessen, 1931: 27; Schaffer, 1984; Werskey, 1988: 142-44 and Graham, 1985).

The impact of Hessen's talk was felt particularly keenly in a land where Newton was held by all to be a paragon of scientific genius. Yet he was by no means the only person to attempt to develop what were ostensibly plausible connections between capitalist features of the Renaissance and early modern periods, and the advent of modern science. Leonard Olschki had already offered a general account of the relations between the two processes in his three-volume history of vernacular scientific literature (1919-27). Elsewhere, Franz Borkenau, Henryk Grossman and Edgar Zilsel all wrote narratives that placed capitalism or industrialisation at the heart of the revolution in science that had occurred from the sixteenth century onwards. Zilsel's legacy is of particular interest since much of the work he produced in the 1930s was translated into English and published in the 1940s, notably in the *Journal for the History of Ideas* and *the American journal of Sociology*. Otherwise, serious history written from a Marxist point of view was best represented in the work of the classicist Benjamin Farrington, who wrote a book in 1936 on science in the Ancient World and after the war on the philosophy of Francis Bacon.

While the German-language histories of science were largely unknown in Britain, the much less sophisticated approach of Hessen became popular in Britain at the very moment that Hessen and Bukharin themselves were liquidated (in 1936 and 1938). Although the Marxist historiography was seen as overly crude by Singer, Wolf and McKie, the Soviet contribution to the London Congress was highly influential on a number of left-wing scientists and science writers, such as Hyman Levy, Lancelot Hogben, Joseph Needham, J.B.S. Haldane and J.D. Bernal. Although they differed considerably in their political outlook – Hogben believed that both communist and capitalist industrial technology was poisoning the planet -- they were united by a belief that in perilous times, socialism was the only means of combatting fascism while increasing the quality of life of the common man. They placed science and technology at the heart of their analysis of society, and they argued that increased investment in these areas would be both morally and socially uplifting. Nevertheless, many were practising scientists engaged in 'blue sky' research and some found it difficult to support the official Soviet position regarding the overwhelming significance of applied scientific research. (Werskey, 1988: 115-26, 146-56).

For Bernal, who made his scientific reputation in the field of X-ray crystallography, the character of science was straightforwardly conditioned by the economic foundations of the society in which it took place. However, science was also in some sense value free, and could be the most potent instrument for human progress. In the right social conditions, which Bernal believed were achieved under socialism or more accurately, communism, science was the quintessential force of social progress and human liberation. In turn, socialism itself was a 'scientific' system, the result of the application of the scientific method to social order. Since all science was conditioned by its social contexts, Bernal called for history of science to be properly *historical*, that is, to take into account the socio-economic and other 'non-scientific' backdrops against which scientific research had been practised. In 1936 he was critical of Wolf's history of sixteenth and seventeenth century science and technology precisely because the professional historian had analysed only what he took to be praiseworthy 'modern' features of past practice and had ignored the key role played by religious values in shaping the character of science. That is, he had failed to take on board the advances offered by Hessen's socio-economic account of science (and failed to mention Hessen's paper at all). Moreover, any history of science, like Wolf's, that failed to indicate the social effects of science was a failure (Werskey, 1988: 185-97; Mayer, 2004: 49; Brown, 2005).

In *Science in History*, published in 1954, Bernal expanded upon a topic first aired 6 years earlier in lectures given at Ruskin College. In the first half of the book he attempted to contextualise specific ways of understanding nature dating from neolithic times to the start of the twentieth century, understanding 'contexts' as the social and economic structures of the societies in which knowledge of the natural world was prized. This was a competent, rigorous and sustained Marxist account of the history of science in the Hessen mould, stressing the needs or requirements of societies in England, Italy and the

Netherlands that gave rise to the Scientific Revolution. In the second half Bernal dealt with the social sciences, praising Lysenko and others who had pioneered 'Soviet' styles of science, and went on to lambast contradictions and drawbacks in 'capitalist' science. However, the development of fission and fusion nuclear weapons, and the appearance of the military-industrial complex corroded Bernal's view that the science-technology nexus was intrinsically progressive (Bernal, 1969 and Werskey, 1988, 318-9).

Joseph Needham was by far the most significant Marxist contributor to the history of science. A Christian Marxist, Needham was already a well-known figure in the Cambridge academic community. Made university demonstrator in biochemistry in 1928, Needham was an accomplished scientist and in 1931 he published the three volume *Chemical Embryology*; three years later the introductory historical part was published separately as *A History of Embryology*. By the late 30s his left-wing sympathies were becoming clear, while he also became fascinated by Chinese culture and history. In 1942 he travelled to China to be Scientific Counsellor to the government of Chiang Kai-shek, and became obsessed with why the great historical intellectual and technical triumphs of China had never given rise to the sort of science produced by the West. Needham was director of the Department of Natural Sciences at UNESCO from 1946-8 but returned to Cambridge after this stint, now able to devote himself full time to the study of the history of science, technology and medicine in China. The first of his monumental volumes entitled *Science and Civilisation in China* appeared in 1954, and many tomes in the series have appeared in the half-century since, a number of which have been edited by the researchers who supported Needham in his work (Davies, 1997: 95-100).

In 1937 the Oxford scholar George Clark responded to Hessen in his *Science and Social Welfare in the Age of Newton*, based on lectures given in 1936 at the LSE. Despite Clark's rejection of what he took to be Hessen's crudely materialist account of Newton's motivations and achievements, the role of the state in addressing practical problems in areas such as navigation and mining played a central role in his thesis. Nevertheless, according to Clark, the Marxist position was overstated and many non-economic factors had to be considered in accounting for the development of science. These included medicine, warfare and particularly religion, and he urged the view that most of the greatest advances occurred because scientists exhibited "the disinterested desire to know," and gave rein to "the impulse of the mind to exercise itself methodically and without any practical purpose". (Clark, 1970, 79-80 and 85-6). In turn Clark was criticised by Needham for assuming that Hessen was attributing economic motivations to Newton, when this was clearly not what Hessen was doing. As Needham wrote in his review of Clark: "The scientist may suppose himself to be primarily investigating the works of god, but he is doing so not in a vacuum, but in a social matrix which may mould his thought without him being aware of it." (Needham, 1938: 198).

3.2 Capitalism, Protestantism and the Scientific Revolution

The Marxist approach was not the only grand narrative that could offer a plausible set of contexts for the appearance of science in the early modern period. The Protestant

Reformation was clearly an event of immense historical significance and it seemed close enough in time to the great events of the sixteenth and seventeenth centuries to be a plausible factor in their occurrence. Even after Duhem had argued almost exactly the opposite, both Catholics and Protestant historians alike assumed that Protestantism was somehow more modern and science-friendly than Roman Catholicism. In addition to this, it was broadly believed that *as a matter of fact*, the greatest contributors to modern science (sc. the 'Scientific Revolution') had been northern European protestants. Evidently, all who worked according to these tenets were forced to explain away the great successes of Catholic philosophers and mathematicians in the same period, and were under some obligation to state what it was about Protestantism that made it conducive to innovative scientific research.

Although it had very little to say about science, the most relevant analysis of the role of the Protestant Reformation on the evolution of science was Max Weber's *Protestant Ethic and the Spirit of Capitalism* of 1905. Weber famously argued that the Calvinist doctrine of predestination gave rise (paradoxically) to the Puritan demand that one adhere rigorously to one's calling. This brought about an intense this-worldly activity that was energised by the view that one could demonstrate to others that one was part of the godly community, even if one could not prove for certain that one was saved. As an (ironic) unintended consequence, this in turn produced the formative 'rational' habits of capitalism, in which immense hard work was directed towards the production of capital, much of which was invested back into the improvement of machinery. Weber had little to say about science and not much more about the role of technology in the appearance of capitalism, but his book was explicitly intended to add religious-idealist elements to those materialist processes depicted by Marx in the *Communist Manifesto* and *Capital*. Weber's work (like the *Communist Manifesto*) can also be seen as a precursor of the modern genre of 'Rise of the West' books, many of which merely dress up in new clothing many of the central assumptions and conclusions reached by Weber.

Critics of Weber 's argument (including, to some extent, Weber himself) claimed that the historical relations between religion and capitalism were very different from or more complex than the position offered in *The Protestant Ethic*. In England, the economic historian and Christian socialist Richard Tawney published a work in 1926 entitled *Religion and the Rise of Capitalism*, which to some extent reversed the causal and chronological tale offered by Weber. Tawney concurred with many German economic historians who had written before Weber, in holding that a powerful and viable form of capitalism had existed before the Reformation. Although usury and profligate spending on luxuries had been condemned by the churches in the medieval period, Renaissance and early modern Christian churches failed to control the worst excesses of this form of capitalism and developed a cosy relationship with it. According to Tawney, once a powerful bourgeoisie was in control in England, then it reached to Calvin as a way of justifying many (but not all) of its religious and economic activities.

Coupled with the new interest in Marxist approaches to history, accounts like those of Weber and Tawney offered up the possibility of grafting the insights of the *Protestant Ethic and the Spirit of Capitalism* onto those of the *Communist Manifesto*, and of producing a super-narrative embracing the Reformation, capitalism and the appearance of modern science. A seminal contribution to the field along these lines was made in his 1938 book *Science, Technology and Society in Seventeenth-Century England* by the American sociologist Robert Merton. Co-supervised by Sarton, Merton took into account both religious and materialist approaches to the history of science, and the 'Merton thesis' has often been taken to provide an overarching explanation of the rise of modern science. However, in fact he was more narrowly concerned with the influence of 'Puritan' values on late seventeenth century English science, as understood from papers published in the *Philosophical Transactions* (published from 1666).

The role of these religious interests in forming the character of English science in the period took up less than half the book, and the remaining pages were concerned with other factors, many of them already outlined in Hessen's thesis. In these pages Merton argued from a sort of Weberian-Hessenian position that there was a consonance between the emphasis on this-worldly activity in puritan culture, and the stress on the practical consequences and experimental style of English science. In England at this time, intense Protestantism licensed a hostility to unexamined traditional thought and support for a specifically utilitarian approach to the natural world and science could become a religious vocation. As a semi-quantitative sociologist, Merton also did a prosopography of the early Royal Society and found that an unexpectedly high proportion were 'Puritans'. Although his thesis was heavily qualified, Merton had offered a powerful argument that amongst other things, had exemplified the benefit sociological theory could offer to the history of science. Nevertheless, his work appeared at an inopportune moment, and in the following two decades British and American historians would be wary of using any approach that seemed to endorse an anti-individualist account of scientific development.

3.3 Intellectualism triumphant?

In Britain and the US, the socio-economic approach to the history of science made little headway in the post-World War 2 university. In its most revolutionary form, science had been practised in college libraries and minds, and not in factories or military arsenals. Historians had a duty, most assumed, to show how it was that pure, beautiful scientific thought had extricated itself from its messy contexts, a retreat that was vital for science to have progressed as it had. Fortunately, a number of texts were available for historians who wanted to avoid being associated with 'externalist' accounts of science. Foremost amongst these was Edwin Burtt's brilliant *The Metaphysical Foundations of Modern Science*, which had originally appeared in 1924 (with a revised second edition in 1932). Burtt argued that one could best discover a particular 'world-view' of any period or culture by looking at the sorts of problems addressed by its philosophers. In the case of science, one was inexorably drawn back to the period when an older approach to the world was overthrown, a task that demanded an analysis of the metaphysics of Newton. According to Burtt, Newton was a philosopher rather than a 'scientist', and much of his triumph was in preparing "the metaphysical groundwork for the mathematical march of mind" at which he was adept. After his masterwork was published in 1687, the force of his demonstrations meant that his metaphysical notions were imbibed by whoever assimilated his scientific theories (Burtt, 1980: 15, 29).

For Burtt, Newton had obscured the means by which he had arrived at his great discoveries, and worse, his own "ultimate philosophy of the universe" was composed of ideas borrowed from his predecessors: "as a philosopher he was uncritical, sketchy, inconsistent, even second rate". Burtt astutely pointed to a tension between Newton's positivist dismissal of hypotheses, and his clear allegiance to a metaphysical system that was at best only partly revealed to his readers. Newton's positivist pose meant he was unable to formulate an internally consistent or original metaphysics, a position that inevitably gave rise to internal tensions such as the one between his commitment to the existence of an invisible ether and his conception that objects exerted an irreducible gravitational attraction on each other. Other inconsistencies existed, such as the fact that Newton's avowal of empiricism was incompatible with his belief in an absolute space and time, both of which were strictly unobservable. Burtt argued, that Newton's metaphysical views flowed from an even more fundamental belief, namely his theological notion that absolute space was intimately related to the divine sensorium. He concluded by showing that these views were themselves related to Newton's antitrinitarian theology (Burtt, 1980: 208, 227-30, 243-4, 256-63, 282-97 (esp. 284)).

Burtt's work was noticed by Alexandre Koyré (1892-1964), whose conceptualist style was to become the dominant historical approach in the US and Europe in the 1950s. Trained at Göttingen and at the Sorbonne, in the 1920s he wrote on theological themes in the writings of Descartes and St. Anselm, before composing studies on Russian philosophy, the work of Kant, Fichte and Hegel, and the writings of German mystics – all of which appeared in the late 1920s and early 30s. From the middle of that decade, largely persuaded by his reading of Burtt, he turned his attention to Galileo and produced a series of articles that were published in 1939 as *Études Galiléennes*. He moved to the US during the war and based at the Princeton Institute of Advanced Studies in the late 40s and 50s, concentrated on the Scientific Revolution and in particular, on the work of Isaac Newton.

Koyré's most influential work in the Anglo-American world was his *From Closed World to Infinite Universe* of 1957, a book that replaced Burtt as the basic set text for the Scientific Revolution in Britain and the US. Koyré set out to explain "a spiritual change" that had taken place in the seventeenth century, as a result of which the old anthropocentric cosmos of Aristotle and Ptolemy had been replaced by an infinite universe capable of mathematical description. In the late 1950s he worked on a series of Newtonian studies with the Harvard historian of science, Bernard Cohen, in connection with their efforts to produce a variorum edition of the *Principia* (which appeared in 1972). Cohen and Koyré mined a rich seam of Newtonian materials, especially the manuscript materials at Cambridge University Library, but their work on Newton's papers was oriented around an analysis of the conceptual development of his thought. While Cohen had greater expertise in the manuscripts and was more inclined to a positivist and internalist account of Newton's scientific work, Koyré held fast to the view that metaphysics and physics were closely linked. According to Cohen, this was the view that "that the central concepts of philosophy at any given time may be a determining element of the nature of the scientific thought of that age, and vice versa, [such as] the effect of the geometrization of space in the Renaissance, the concept of an infinite universe, matter and spirit". Physics was geometry made real, and as the originator of true mathematical physic, Galileo would always be Koyré's hero (Cohen, 1966: 159).

Koyré believed that his work displayed the workings and effects of pure reason, which worked independently of, and against external religious and political values. Having left to join the Free French in the early 40s, he became an ardent anti-Marxist in the later 40s and 50s, deploring Marxism, positivism or empiricism either as social theories or as philosophies of science. He gave as little weight to religious (i.e Protestant or Catholic) explanations of scientific views as he did to Marxist accounts; great thinkers were not conditioned by their social, economic, national or religious contexts, and science had not progressed through the observation or even experimental manipulation of nature. Downplaying the role of celestial observations, astronomy became the progressive conceptual science par excellence, while the role of experiment and experimental skill in the work of heroes such as Galileo and Newton was systematically downplayed. Mere, untutored experience provided only 'obstacles' to progress, which had to be overcome by thought; indeed, in one ecstatic moment of hyper-platonism, Koyré argued that scientific research ultimately only discovered laws that were already present in the mind. Nevertheless, these statements sit somewhat uncomfortably alongside his view that individuals had unified and connected 'transscientific' beliefs that flowed between science, metaphysics, philosophy and religion. Koyré's 'conceptual analysis' would be the template for much of Anglo-American history of science in the 1950s and 60s (Koyré, 1972: Iliffe, 1993, Cohen, 1966: 159).

4 The Postwar Profession

4.1 An expanding community

The Second World War would have major effects on the teaching of history of science at UCL, Cambridge and Oxford. After the war, the physicist Herbert Dingle left his position at Imperial College to join Armitage and McKie at UCL. He became head of a department that by 1947, with the addition of Niels Heathcote and Alistair Crombie, boasted 5 members of staff (Smeaton, 1997: 25-8). On his retirement in 1955 Dingle was elected president of the British Society for the History of Science (BSHS) for two years, but at exactly this time he began his unsuccessful and notorious efforts to show logical absurdities in the Special Theory of Relativity (and this despite having written a popular exposition of the theory in 1922). In the late 1940s, the UCL department was still dominant in the field: while McKie exerted influence through the recently revived *Annals of Science* and *Ambix*, Singer became the first president of both the BSHS and the International Union for the History of Science. He continued to produce scholarly works in the history of medicine, especially on the works of Vesalius, but he also edited the 5-volume *History of Technology* that appeared between 1954 and 1958.

A key moment in the development of the discipline was the founding of the BSHS in the spring of 1947. Singer was elected President and Dingle Vice-president, and the creation of the society was the culmination of energetic lobbying by Singer, Dingle and others to the Royal Society, UNESCO and the International Council of Scientific Unions (Cantor, 1997, 15-19). Surprisingly, it was another fifteen years before the society published its own journal, the *British Society for the History of Science*, papers having previously appeared as a bulletin attached to issues of *Annals of Science*.

As Geoffrey Cantor has shown, Singer increasingly viewed science as the exemplary form of international cooperation, and as the natural and proper replacement for religion (Cantor, 1997: 7 and 10-13). In his BSHS Presidential Address of 1948 he pointed to the wide range of places around the globe where scientific progress had been achieved, and noted that the current strongholds of the history of science (France, America and Britain) were bulwarks against the sorts of "tribal disruption" that had regularly threatened civilization. Science represented the only robust and trustworthy way of knowing both the world and Mankind, and the history of science could be part of Sarton's "new humanism", a project that could bridge social, national and intellectual divisions just as the humanist movement had done half a millennium earlier (Singer, 1997: 72-3).

4.2 History of Science in Cambridge

History of science had been taught at Cambridge from the 1930s and a History of Science Committee was set up in 1936 by Joseph Needham and Walter Pagel. Needham included historical discoveries and episodes in his scientific work believing that an awareness of major developments in the history of science could be the basis of scientific innovation. Pagel was a refugee, who had published a book in 1930 on Johann Baptista Van Helmont. Having been a university lecturer in pathology and history of medicine at Heidelberg before leaving in 1933, he was now working at the tuberculosis sanatorium in the Cambridgeshire village of Papworth. As Anna Mayer has shown, Needham's work embodied his own wish that history of science should be closely connected to current scientific research, while Pagel was committed to showing that the proper understanding of past medical and scientific theories should not artificially disconnect these views from religious and metaphysical 'ideas'. Indeed, Needham himself pointed to Pagel's work on "cabbalistic mysticism and 'magical' empiricism" as a corrective to Hessen's economic determinist account of history. In the late 1930s, Pagel's historicist approach, and Needham's view that the history of science could be relevant to modern practice, were not mutually incompatible (Mayer, 2000: 668-70 and 2004: 48-9; Needham, 1938: 199).

The formation of the History of Science Committee followed closely upon an exhibition of scientific instruments that featured many items belonging to Robert Whipple, the managing director of the Cambridge Scientific Instruments Company. Soon after its inception, the committee organised a series of lectures on the history of science over the previous four decades. These were given by prominent Cambridge scholars such as Dampier, Arthur Eddington, Ernest Rutherford and J.B.S. Haldane, as well as invited lecturers such as Fritz Saxl from the newly arrived Warburg Institute. Needham and Pagel published versions of these lectures in 1938 as the *Background to Modern Science*, and they played a significant role in efforts to present the nature and purpose of science to a popular audience. They also called for the establishment of a museum along the lines of the one already in existence at Oxford (Bennett, 1997: 34-5; Mayer, 2000: 672-5).

Once Needham left Cambridge for China in 1942, the committee was reformed, this time being dominated by scholars from the humanities. The leading figure was the historian Herbert Butterfield, author of *The Whig Interpretation of History* (1931) and Professor of Modern History after 1944. Thanks largely to Butterfield's view that the subject should be taught by people with historical training, he brought onto the committee scholars such as G.N. Clark, newly arrived from Oxford, Basil Willey, author of *The Seventeenth Century Background* (1934) and King Edward VII Professor of English; Charles Raven, who in 1947 wrote a work on English naturalists and who specialised in the works of the natural theologian and botanist John Ray; and Michael (Mikhail or Munia) Postan, Professor of Economic History, who worked in the Ministry of Economic Warfare during the Second World War, producing a major work, *British War Production*, in 1952. His major contributions to economic history during the middle of the century came in the form of his editorship of 5 volumes of the *Cambridge Economic History of Europe*, and of the *Economic History Review* (joint or sole editor from 1934 to 1960).

Post-war discussions concerning the role of history of science and other humanities subjects within the general curriculum were affected by government interest in promoting liberal humanist attitudes that would mitigate the worst effects of scientific and technical specialisation. Marxist and Warburg-style accounts were downplayed in or absent from lectures, and in parallel with Burttian-Koyréan developments in the US, the central topics concerned the history of *thought*. Potential links between history of science and economic history were also severed, but philosophy of science came to occupy a significant and in a short period of time, equal place in an HPS curriculum. Having given the Tarner lectures at Cambridge the year before, the moral philosopher Richard Braithwaite helped found the Philosophy of Science group in the British Society for the History of Science when it was founded in 1947. For many years he taught philosophy of science to philosophers and then from the early 1950s to natural scientists, when HPS was offered as part of the Natural Sciences Tripos (Mayer, 2000: 676-81).

Through his radio broadcasts and his position as a public intellectual, the best known of the historians of science at Cambridge was Herbert Butterfield. Butterfield added his authoritative position as an Oxbridge historian to the arguments made by Pagel and others to the effect that the history of science should properly be a branch of history. Butterfield's main contribution to the field outside of Cambridge was his 1949 book, *The Origins of Modern Science.* Based on his 1948 lectures at Cambridge, and promoted in the radio programmes he was making at the same time as the book appeared, Butterfield concentrated on Europe in the seventeenth century, with a glance towards the medieval theory of impetus, and two final chapters on the 'postponed revolution' in chemistry, and 'ideas of progress and ideas of evolution in the eighteenth century'. Famously, Butterfield announced on the first page that the 'scientific revolution' (a term he made famous) "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes, mere internal displacements, within the system of medieval Christendom." In the light of such a momentous opening, the book ends with a whimper. Following cursory treatments of the works of Cuvier and Lamarck, Butterfield claimed, with a whiff of teleology, that by the time of Cuvier, "all the ingredients of Charles Darwin's theory had already been discovered save the idea of the struggle for existence" (Butterfield, 1957: vii, 233).

There is a prima facie inconsistency between Butterfield's sophisticated anti-presentist attack on contemporary constitutional history in the *Whig Interpretation*, and the notion that one could pinpoint the *origins* of *modern* science. In various comments at the start of the work, Butterfield attacked the tendency to concentrate on heroic great thinkers and the assumption that the history of science merely required joining up their theories and discoveries. He claimed that he would explore what he called "the misfires and mistaken hypotheses of early scientists," and he remarked on the perils for the historian of dealing with 'anticipations' of later or current theories ("things which often owe a little, no doubt, to the trick-mirrors of the historian"), or with their cousin, fascination with 'what might have been.' Moreover, in dealing with the medieval period, he pointed out that because of the inferior role science enjoyed within the intellectual system of the time, "what we call 'natural scientists' could hardly be said to have existed then" (ibid. 1985: viii-ix, 10 and 78). Nevertheless, what current scholars take to be retrograde 'Whiggish' tendencies in the history of science -- such as present-centredness, progress through liberation from social or conceptual 'obstacles', and the selection of 'rational' and forward-thinking elements from an individual's oeuvre -- occur frequently and unapologetically in *Origins* (more generally, see Skinner, 1969 and Wilson and Ashplant, 1988).

Butterfield's work was, at least in part, an effort to position history of science at a disciplinary level. At the same time as he argued that the field should properly be the province of historians, so he brought to bear historiographically-informed approaches to the science of the past that were of growing concern to professional historians. He recommended the avoidance of moral judgements about the past as well as a sympathetic engagement with the views and intentions of historical characters. He stressed that historical research was a craft skill; sources had to be examined with great care but they also had to be understood in their proper contexts. Butterfield argued strongly in favour of what he called 'general history', by which he meant the need, where possible, to take a broad view of a very wide historical field, although he thought that a final grand narrative, or a true total history, was the province of God alone. History was complex, and both socio-economic and intellectualist approaches had to be taken into account in order to give a balanced narrative of the past. In such a view, no one part of history could dominate over others as constitutional history had done, and no field should be excluded. Nevertheless, like virtually all of his contemporaries, Butterfield believed that science was a unique example of progress in human endeavour, and placing science in its historical contexts could not diminish the fact that it had made fundamental and true discoveries about nature (Mayer, 2000, and particularly Jardine, 2003, 127, 129-32 and 135).

As Jim Bennett has shown, the institutionalisation of HPS at Cambridge was closely bound up with the inauguration of a specialist museum for housing Whipple's books and instruments. Whipple presented these to the university in November 1944, but only in February 1946, when a new History of Science Committee was formed, was a proper body set up with the joint task of promoting history of science and of curating Whipple's collection. Rupert Hall was appointed as the first curator of the collection in June 1948, giving a series of lectures in the following year. He was chosen as the first lecturer in the history of science in 1950 and made his name with the publication in 1952 of his strongly anti-Marxist thesis on early modern ballistics, erasing the links made by Hessen between the military need for improved gunnery and contemporary research in ballistics. In 1954 he published an influential work on the Scientific Revolution, strongly defending the notion that transformative episodes in seventeenth century astronomy and physics collectively marked a major development in human progress. However, Hall found it difficult to fit the life sciences into his account, and argued that there was no 'scientific method' that characterised all the different forms of natural knowledge. In the next three decades he was one of the most vocal critics of Hessen-type (i.e. Marxist) efforts to explain science in terms of its socio-economic contexts, although these were becoming outdated by the 1960s. Hall remained at Cambridge until 1959, when he was replaced by Michael Hoskin (Mayer, 2000: 676 and 680-1; Mayer, 2004: 55-6 and 60; Bennett, 1997, 40-1).

Despite the move to make history of science a province of history, in 1951 Cambridge was granted an undergraduate programme in HPS available for one year as part of the Natural Sciences Tripos, along with a more advanced Certificate in History and Philosophy of Science that could be taken by final year students or postgrads. The Postgraduate Certificate was aimed at both science and humanities students, while the UCL MSc was normally open only to those who already had an excellent degree in a scientific subject. At Cambridge, HPS gained the status of a department in 1972, and students were able to concentrate on the subject in their second and third years (Parts 1B and II respectively), while the postgraduate certificate became an MPhil degree (Crombie and Hoskin, 1963: 757-62; Bennett, 1997: 43-4).

As with Cambridge, the institutional origins of history of science at Oxford were closely bound up with the consolidation of a world-class collection of scientific instruments. The prime mover in setting up the study of the history of science at Oxford was R.T. Gunther, a lecturer in the natural sciences at Magdalen College Oxford. During the First World War, Gunther began to draw up inventories of extraordinary instruments held at the various Oxford colleges and after he retired in 1920, he started a multi-volume celebration of the history of science at the university. By 1925 he was curator of a major collection of scientific instruments (given by the manufacturer Lewis Evans) on the top floor of the Old Ashmolean Building, next to the Sheldonian Theatre. A decade later, after intense lobbying by Gunther, the building and its contents were given the title of 'Museum of the History of Science'. His position as curator was taken over by Frank

Sherwood Taylor, a lecturer in inorganic chemistry at Queen Mary College London, who had recently completed a doctoral dissertation under Singer on Greek alchemy. Sherwood Taylor made strenuous efforts to institute a more formal standing for history of science at Oxford, which would be based in the museum and its instruction closely allied to its contents (Bennett, 1997: 31-6 and 46).

Despite interest from the faculties of Theology and Modern History, Sherwood Taylor's plans in 1946 to create a department of history of science with five members of staff did not come to fruition, and he left to become director of the Science Museum in 1950. However, the chemist and assistant demonstrator Stephen Mason gave lectures on the history of science between 1947 and 1953, and in the latter year published his popular *History of the Sciences*. On the back of his two seminal works on medieval natural philosophy, Alistair Crombie moved from UCL to became the first lecturer in the history of science at Oxford in 1953, and remained a dominant presence at the university over the next three decades. Originally a research scientist in zoology specialising in inter-specific competition, Crombie followed Duhem in attacking the 'discontinuist' prejudices of most historians of the Scientific Revolution. He argued that the origins of modern attitudes to experiment and scientific method were to be found in the medieval work of men like Roger Bacon and Robert Grosseteste, as well as in the practical manipulation of nature performed by the Renaissance artist-engineers. At the end of his career, Crombie argued in a multi-volume work that Western science was characterised by six 'styles' of scientific practice, which were either absent or present in only a limited extent in the approach to nature employed by non-European cultures (Crombie, 1994; Iliffe, 1997 and Mayer, 2004).

5. The Philosophy and Sociology of Science

5.1 The Rebirth of the History and Philosophy of Science

At the start of its career as an academic subject at UCL, the close institutional connections between history of science and philosophy of science were premised on the notion that science (usually understood as Western science) was a unique form of human rationality. What was at issue for philosophers of science was how science was distinctive from other forms of human activity, that is, how it progressed through operations that elevated it above and extricated itself from the base interests and drawbacks of the wider culture. From a similar standpoint, the task for most historians of science was to understand how pure science had come to distinguish itself from the morass of superstitious metaphysical, occultist and religious opinions (or 'obstacles') that held back scientific advance. Inevitably, attention fell on a handful of Great Men, selfless geniuses whose attitudes to science were abstracted from their other concerns and held up as exemplary of scientific practice. History of science and philosophy of science existed in harmony so as long as these core assumptions remained within the profession.

Philosophy of science gained a new lease of life after the Second World War and became a central element of the HPS programme in Cambridge. Richard Braithwaite taught the subject until 1953, the year his *Scientific Explanation* appeared, and he was chosen as Knightbridge Professor of Moral Philosophy. This paved the way for the appointment of Norwood Russell Hanson as the first lecturer in the subject but he only remained in the position until 1957 (a year before his influential *Patterns of Discovery* appeared), when he moved to the University of Indiana. Having been deported from England to Australia as a ‘dangerous alien’ in 1940, Gerd Buchdahl replaced Hanson in 1957, also taking over the role of curator of the Whipple Museum when Hall left in 1959. He specialised on Kant but wrote on a broad range of subjects related to metaphysics and philosophy of science (the title of his 1969 book). Committed to the notion that historians should know philosophy of science and vice versa, Buchdahl founded the journal *Studies in the History and Philosophy of Science* in 1970 with the philosopher of science Larry Laudan to create a home for articles that explored the intimate connections that existed between the history of science and the philosophy of science. Two years later he became the first head of department (Jardine, 2001).

A major presence at Cambridge for two and a half decades was Mary Hesse. Hesse began her career in 1951 as a lecturer in mathematics at Leeds before turning her attention to HPS in the mid-50s. Having taught the subject at UCL for five years she moved to Cambridge in 1960, and remained there until her retirement in 1985. Much of her work in HPS aimed to show the overwhelming influence of explicit and intuitive models adopted by scientists in developing and accepting theories. These views were outlined in *Forces and Fields* of 1961 and *Models and Analogies in Science* of 1963. Along with *The Structure of Scientific Inference* of 1974, this work was based on a belief on the more traditional need to integrate history of science and philosophy of science. In the second half of her career she moved away from more traditional HPS and ventured more broadly to write influential articles about the work of Jurgen Habermas and the sociology of scientific knowledge.

By the 1950s, the major centre for the study of philosophy of science in Britain was at the London School of Economics (the ‘LSE school’), which was dominated by the presence and philosophy of Karl Popper. Popper had been associated with (but by his own admission, never a member of) the logical positivist Vienna Circle in the 1930s, but his philosophy of falsificationism was at odds with the dominant views of the leading logical positivists. In his *Logik der Forschung* of 1934 (translated and expanded in 1959 as *The Logic of Scientific Discovery*) he argued that science had progressed in the past through the brilliant and unlikely conjectures of a small number of great thinkers, whose theories had survived a series of demanding tests.

Against the logical positivist view that the goal of scientific work was to confirm or verify scientific statements, Popper argued that great and fertile theories were risky hypotheses that had been initially implausible, but which had survived a hostile series of tests. Concurring with the Vienna Circle that a central task of philosophy was to *demarcate* ‘scientific’ from ‘meaningless’ theories and statements, Popper went on to distinguish those theories that could be falsified from those, such as Marxism and

Freudianism (which he termed 'pseudo-sciences'), which could not. Nevertheless, he rejected the logical positivist line that all metaphysical claims or religious beliefs were meaningless. Popper argued that the properly *scientific* attitude was critical and destructive, and scientists had a moral obligation to try to 'falsify' all theories, including their own, and even the deeply entrenched meta-structures of Darwinianism and Newtonianism. The former remained robust, and indeed in later works Popper believed that natural selection offered a good model for accounting for how certain theories triumphed over others. Newton's conceptual edifice, on the other hand, had been shown to be true only in extremely limiting cases of what, after Einstein, was understood to be a universe with a curved space-time.

Popper moved to University College, in Christchurch New Zealand in 1937 and in the early 40s wrote drafts of *The Poverty of Historicism* (part of which was published as a long article in 1944 but which did not appear as a book until 1957) -- an attack on the totalitarian views of Plato, Hegel, and Marx -- and an offshoot of this project, *The Open Society and its Enemies*, which was published in 1945. With reference to the contemporary regimes of Hitler and Stalin, *The Open Society* was a major defence of a liberal, democratic society in which political pluralism and the opportunity for public criticism played major roles. With the support of his friend Friedrich Hayek, whose political views concurred with Popper's, he was made Reader (and in 1949, Professor) in Logic and Scientific Method at the LSE, where he attracted a number of historically-inclined students and colleagues such as Abdelhamid Sabra, Imre Lakatos and Paul Feyerabend.

5.2 The Structure of Science and the Dangers of Dogma

The harmonious relationship between history of science and philosophy of science was soon to be shattered. In July 1961 Alistair Crombie organised an 'International Conference on Scientific Change', whose proceedings were subsequently published as *Scientific Change*. The conference offers a fascinating snapshot of the discipline at a moment when it was about to be changed by powerful and novel sociological attitudes to the history of science. In one of the sessions the American historian Henry Guerlac praised "the leadership of those men who have taught us how to focus upon the evolution of key scientific ideas and concepts," by which he meant Duhem, Koyré and Burtt. This, according to Guerlac, necessarily took historians closer to the philosophy of science, which was a good thing, but its focus on concepts had left it with an unhealthy flavour of 'idealism' and 'super-rationalism'. This ran the risk of becoming a 'new specialism' in much the same way that the history of technology had branched off to become a sub-field. Guerlac rejected the disdain directed towards the history of craft practice, apparently on the grounds that it was tainted with deleterious ideological elements. What was required was a more totalising or synthetic picture of the development of human culture, which presented the history of science as part of the "larger cultural fabric" (Guerlac, 1963: 808-11).

Koyré himself responded to Guerlac's plea for generalism by stating that he agreed with him on this point, but that he also believed that specialisation was "the price to be paid" for progress to take place in any field. Indeed, Koyré argued that the links between 'pure' and 'applied' knowledge were a peculiar feature of the modern world, and that in the Ancient World, the condition of organised theoretical knowledge, or *episteme*, had been that members of the leisured classes devoted themselves to theory. Science had developed in the West from this disengaged basis, and not elsewhere, such as Persia or China, where there were great bureaucracies that were always "hostile to independent scientific thought" (Koyré, 1963: 850-3, 855-6 and 856-7).

The same conference was the occasion at which Thomas Kuhn outlined some of the most radical features of his theory of scientific revolutions. Where Butterfield and others had argued that the history of science should be properly historical, Kuhn now argued that scientific practice itself should be analysed as if it was fundamentally *similar* to other sorts of human activity. In 1947 Kuhn had been asked by the President of Harvard, J.B. Conant, to teach a course on the origins of seventeenth century mechanics and in preparation for this he went back to discussions of motion in Aristotle's *Physica*. At first his reaction was negative: "Even at the apparently descriptive level, the Aristotelians had known little of mechanics: much of what they had had to say about it was simply wrong." Why was Aristotle so apparently misguided in mechanics, when he had said so many significant things in other areas? Kuhn realised that Aristotle had been dealing with the problem of *change in general* – of motion, or of generation and corruption -- and that the problems raised by mechanics were a small subset of a much broader topic. Kuhn noted that he had learned to read Aristotle in a new way and in Aristotle's own terms, that is, he had begun to divest himself of the present-centred attitude condemned by Butterfield (whose book he read soon afterwards). His own personal conversion experience was thus a de-enactment of the much larger historical change that had taken place in moving from the Aristotelian to the Newtonian perspective (Kuhn, 1977: xi-xiii).

Kuhn published a major work on the Copernican Revolution in 1957, but his influential historiographical insights arrived in the years that followed. He came to recognise that revolutionary episodes in the history of science were extremely rare, and that most of science was "a complex and consuming mopping-up operation that consolidates the ground made available by the most recent theoretical breakthrough and thus provides essential preparation for the breakthrough to follow." After trying unsuccessfully to account for the type of 'consensus' that had to exist for most scientific work to take place, Kuhn's 'Eureka' moment in 1959 was the realisation that ordinary science required the routine training in solving certain problems in which various scientific terms appeared. Sharing these paradigmatic ways of dealing with problems was the basis of the wider scientific community, and this expertise was the basis for further research (Kuhn, 1977: xiii-xxiii and 188).

These ideas were developed in two papers from that year entitled 'The essential tension' and 'The function of dogma in scientific research'. In the first he argued that doing science required managing a conflict between the demand for critical, innovatory thinking, and the need for working with theories, concepts and standards that were accepted by the wider community: “only investigations rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one”. In ‘The function of dogma’ he argued that “a deep commitment to a particular way of viewing the world, and of practising science within it,” was not the result of individual limitation but was characteristic of any mature scientific practice. This complex mixture of worldview and training began with elementary and then advanced textbook instruction, both of which were constrained by a much deeper ‘paradigm’ that continued to guide most scientists throughout their careers (Kuhn, 1977: 225-40 esp. 227; Kuhn, 1963: 348-9, 351).

Kuhn’s1962 book *The Structure of Scientific Revolutions*, was published under the auspices of the International Encyclopedia of Unified Science, a series edited by the leading logical positivist Otto Neurath. Despite this unlikely context, it revealed powerful and novel ways of looking at the history of science, and became the landmark book in the field. For Kuhn, science was generally characterised by long periods of ‘normal science’, during which practitioners worked within a paradigm that structured, in both general and specific ways, how scientists thought about and investigated the natural world. In time, paradigms would generate an awareness of anomalies that scientists cannot address in a satisfactory way, and a ‘crisis’ arises: “all crises begin with the blurring of a paradigm and the consequent loosening of the rules for normal research.” A problem that cannot be solved in a specific paradigm “is labelled and set aside for a future generation with more developed tools.” At this point there is a fundamental realignment of the discipline and younger scholars engage in what Kuhn called ‘revolutionary science’. This would become a dominant mode of viewing the world and of solving ordinary problems, and thus would become the next form of ‘normal’ science (Kuhn, 1970: 66-91, esp. 84).

Kuhn argued that because paradigms generated all the beliefs, techniques and guides for practice associated with theories, different paradigms were ‘incommensurable’ with each other. In evaluating what it was like to experience the world in a new paradigm, Kuhn borrowed from the work of a number of scholars in different fields, including Butterfield, Hanson, Ludwik Fleck, Michael Polanyi and gestalt psychologists. The latter provided him with the notion of the ‘gestalt switch’, a visual metaphor that captured the fact that the same basic text or image could be ‘seen’ in different ways. These sources allowed Kuhn to criticise older ‘discovery stories’ that treated such episodes as ‘aha’ moments, for discoveries always occurred to the prepared or trained mind, primed with new conceptual categories. As Kuhn recognised to some extent, his emphasis on the primacy of the visual elements within paradigm change still conceded too much to the previous account. It hardly captured the momentous changes that occurred during a revolution and Kuhn noted that “when the transition is complete, the profession will have changed its view of the field, its methods and its goals” (Kuhn, 1970: 52-65 and 85).

In the final chapter Kuhn admitted that he could not accept the usual accounts of scientific progress, and he argued that the group whose paradigm wins out in a period of extraordinary science usually wins the right to call what they have achieved ‘progress’. Kuhn did not wish to endorse a form of epistemological anarchism, and progress was not *simply* a case of the strongest imposing their will on history. Still, his solution was decidedly sociological, and he claimed that the arbiters of progress were the close-knit scientific community who were "uniquely competent" to pass judgment. On the other hand, he also offered an olive-branch to more traditional believers in progress by stating that a new paradigm should be able to solve outstanding problems while preserving the problem-solving ability of its predecessor. Perhaps most striking was Kuhn's view that science did not progress towards some 'truth'; like Darwin's theory of evolution by natural selection, science did not progress *towards* some goal. Indeed, Kuhn concluded that the evolutionary view of scientific progress best captured what he was arguing in the book (Kuhn, 1970: 160-73, esp. 168 and 172-3).

5.3 Popper versus Kuhn

By the mid-60s *The Structure of Scientific Revolutions* had made Kuhn widely known outside his field, and the term 'paradigm' was gaining wide currency in other areas, especially the social sciences. As Kuhn himself recognised, this was partly because any field that displayed the characteristics of progress would for that reason be scientific (Kuhn, 1970: 162). However, the book was also subjected to a number of criticisms, perhaps the most important being the claim that while Kuhn's approach was sociological, it was not in the least contextual. As Kuhn later admitted, he had done little to show how the values and practices of science at any given period were related to contemporary issues in the religious or socio-economic spheres (Kuhn, 1977: xv).

Other criticisms came from philosophers of science, who lamented the fact that Kuhn had provided no rational means of showing how one theory could be shown to be better than another. By failing to give grounds for preferring one theory to another, he was engaging in a form of relativism according to which all theories were equally valid. If this were so, then science could no longer be a central plank of rationality. The International Colloquium for the Philosophy of Science that was held at Bedford College, London, in July 1965 was noteworthy for pitting Kuhn directly against Popper and for demonstrating the vast gulf that was opening up between descriptivist, sociologically-informed history of science, and prescriptivist philosophy of science (i.e. in which philosophers identified the scientific method and recommended best practice to scientists).

Kuhn's paper began by stating that his own and Popper's views had much in common, not least in rejecting the possibility – required by logical empiricists -- of producing any neutral observation language. Nevertheless, Kuhn showed that what Popper took to be characteristic of best scientific practice, epoch-making tests of theories where an old way of looking at the world was put to death, were actually incredibly rare and unrepresentative episodes. Professional scientists were those who had been trained to think in a particular way, and it was 'normal' science (with puzzles or questions offering paths towards research) that marked out science as being different from other forms of human activity. Moreover, Kuhn stressed the importance within science of possessing a marked degree of *commitment* towards dominant paradigms or traditions. Theories were not rejected lightly, or at all, by their proponents, and Popper's notion of falsification was false both as a description of practice and also as a desirable goal of scientific activity. Significantly, Kuhn defended the view that there were no 'objective' extra-theoretical criteria or points of view by which theories could be compared with one another, or by which scientific progress could be explained (Kuhn, 1970: 5-6, 9-10, 13-14 and 19).

Popper's reply in the same volume shows how fear of totalitarianism and irrationalism lay at the heart of his project. Popper's response commences with the compliment that Kuhn's "criticism" of his views was "the most interesting" he had encountered, though there were various points where Kuhn had 'misunderstood' or 'misinterpreted' him. He knew that scientists who did 'normal' science worked within "an edifice" that provided a scientist "with a generally accepted problem-situation into which his own work can be fitted." The fact that Kuhn had missed this, Popper claimed with irony, showed the truth of the general anti-empiricist point that everyone approaches external objects, such as texts and the world, "in the light of a preconceived theory." However, Popper stated, one should feel sorry for 'normal' scientists; throughout history such people were taught badly and in a "dogmatic spirit", uncritically learning a technique and ultimately becoming an 'applied scientist' (Popper, 1970: 51-6).

Such views, Popper asserted, were dangerous to science and to civilization. In any case, Kuhn's view that there had been dominant paradigms within scientific fields for extended periods of time was also false, for various theories of matter, e.g., had always co-existed and were in a sort of "constant and fruitful discussion" with each other. Some dogma, or tenacity, adopted as an attitude to one's own work, was good, but too much smacked of totalitarianism. Worst of all, Kuhn was a "historical relativist", who thought that the theories of scientists were inexorably constrained by or bound up within a framework that provided both theories and any criteria that could be brought to bear to test them against other theories or the external world. They could not be objectively compared with one another and were thus 'incommensurable'. For Popper this was absurd irrationalism and he argued otherwise: if it was true that we were "prisoners caught in the framework of our theories", then it was always a temporary incarceration, from which we could break free at any time. The aims of science, whose best examples were and ought to be *extraordinary*, could not be discerned from analysis of sociology, or psychology. Ultimately, the moral and political values associated with the pursuit of falsifiable scientific theories were diametrically opposed to those found in Kuhn's account of science, which according to Popper amounted a sort of paean to totalitarianism (Popper, 1970: 56-9).

6. Revolutionary History of Science

6.1 Science and the 'Occult'

Even as Popper gave his paper at the 1965 conference, the discipline was opening its doors to a tidal wave of irrationalism. Historians of science had long known that virtually all the heroes of the Scientific Revolution had been committed Christians, while a substantial proportion of the same Great Men had believed in and practised astrology and alchemy. Historians could easily separate the fertile kernel of progressive research from the unfortunate chaff of superstitious interests that had been obstacles to science in the past. This demarcation between rational science and irrational or occult belief was deeply unsatisfactory, since it implied that superhuman greatness existed alongside catastrophic intellectual frailty in a single mind. Nevertheless, historians of science in the 1940s and 50s were as unwilling to contemplate that there could be conceptual or any other links between rational and occult beliefs, as they were to allow that science might be conditioned or even influenced by socio-economic forces.

Ambix was a forum for work on alchemy, although many historians of chemistry were prepared to see the practice as merely a deviant form of early chemistry. As for magic, it was firmly associated by modern anthropologists with the ‘primitive’, and accounts of the Renaissance revival of interest in the subject were deeply unhistorical. The most impressive achievement in the history of the occult sciences was undoubtedly Lynn Thorndike’s 8 volume *History of Magic and Experimental Science* (1924-58), whose title made it clear that the twin concerns of magic and experiment were bound up with one another. Nevertheless, professional historians of science by and large shared the view that the Renaissance attempt to understand magical symbols and powers had been a perverse and scientifically infertile activity that could have nothing to do with proper natural philosophy, or for that matter, religion (Copenhaver, 1988: 79-80). However, in the 1960s historians came to terms with the fact that not only was the magical tradition bound up with these same pursuits, but that these were linked them together with Neoplatonist themes as well as astrological, alchemical and other practices. Renaissance and indeed early modern writings in this vogue were also governed by the view that a form of perfect knowledge (an *occulta philosophia*, linked to a *prisca theologia*) embracing all these traditions had been revealed to Mankind at the beginning of time, but had subsequently been corrupted and lost.

In the1960s, two major revolutions in historiography blasted a hole in positivist assumptions about the separateness of science from other more unfortunate beliefs. The first concerned the new emphasis on the significance of the role of ‘occult’ sciences such as alchemy, astrology and magic, while the second involved taking seriously the beliefs of men and women who did not normally feature in accounts of early modern science. Since the end of the war, the Warburg Institute in London, which became part of the University of London in 1944, had become the focus for Medieval and Renaissance intellectual history, art history and history of philosophy. Aside from *Ambix*, the *Journal of the Warburg and Courtauld Institutes*, also founded in 1937, was an outlet for historians who believed that general worldviews, at least in the Renaissance, had traversed the boundaries between ‘scientific’ and ‘nonscientific’ that underlay most academic history of science. The effect of the renewed attention to the disparaged intellectual traditions was to force more mainstream historians of science to rethink the relations between the scientific and the occult.

The doyenne of the Warburg was its Reader in the History of the Renaissance Frances Yates, who had published on a number of topics such as Raymond Lull, mnemonics (techniques for improving memory) and Renaissance French academies. Her *Giordano Bruno and the Hermetic Tradition* of 1964 blazed a pioneering trail into the intellectual contexts of Renaissance and early modern natural philosophy. Building on the work of scholars such as A.–J. Festugière, Eugenio Garin and D.P. Walker, *Giordano Bruno* located its eponymous hero in the context of a movement Yates called 'Hermeticism'. From the late fifteenth century Italian elites supported the printing of the works of a man, Hermes Trismegistus, who had apparently supplied Moses with Egyptian knowledge of both nature and magic, including the capacity to manipulate natural forces and spirits for the benefit of humanity. This eclectic animist, astrological, alchemical, Cabalist, Pythagorean and magical knowledge was 'rediscovered' by Renaissance Neoplatonist scholars such as Marsilio Ficino and Pico della Mirandola, who developed their own varieties of the 'Hermetic' philosophy and lauded the power of the 'operator' or 'magus' to control natural powers for the good of humanity. In the late sixteenth century, Bruno bought wholesale into the Hermetic tradition, adding to it the heretical ideas that the cosmos was infinite and was populated by innumerable worlds, all teeming with life. It was for these ideas, as much as for some scurrilous views about Christ, that Bruno was burned at the stake by the Inquisition in 1600 and earned his title as a martyr of science.

A number of historians criticised Yates for attributing an unwarranted coherence to the notion of Hermeticism, but she wrote with *brio* and succeeded in placing Bruno in more relevant contexts than had hitherto been the case. She had made her subject seem very different from a modern day scientist or astronomer, and she had shown how his more palatable ideas could not be wrenched from his other interests and theories. His Copernicanism, for example, was not the result of some rational decision but was a symbol of his commitment to the Pythagorean aspect of Hermeticism. Underlying the Hermetic philosophy, as Yates saw it, was the figure of the magus – a precursor of the virtuoso experimental philosopher of the seventeenth century, and the recognition that for individuals like Bruno, natural philosophy was in part the rediscovery of a pristine knowledge or *prisca sapientia*. Yates's thesis went further than other accounts of the occult philosophy in arguing that it lay at the very heart of the Scientific Revolution.

Although it could not be classified as 'occult' science, it is perhaps no coincidence that the 1960s also witnessed an explosion of academic interest both in Aristotle's 'scientific' writings and in Ancient Greek science in general. This was largely inspired by the writings of Geoffrey Lloyd, who in 1966 published a book on the role of the concepts of polarity and analogy in Aristotle's work. In two works of the early 70s, Lloyd wrote general histories of Greek science both before and after Aristotle that became set books in most courses on ancient science. He pointed out that the term 'scientist' was problematic for describing early Greek investigators of nature, since many different sorts of individual were engaged in producing theories explaining the world around us. Nevertheless, Greek naturalists before Aristotle were concerned with the empirical study of nature and not merely with attaining a theoretical understanding. One could not ascribe economic motivations to these people, since the majority of people who discoursed about the natural world were either of independent means, or teachers who were not concerned with earning money for its own sake: for most classical writers, "the dominant theme, found in many variations, is undoubtedly that the inquiry concerning nature is its own reward" (Lloyd, 1970, 125-36).

6.2 Popular Science

The second major change in the way history was practised was influenced by social histories 'from below' such as E.P. Thompson's *The Making of the English Working Class* of 1963. This involved taking seriously the experiences of the ordinary people whose lives and beliefs were not usually considered in the history of science. Three works stand out in this period. Christopher Hill's *The Intellectual Origins of the English Revolution* of 1965 combined an account of the rise of puritanism with an analysis of the significance of military patronage and research from the Elizabethan period onwards. Hill was an ex-communist who was arguably the dominant historian of the English Revolution, a period and extended event that exercised the same hold over general Anglo-American history as the Scientific Revolution did in the history of science. Despite the conceptualist title of the book, Hill argued that the values that were to bear fruit in the 1640s and 50s were forged in workshops and dockyards, in the writings of ordinary craftsmen, and in the work of professors of geometry and astronomy at Gresham College.

Keith Thomas's *Religion and the Decline of Magic*, which appeared in 1971, was a gargantuan attempt to explain how older and 'popular' beliefs were replaced by more modern attitudes. Bolstered by overwhelming amounts of data, the book examines different sorts of interests and associated practices, such as medieval religion, magic astrology, alchemy and witchcraft, that were interrelated at a number of levels and which were believed by both elites and lay people. Drawing to some extent from contemporary developments in social anthropology, Thomas gave a functionalist account in which astrology, religion and magic were said to offer to help individuals when they were faced by misfortune. A central plank of Thomas's thesis is that protestantism denuded the traditional religion of many of its magical or supernaturalist elements, thereby allowing magic and astrology to flourish. However, by the end of the period, the greater social importance of religion facilitated its triumph over animist magic. There was a brief period of flirtation between magic and science (here Thomas cited Yates on the hermetic philosophy), but the victory of religion was virtually assured by the advent of the mechanical philosophy, the gradual acceptance by elites that the world was governed by natural laws, and a new critical attitude to evidence and theory alike. The change that took place was thus primarily 'mental', and ultimately, the early modern period was defined not by technical achievement, but by 'aspiration', "the expectation of greater progress in the future" (Thomas, 1971, 755, 761-5, 769-70, 773, 788-91).

Finally, Charles Webster's *Great Instauration* of 1975 was an extraordinary account of Puritan-inspired interest in scientific and technological work in the politically turbulent decades between 1626 and 1660. Webster argued that millenarian puritan values merged with Baconian utilitarian attitudes to produce a series of efforts to transform society via educational reform, agricultural and medical improvement, and technical and scientific progress. Like Thomas's work, this was an extensive and meticulously researched argument, and like Hill's thesis, Webster located the geographical centre of these developments outside the universities of Oxford and Cambridge. That is not to say that radical reformers did not come from within the two institutions, nor that there was not a new vogue for mathematics and natural philosophy inside them. However, the key figures, whose plans enjoyed state support and which were partly realised in the 1640s and 50s, formed a constellation around the London-based intelligencer Samuel Hartlib. The Hartlibians were too firmly connected to the fortunes of the Republic for many of its key members to survive the Restoration, yet others like Wren, Wilkins and Boyle made a relatively seamless transition to membership of the Royal Society (founded in 1660).

The Great Instauration was on firmer ground than was Robert Merton in identifying individuals who could conceivably be said to have had puritan values, although there has long been debate over whether the term possesses any explanatory power. In any case, the generality of Puritanism and Science theses inevitably results in people whose views could not conceivably be called puritan, being invoked as fellow travellers. No one who reads *The Great Instauration* can fail to be convinced of the intimate conjunction between millenarian protestant and social reformist values in the works of most members of the Hartlib Circle. Nevertheless, a signal feature of the natural philosophy of the later seventeenth and eighteenth centuries was that an empiricist, Baconian strain could be taken up with fervour by social and religious conservatives. In 1980 Margaret and Jim Jacob published a major article that demonstrated that the bulk of the early Royal Society were Anglicans who endorsed an epistemology favouring a wide community of practitioners. While the vast majority of the Society expressed disdain for atheistic views, in fact the Society created a space where troublesome discussions about religion and politics were banned.

6.3 The Other Newton

The heroes of the Scientific Revolution were by no means immune from the reorientation in historiography, which allowed historians to reassess the significance of beliefs that had long been dismissed as unfortunate or irrelevant. The hard case for this was Isaac Newton, lauded by most as the founder of modern physics and the totemic figure of Enlightenment in general. On both sides of the Atlantic, Newton's great triumphs in mathematics, optics and physics enjoyed pride of place within the nascent discipline of history of science. Historians engaged on a number of projects devoted to interpreting and publishing Newton's published and unpublished works, while philosophers of science naturally took his extraordinary triumphs to be exemplary of progressive and revolutionary science. Under the general editorship of H.W. Turnbull (succeeded after his death by Rupert Hall), *The Correspondence of Isaac Newton* appeared in 7 volumes between 1959 and 1981, while in the late 1950s D.T. 'Tom' Whiteside began his monumental edition of Newton's mathematical papers, an undertaking that appeared (with the assistance of Michael Hoskin and Adolf Prag) between 1967 and 1984. Other collections of Newton's writings in physics appeared in the early to mid-1960s under the editorship of John Herivel, and the Halls, while Cohen and Koyré's variorum edition of Newton's *Principia* appeared in 1972 (with Cohen's 'Introduction' to Newton's *Principia* appearing the previous year).

Despite this, historians had long been aware of another Newton. It was known that Newton was a student of theology, and that he had spent a great deal of time immersed in alchemical studies, while all good commentators lamented the time Newton had wasted on his duties as Warden and then Master of the Royal Mint (from 1696 to his death in 1727). Reconciling these interests with Newton's greatness as a mathematician and scientist had taxed his greatest biographer, David Brewster, when he had composed his *Memoirs* of Newton in 1855. In 1936, a sale at Sotheby's had released about 5 million words of Newton's alchemical, personal and theological writings into private hands. The bulk of these were bought by the economist John Maynard Keynes, who purchased most of Newton's alchemical writings, and the scholar and collector Abraham Yahuda, who bought the majority of the theological papers. Despite being taken up with wartime work, Keynes composed a short paper 'Newton the man' which he delivered at Trinity College in December 1942, to celebrate the tercentenary of Newton's birth. Keynes provocatively concluded that Newton was not the first of the moderns, but the last of the Renaissance magi. The Yahuda papers were not to be made available to scholars until the end of the 1960s, and are only now being assessed in their entirety.

In an act of great generosity, Keynes bequeathed his papers to King's College Cambridge, where they became available to first historian to take seriously the non-scientific interests of Isaac Newton. In his *Isaac Newton, Historian* of 1963, the intellectual historian Manuel examined the Keynes papers as well as a bundle of papers at New College Oxford that constituted Newton's substantial researches into ancient chronology. Manuel showed that Newton was a serious historian – indeed, an expert in the field -- whose work in this area was no senile or dilettantish hobby. At a time of social revolution in the West, the historical Newton was adapted to suit the times. In a seminal essay of 1966, Piyo Rattansi and Ted McGuire showed that Newton was a firm believer in the *prisca sapientia*, and that in the early 1690s he had planned a second edition of the *Principia Mathematica* that would show how ancient poets and other writers had veiled their knowledge of Universal Gravitation and other Newtonian truths in various mysteries. The next year David Kubrin showed how organicist elements informed Newton's general cosmology and in 1968 McGuire showed how Newton's anti-mechanist notion of 'active principles' played a central role in his natural philosophy. In the same year Manuel published a 'Portrait' of Newton, building on the historical studies of the psycho-historian and psychoanalyst Erik Erikson to give new insights into the deep sub-structures of Newton's warped 'personality'. Newton now looked much more like Bruno than he did Michael Faraday. Once the shock value of this revolution had abated, research into Newton's alchemy, and more recently into his voluminous theological writings, became the dominant focus of research.

7. An Expanding Discipline

7.1 The Northern Seminar

The 1950s and 60s witnessed a dramatic expansion in the number and size of British universities. A number of influential figures argued that post-war higher education should be producing broadly educated scientists – who might in the future be part of a technocratic elite -- and many universities attempted to cater for this need by creating innovative interdisciplinary courses. A related opinion held that scientific creativity could be triggered by allowing scientists to discuss different sorts of issues from the more mundane problems they faced in their ordinary work. In many cases, senior industrialists agreed that the narrowly focussed specialist was less likely to engage in creative or independent thought. On the other hand, many scientists continued to hold a disdain for the humanities in general, and for its inability to ameliorate the human condition in comparison with the benefits of science, technology and medicine.

The most powerful attack in this direction was launched by the scientist and novelist C.P. Snow, who argued in his book *The Two Cultures* that 'literary intellectuals' and scientists no longer communicated: "Between the two a gulf of mutual incomprehension – sometimes (particularly among the young) hostility and dislike, but most of all lack of understanding." Snow was anything but even-handed in his treatment of the two cultures, condemning elements of the literati of being self-indulgent, ignorant of the benefits brought to humankind by science and technology, and even indirectly responsible for Auschwitz. On the other hand, scientific culture "contains a great deal of argument, usually much more rigorous, and almost always at a higher conceptual level, than literary persons' arguments." Snow thought that this state of affairs was common to all parts of the western world, but that it was particularly entrenched in England, where narrow specialisation was built into the educational system. Indeed, his short book, based on lectures originally given at Cambridge in 1959, was a plea for a change in the British educational system so that an awareness of scientific principles could be much more widely distributed in the highest levels of government as well as in society as a whole. Britain needed more 'alpha plus' scientists and engineers, and more appreciation among the general public of what science had achieved (Snow, 1960, 4, 8-9, 13, 35-6).

Snow's provocative thesis was roundly condemned as being a philistine and/or inaccurate depiction of the role and purpose of humanities, and many critics argued that the divide between the two cultures was neither as large as Snow represented it, nor was it unbridgeable. For those who wanted to address the question of how to produce a more broadly educated scientist, promoting the history of science in the university curriculum was an ideal solution. In virtually all of the courses created in the 1950s and 60s, the emphasis was on exposing those students who were pursuing science degrees to the humanities, and not the other way round. A clear majority of the post-war professionals in the field had been trained in the sciences, and the expansion of the subject was most obvious in the provincial institutes and universities with strong historical links to industrial settings (Mayer, 1999: 232 and 234).

In the 1950s there was substantial interest in the history and philosophy of science at the universities of Leeds and Manchester, and the Northern Section of the BHS met regularly (switching meetings between the two locations), becoming the well-known ‘Northern seminar’ in the 1960s. The seminar also embraced scholars from neighbouring universities, such as Jack Morrell at Bradford. At University of Manchester Institute of Science and Technology (UMIST) emphasis lay almost entirely with the history of technology. The subject was taught by Donald Cardwell (who left Leeds to head the new Department of the History of Science and Technology in 1963), Arnold Pacey and Richard Hills, who published pathbreaking works on the place of technology in society, the relationship between science and technology in the Industrial Revolution, and the history of steam power. For two decades, UMIST was the world leader in the history of technology.

In the 1950s and 60s a number of philosophers, scientists and historians showed an interest in the field at Manchester University. These included the Marxist theoretical physicist Léon Rosenfeld, the Hungarian refugee Michael Polanyi, and Wilfred and Kathleen Farrar, the latter historians of nineteenth century chemistry. Concerned by Bernal’s promotion of centrally organised or planned scientific research, Polanyi had been one of the founders of the Society for Freedom in Science in 1941. By the early 50s he had an exalted status, having exchanged his Professorship in Physical Chemistry in 1948 for a chair in the social sciences with no lecturing demands. His major book, *Personal Knowledge* sought to incorporate insights from gestalt psychology to show that all knowledge, especially scientific knowledge, demands the active contribution of a skilful knowing subject. Such knowledge, while personal, was nevertheless objective, and the ideal of a disinterested researcher was unobtainable, unreal and inhuman. Local knowledge, like skill and connoisseurship, was passed on by master to apprentice, and was as necessary for scientific work as it was to industry and other areas of life. Scientific research demanded passion and a skilfulness that was inarticulable, and of which the skilful person was usually unaware (‘subsidiary awareness’). This idea, also closely allied to the later philosophy of Wittgenstein, was especially fruitful for Thomas Kuhn and later sociologists of science (Polanyi, 1958, 49-65).

At Leeds, Asa Briggs and the philosopher Stephen Toulmin attracted Jerry Ravetz and June Goodfield to the philosophy department in 1957, with Donald Cardwell arriving the following year. With the support of Briggs, the history department had introduced a special subject paper on the History of Scientific Thought, entitled ‘Scientific and Technical Change in Britain from 1780-1830’ (Crombie, 1963: 768). However, on Briggs’s departure to Sussex in 1961 the subject was based entirely in the philosophy department under the leadership of Jerry Ravetz, who had taken over the HPS group following Toulmin’s departure in 1959. In the 1960s and 70s Leeds was the most significant centre for history of science in Britain and became home to many prominent British historians of science, including (in order of appointment) Piyo Rattansi, Charles Webster, Ted McGuire, Maurice Crosland (who replaced Donald Cardwell in 1963), Charles Schmitt, Alex Dolby, Robert Olby (who replaced Webster in 1969), John Christie, Geoffrey Cantor and Jon Hodge (Gooday, 2006: 183-6).

7.2 Departments Big and Small

In the late 60s the UCL department teamed up with Imperial College, the LSE, Chelsea College and the Warburg Institute to form the 'Intercollegiate Course in the History of Science, Medicine and Technology, and the Philosophy of Science.' Chelsea College specialised almost entirely in the philosophy of science, while the LSE School produced a number of historians and philosophers of science who would have a major impact in the following decades. The outstanding historian was Abdelhamid Sabra, who went on to teach at the University of Alexandria (1955-62), the Warburg Institute (1962-72), and Harvard University (1972-2003). Sabra published a major work on early modern theories of light in 1967 and thereafter did pioneering work examining the way that Greek philosophy and mechanics was actively appropriated and reconstituted (rather than being passively received) by Islamic scholars in the medieval period.

Having survived the Nazi murder of over half a million Hungarian Jews, Imre Lakatos fled Hungary following the Soviet invasion of 1956. After doctoral study at Cambridge, he was appointed as a lecturer in philosophy of science at LSE in 1960. His most influential work was based on his PhD thesis and was published as 'Proofs and refutations: the logic of mathematical discovery' in four parts in the *British Journal for Philosophy of Science* in 1963-4. Lakatos presented his work in a dialogue form that was meant to imitate the strictly non-logical means by which mathematicians discussed and accepted or rejected various proofs (see Bloor, 1991: 152-4). Superficially similar to Popper's theory, Lakatos's philosophy of science incorporated insights from Kuhn concerning the fact that certain elements of what Lakatos called a 'research programme' constituted a 'hard core' or 'negative heuristic' that would never be falsified by people working within the programme. Nevertheless, theories could be compared rationally since those programmes that concentrated merely on preserving their core, without generating new insights or guiding scientists towards the discovery of new facts, was bound to fail.

Feyerabend, who enjoyed a provocative intellectual relationship with Lakatos, was the most fascinating of the characters to emerge from the LSE. Feyerabend was drawn to the philosophy of the later Wittgenstein in the early 1950s and went to the LSE where he briefly studied under Popper. However, he soon became disillusioned with conventional rationalist accounts about scientific progress. In the 1960s he wrote a number of insightful articles criticising the notion that there was a 'scientific method' that scientists had actually followed (whatever their claims to the contrary) in their work. With increasing gusto he lauded the proliferation of different theories and celebrated the fact that different theories were 'incommensurable' with each other. In 1975 he brought together much of this work in *Against Method*, a provocative account of Galileo's brilliant use of rhetorical devices and epistemological tricks to undermine the presuppositions of his opponents. While Feyerabend cultivated the guises of a Dadaist asserter of the absurd and philosophical maverick, in fact his later work was a sophisticated melange of approaches drawn from sociology, anthropology, history and philosophy of science. Increasingly disillusioned with Western academia, in his *Farewell* *to Reason* of 1987 he offered a trenchant critique of Western science, which arrogated to itself a uniquely valid account of the 'world', whereas it was "one tradition among many and a provider of truth only for those who have made appropriate cultural choices" (Iliffe, 1992: 206).

While LSE boasted the spectacular productions (and personae) of Popper, Lakatos and Feyerabend, other institutions in London offered more conventional fare. In the early 60s, the support of the Rector of Imperial College, Reginald Linstead, who was committed to a much less specialised curriculum for students doing science A-levels, was crucial in securing a chair in the subject within the institution (Gay, 2007: 293-4 and 584n.5). There had been some presence of HPS within Imperial prior to this. For example, in the 1950s Hyman Levy had given some lectures on the history of physics and the civil engineer Alec Skempton had lectured on aspects of the history of engineering. The physicist and mathematician Gerald Whitrow had taught courses in HPS, with a special interest in the history and philosophy of time. However, the arrival of Rupert Hall to the new chair in 1962 and of his wife Marie Boas Hall (who was a senior lecturer) enabled the creation of a Department in the History of Science and Technology, which oversaw a successful postgraduate programme. In the mid-60s it seemed at one point that a sociology of science unit might be created, but as Hannah Gay notes, the new Rector William Penney was concerned that the social sciences were the main source of student radicalism and he did nothing to further the idea (Gay, 2000: 571).

The civil engineer Norman Smith taught the history of technology at Imperial from 1970, but when the Halls retired in 1980, the department was merged with other non-scientific services to make up a Department of Humanities. Simon Schaffer was the resident historian of science between 1981 and 1984, and Jim Secord arrived in 1985. With Smith's retirement and Secord's transfer to Cambridge in 1992, the college hired David Edgerton and Andrew Warwick as replacements. From 1987 Imperial had worked with the UCL department of HPS (later Science and Technology Studies) and the London Wellcome Unit to run an MSc programme under the aegis of the London Centre for the History of Medicine and Technology, and the Imperial group separated from the humanities department to form a new Centre in 1994.

From the mid-1970s, the Cambridge HPS department was headed by Michael Hoskin, and bolstered by the Wellcome Unit (see below), became the dominant group in the field. Hoskin, who worked on William Herschel and pioneered the study of Iberian archaeo-astronomy, began a major review journal in 1962, *History of Science*, which published surveys of the field and historiographical essays, and eight years later he founded the *Journal for the History of Astronomy*. Nick Jardine joined the department in the mid-70s and has worked on a number of areas including Kepler's philosophy of science, German *naturphilosophie*, and the historiography of science. In 1978 Jim Bennett became curator of the Whipple Museum, a position he held until he moved to become curator of the Oxford Museum for History of Science in 1994, and composed a number of important works on the history of the instrument-making trade. In 1984 Simon Schaffer and John Forrester joined the department. Schaffer published a number of sociologically-informed and articles on the history of science from the seventeenth to the nineteenth centuries, and produced a host of students who went on to take up major positions in the field. Forrester composed a number of works on the history of psychoanalysis, while in 1992 Jim Secord joined the department from Imperial College, offering expertise in nineteenth century life and earth sciences.

Following the expansion of the university system, and given the unique bridging role that HPS could play, a number of other HPS units were created in the early 1970s. At the Open University a Department of History of Science and Technology was created in 1970 and headed by Colin Russell. The OU has always taught by distance learning, and staff in the department produced a number of innovative teaching tools including well-structured set books and television programmes. With funding from the Nuffield Foundation, the University of Kent created a Unit for the History, Philosophy and Social Relations of Science in 1974. This was headed by Maurice Crosland for two decades and he was joined soon after its inception by Alex Dolby and then by Crosbie Smith. Smith took over as director in 1994 when it was renamed the Centre for History and Cultural Studies of Science. The history of science and medicine at Lancaster has been strongly represented for a number of decades within the history department by Peter Harman, John Brooke, Robert Fox, Roger Smith and more recently by Stephen Pumfrey. Their interests have covered the work of James Clerk Maxwell (Harman), the relations between science and religion (Brooke), the history of thermodynamics and the history of scientific institutions (Fox), the history of the notion of inhibition (Smith), and the history of the 'magnetical philosophy' in the sixteenth and seventeenth centuries (Pumfrey).

Occasionally, the discipline has fallen victim to fashion or economic cutbacks. HPS was taught at Queen's University Belfast from the early 1960s and boastd major figures in the field such as Alan Gabbey and Peter Bowler until its demise in the early 90s. Elsewhere, the discipline has been represented by single scholars in various institutions. As a founding fellow of the Science Policy Research Unit in 1966 Roy MacLeod promoted history of science at the University of Sussex for over a decade before moving to the Institute of Education at London. Frank James began his edition of the correspondence of Michael Faraday from his position as historian of science at The Royal Institution, while at Durham David Knight taught and wrote on Romantic chemistry and the relations between science and religion. At Birkbeck College Michael Hunter played a major part in the 1980s and 90s in reviving interest in the life and writings of Robert Boyle, while John Henry has upheld the significance of the Scientific Revolution at the University of Edinburgh. The history of science has also figured strongly in courses in the history of philosophy, such as those given by John Rogers at Keele, and by Sarah Hutton at the University of Hertfordshire.

7.3 History of Science and Medicine

A major boost to the study of the discipline was given by the decision of the Wellcome Trust in the early 1970s to fund a number of centres for the history of medicine. A dominant group at this time was the Cambridge Wellcome Unit led by the Texan Marxist Bob Young. Young, whose work is considered in more detail below, pioneered the new social history of science in the 1960s but left Cambridge in 1976 to start a new career in psychoanalysis. In addition to this, he founded the journals *Radical Science Journal* and *Science as Culture*, and the publishing house Free Association Books, and he also produced a number of TV programmes that emphasised the ways in which science was imbued with a number of social values. A number of historians of science and medicine were associated with Young in the 1970s, including Roy Porter and Ludmilla Jordanova. Roger French took over from Young as director of the Wellcome Unit in 1974 and was joined by Adrian Wilson and Andrew Cunningham. While Wilson produced a book on eighteenth-century man-midwifery, Cunningham and French bridged history of science and history of medicine with singly and jointly authored monographs on Harvey's natural philosophy, medieval natural philosophy, and the Renaissance 'recovery' of Ancient anatomy (Pickstone, 1999: 462-4; French, 1994; French and Cunningham, 1996 and Cunningham, 1997).

The largest and most influential Wellcome Unit, committed like most of the others, to bridging ideas and approaches in history of medicine and history of science, was the London Wellcome Institute for History of Medicine (later the Wellcome Trust Centre for History of Medicine at UCL). This was headed by Bill Bynum, and became a major presence in the field, housing scholars with a wide range of interests bridging history of medicine, history of science and history of psychiatry. Chris Lawrence published works on the history of surgery, and the role of liberal humanism in interwar Britain, while Michael Neve worked on the history of science and medicine in eighteenth century Bath and Bristol, and on the history of psychiatry. Janet Browne combined roles in the Darwin Correspondence Project in Cambridge with a position in the Institute, and completed her major 2-volume biography of Darwin in 2002. Expertise in ancient and non-European medicine was represented by the work of Vivian Nutton and Lawrence Conrad respectively, while Andrew Wear wrote on early medical practice and Stephen Jacyna composed a major monograph on the history of the understanding of the nervous system (Pickstone, 1999: 466-7).

Bynum himself concentrated on nineteenth century medicine and the history of malaria, and edited the journal *Medical History* as well as a series of collections with Roy Porter. Porter moved from Cambridge to the Wellcome Institute in 1979, and thereafter published widely in social history, the history of medicine, the history of science and in particular, the history of psychiatry. Porter embodied the close connections between history of medicine and history of science, and amidst numerous other editing and writing activities, edited *History of Science* from 1972 to 2001. Porter also pioneered the study of the history of psychiatry, and helped found the major journal in the field. From very different perspectives, other historians have dealt with the history of the brain, including Graham Richards's history of psychology in Britain, Roger Smith's history of the notion of inhibition, Stephen Jacyna's history of nineteenth century neuroscientific concepts, and Roger Cooter's history of nineteenth century phrenology.

When a Chair in the History of Science was created at Oxford in 1973, it was awarded to Margaret Gowing, the leading authority on Britain's military and civilian nuclear programmes. Gowing was the archivist of the UK Atomic Energy Authority and was drawn to areas of archival preservation and contemporary history rather than to the history of science per se. However, history of science at Oxford benefited greatly from the arrival in 1972 of Charles Webster from Leeds, as Reader in History of Medicine and Director of the Wellcome Unit for History of Medicine. After *The Great Instauration* appeared in 1975, Webster went on to compose the official history of the NHS and under his leadership, the Oxford Wellcome Unit nurtured a number of historians of medicine, including Margaret Pelling and Paul Weindling who worked on early modern medicine and late nineteenth century German biology respectively.

In 1986, the UMIST history of science department moved to the Victoria University of Manchester, where a new Centre for History of Science, Technology and Medicine unit was formed. The group gained the status of a Wellcome Unit for History of Medicine and by the end of the 80s its emphasis was firmly on the history of medicine and the bio-medical sciences. Central to its reputation were John Pickstone's writings on the development of medical care in Manchester, Jonathan Harwood's publications on the history of genetics, and the work on nineteenth century neuroscientific concepts and phrenology of Stephen Jacyna and Roger Cooter (both of whom subsequently moved to the Wellcome Trust Centre in London) (Pickstone, 1999: 468-9).

8. The Sociology of Science and the Social History of Science

8.1 The Sociology of Scientific Knowledge

One of the most influential developments in the history of science arose from the Edinburgh University Science Studies Unit, founded by C.H. Waddington in 1966 and led from its beginning by David Edge. In the following decade Edge brought together a number of scholars from various academic fields, including David Bloor, Donald Mackenzie, Barry Barnes, Michael Mulkay and Steven Shapin, who collectively came to be known as the Edinburgh School. Having produced a PhD at Cambridge on radio astronomy, Edge worked for the BBC in the area of science communication and enjoyed a unique role operating between the media and academic history and philosophy of science (Bloor, 2003). One of the lasting contributions to the field was the journal *Science Studies* (*Social Studies of Science* from 1974), which Edge co-founded with Roy MacLeod in 1970.

The Edinburgh School assailed many of the most cherished assumptions of practising historians of science. As articulated by David Bloor, the Edinburgh School held that, contrary to the approaches of sociologists such as Joseph Ben-David and Robert Merton, sociological analysis could reach to the *content* of scientific knowledge as well as to its social and intellectual surroundings. The idea that truth could only come from a disinterested and unprejudiced scientist was a myth perpetrated by the vast majority of people who had written on the history and philosophy of science. Scientists were very much of the world, and were people whose beliefs, even if they were held to be true, could be explained by sociological theories. Bloor's *Knowledge and Social Imagery* codified these doctrines in the form of a 'Strong Programme' according to which a scientist's cognitive beliefs should be seen as being *caused* by non-cognitive conditions or interests. This did not imply that knowledge was *merely* social, or that one could concoct serious scientific knowledge in the pub, but it did mean that "the social component is always present and always constitutive of knowledge". Analysts were to use the *same sort of explanation* to account for successful and unsuccessful theories, that is, sociological factors should be seen as operating in both. Finally, the sorts of tools used by the sociologist had to be applicable to the sociological enquiry itself (the so-called *reflexivity* demand) (Bloor, 1991: 3,6 and 166).

Central to much work in the sociology of scientific knowledge of this period was the role of the laboratory, and the study of the routine and skilful use of laboratory instruments. In a range of articles leading up to his *Changing Order* of 1985, Harry Collins showed how scientists acquired know-how of how scientific equipment worked in situ; this skill could be passed on through personal contact with others but could not be written down or replicated elsewhere by, say, reading a journal article. This was a seminally important approach for understanding the routine 'life' of science, but also for understanding why much scientific work carried out on the cutting edge was not and could not be easily replicated elsewhere. In the case of experimental work, rival groups could always point to problems in the assumptions behind, or the design of any particular experiment. This situation would leave competing theories about the world in some sort of limbo (an 'experimenter's regress'), and Collins argued that what we now took to be the correct account of the natural world could not be used to explain why it was that one group rather than another won the day. Rather, it was the ability to enrol powerful allies that allowed one group to win out.

Much of the output from the Edinburgh School was more straightforwardly historical. Mulkay published a history of British radio astronomy in 1976, while Donald Mackenzie wrote a major work on early twentieth century statistics in Britain. The historical work that was most informed by the Edinburgh form of sociology of knowledge was Shapin and Simon Schaffer's *Leviathan and the Air-Pump* of 1985. They examined a key debate between the materialist Thomas Hobbes, author of *Leviathan* (1651) and the natural philosopher Robert Boyle, author of *New Experiments Physico-Mechanical* (1660), and the emblematic natural philosopher of the early Royal Society. According to Shapin and Schaffer, Hobbes and Boyle lived in different 'forms of life', a term drawn from Wittgenstein that captured the very different beliefs that each adopted towards politics and the natural world. The authors use Hobbes's critique of Boyle's natural philosophy to show how precarious were many of Boyle's claims to be building solid knowledge on firm experimental foundations. The fact that Hobbes's approach to science was plausible illustrates their constructivist view that "our forms of knowing [are] conventional and artifactual … it is ourselves and not reality that is responsible for what we know." (Shapin and Schaffer, 1985: 328)

Having lived through the civil wars and anarchism of the 1640s, Hobbes believed that certainty in politics, religion and philosophy could be achieved only by the authority of a central figure who defined the basic elements of each field. In natural philosophy too, individuals should reason according to the mathematical style developed by Euclid and thence they could come to absolutely certain truths, including those concerning the basic building blocks of Nature (in Hobbes's case, matter and motion). On the other hand, Boyle was an empiricist aristocrat who believed that steady observation and experiment, not mathematics, could give fruitful and true information about the natural world, leading ultimately to a 'moral certainty' about matters of natural fact. Where Hobbes emphasised the importance of right thinking as a means to the acquisition of truths, Boyle stressed the significance of properly designed experiments that made use of chemical or philosophical machines – especially the air-pump, which Boyle had had made in the 1650s. The pump, which is central to Boyle's project and indeed to *Leviathan and the Air-Pump*, was rejected as a mere toy by Hobbes.

Hobbes's style of philosophizing was appropriate for an individual who believed in a centralised and absolutist form of government. Boyle's gradualist approach, which employed literary and artistic techniques that Shapin and Schaffer called 'virtual witnessing' in order to convince readers that what Boyle claimed was indeed a matter of fact, was more relevant to a community of experimental practitioners. This group was suspicious of the claims to absolute certainty claimed for their conclusions by philosophers such as Hobbes and Descartes – for how could science progress – or be possible -- when large numbers of such people went around proclaiming that they had a unique access to truth? Boyle's style was appropriate for a community of genteel practitioners, in which natural truths depended on publicly witnessed evidence and on no one person's word.

According to Shapin and Schaffer Boyle won, because he had more powerful allies and a raft of socially endorsed practices surrounding the value of knowledge produced via the air-pump, and his experimental philosophy became dominant in the Royal Society, On the other hand Hobbes was ostracised from the same Society, his mathematical prowess dismissed by experts and his approach to natural philosophy ruled out as arrogant. But paradoxically Boyle won because Hobbes was right, at least in his cynical view about what made comments true in a given community. In spite of Boyle's own belief that nature itself would decide what theory was true or false, Hobbes's claim that might makes right held true in the Royal Society. *Leviathan and the Air-Pump* was held to be a landmark book in the history of science, yet its approach has been imitated by few. The book's sociological rhetoric, particularly its conventionalist/relativistic slant, was unpalatable to some commentators, who argued that it was the superior quality of Boyle's work that made his work preferably to Hobbes's. Moreover, the notion (drawn from the Edinburgh School) that social interests could be used to *explain* intellectual commitments was losing favour by the time the work appeared. Critics argued that invoking some external 'society' to account for intellectual beliefs merely substituted one artificial construct for another. Finally, the book contains an excellent treatment of the troubles with replicating and interpreting Boyle's air-pump experiments. Nevertheless, as we shall see, instead of 'interests', post-*Leviathan* history of science would emphasise much more strongly the central role played by the skilful use and dissemination of machines in deciding the outcome of disputes about experiment.

8.2 The Social History of Science

By the early 1970s the Koyréan conceptual revolution was practically defunct. Many of the great editing projects associated with Newton, or the early modern period more generally, were coming to an end, and students and younger scholars increasingly looked to the eighteenth and nineteenth centuries for new pastures. Reacting against the narrow concern with the minutiae of theories in the exact sciences, and indeed the emphasis on concepts, a new generation of scholars developed what they called a social history of science. This approach moved away from the heroic focus of previous work and looked at the role scientific knowledge played in the wider society. These included institutional histories of science in Manchester, the medical marketplace, and the early history of the British Association for the Advancement of Science written by Arnold Thackray, Norman Jewson, and Thackray and Jack Morrell.

Central to the new social history of science were essays composed by Bob Young between 1968 and 1973 (collected in Young, 1985). Young showed that Darwin was part of a long tradition of debates about man's place in nature, and the possibility of evolution. From the late eighteenth century onwards, this was made up of a fabric of religious, political and economic discourses in which authors appealed to 'nature' as a guarantee of the truth of their theories. Darwin's theory of evolution by natural selection, for example, was informed by a wide range of different texts, ideas and observations including Malthus's analysis of the pressures placed by limited food resources on a potentially exponentially expanding population, a racial theory in which white Europeans had risen to the top of their class because of various features, and contemporary economic ideas in which competition through free trade was extolled as the most natural means of expanding personal and national wealth. In turn, Darwin's theory was coopted by commentators such as Darwin's cousin, Francis Galton and Herbert Spencer. Spencer, the man who devised the term 'survival of the fittest', showed how selection pressures akin to those exercised by Darwinian evolution apparently worked in many social spheres. At a time when there was an increasing division between those in the profession who dealt with 'external' and those with 'internal' elements in the history of science, Young argued that this was a barren dichotomy. Instead, historians should adopt an ecumenical position in which the history of science should be seen to have been determined by many different sorts of factors, both conceptual and social.

Although to some extent the social history of science evolved independently from the sociology of science, various works, particularly those of Shapin and Mackenzie, were influential in thinking about how social commitments (or interests) were related to scientific beliefs. At the same time, the field embraced perspectives offered by new analyses of the role of disciplines in forming the character of scientific knowledge, and a new respect for the central part played by patronage. These novel approaches, most notably expressed in the writings of Robert Westman and Mario Biagioli, showed that the disciplinary structures that underpinned the structures of natural philosophy were closely related to contemporary social hierarchies. Historians also turned their attention to the role of lectures and other public performances in the history of science. Central here was the work of Simon Schaffer, Jan Golinski and Larry Stewart, the last two of whom placed public lecturing in chemistry and Newtonian physics at the heart of their book-length studies of eighteenth century science.

Historians now had an unprecedentedly rich vein of different perspectives from which to address the past. Another source of inspiration for historians of science and especially historians of medicine, was the work of Michel Foucault. Foucault wrote major works in the 1960s on the relations between madness and civilisation, the origins of the clinic in early nineteenth century Paris, and the unconscious rules that linked work in early modern study of plants and animals, grammar and economics. While denying that he was a 'structuralist', Foucault's work nevertheless embodied anti-subjectivist (concentrating on works and disciplines, not individuals and their intentions) and anti-grand narrative (eschewing the 'explanation' of historical events by recourse to super-theories like Marxism) currents in the writings of Claude Levi-Strauss, Louis Althusser and Roland Barthes.

Foucault's 1973 book *Surveillir et Punir: Naissance de la Prison* (translated in 1975 as *Discipline and Punish: The Birth of the Prison*) is by far the most significant work for historians of science. Although it ostensibly concerns the origins of new attitudes to imprisonment and reform in the wake of the French Revolution, in fact Foucault directed attention to all the social and intellectual practices of *training* that were introduced into various domains of human activity at the same time. Although the Enlightenment emphasised the need for freedom from religious and social shackles, and the liberty to think new thoughts, in fact the appearance of new sorts of knowledge in the early nineteenth century was predicated on new and highly sophisticated forms of discipline and pedagogy. In France, Britain and other European countries, people received more intensive forms of education and training in new arenas such as prisons, hospitals, factories and the *grandes écoles*. These new architectural and social arrangements were surveillance mechanisms that were designed to produce self-disciplined individuals – managing highly disciplined and skilful bodies -- that could be used by the state apparatus to further its ends. Underlying this extraordinary and fertile account of state power were two insights: firstly, that knowledge and power were intimately bound up with one another, and that power was not simply exercised centrally and punitively from above but was distributed productively throughout the state. Secondly, as a result of this, Europeans who trumpeted their freedom and superior rationality were actually the unwitting targets of the most subtle techniques of 'subjection' (sc. subject-making) that the world had ever seen.

The anti-Enlightenment views expressed in the writings of Foucault and others, especially in Adorno and Horkheimer's *Dialectics of Enlightenment*, struck a chord with many scholars in the period. In showing that there had once been very different attitudes to nature in Europe, akin to various belief systems outside the West, history of science offered some historians and commentators visions of a different sort of science. In an age of Mutually Assured Destruction, older criticisms of the short-term instrumentalism of science and technology were reinvigorated and some academic history of science took on a radical hue, most notably in Carolyn Merchant's *The Death of Nature*, published in 1980. Merchant combined an attack on the masculinist assumptions underlying some early modern statements about how to explore the natural world, with a lament for the disappearance of more environmentally sustainable views of nature. It is striking that alongside the increasing influence wielded by sociologically informed approaches, the critical tradition represented in the works of Young, Merchant, Lynn White and Jerry Ravetz, fellm out of favour in the early 1980s. Undoubtedly this can be explained to some extent by the advent of a more cynical and materialist Anglo-American politics at the end of the 1970s. Nevertheless, within the profession it had already manifested itself in the form of Pythonesque disputes between social historians of science and medicine, radical historians and sociologists of science.

8.3 Science in the Nineteenth Century

The new social history of science of the 1970s was marked by concern with different topics, periods and individuals from those that had captured the attention of previous generations. Galvanised by the work of Young and others, the main foci of interest surrounded the creation and reception of Darwin's theory of evolution by natural selection. Darwin's work had produced a revolution in scientific and social thought that was every bit as substantial as that effected by Newton, and historians of science examined both the details of Darwin's theory as well as the wider contexts from which his work emerged, and the effects it exerted on the wider society. In a way that mimicked the construction of the Newton industry two decades earlier, fascination with all things Darwin was marked by editing projects carried out at varying scales. Of these, the inauguration of the Darwin Correspondence Project by Frederick Burkhardt in 1974, primarily based amidst the Darwin papers at Cambridge University Library, was the most ambitious. Now online, the availability of Darwin's letters and (in print) his other papers, offers an unrivalled resource for scholars.

A new generation of scholars working within a Darwin 'Industry' investigated the detailed steps by which Darwin arrived at his basic theory and gradually refined it; the religious disputes that followed the *Origin of Species* and *The Descent of Man*; the social contexts surrounding the meaning and implications of Lamarckian and Darwinian notions of evolution; the connections between Darwin's and Alfred Russel Wallace's theories of evolution by natural selection (and the relations between the two extraordinary men); the reception of Darwin's theory by scholars in Europe and America; the use of evolution in the novels of George Eliot and others; and the many idiosyncracies of Darwin the Man. Verily Charles had supplanted Isaac in the pantheon (Moore, 1979; Beer, 1983; Shuttleworth, 1984; Kohn, 1985; Desmond, 1989; Desmond and Moore, 1991; Browne, 1995 and 2002; Secord, 2000)

Another growth area was the history of palaeontology and geology. Martin Rudwick published a major work on the former in 1972, while Roy Porter's *The Making of Geology* of 1977 attacked old-fashioned history of geology in which Scripturally-grounded histories of the earth had to be heroically swept aside by the founding father of the discipline, James Hutton, before a mature science of geology could come into being. This was followed two years later by an influential book on the history of the environmental sciences edited by Porter and Jordanova. In the 1980s Rudwick and Jim Secord composed rich studies of various episodes in the understanding of the geological record. Rudwick in particular addressed methodological debates concerning the extent to which the characters in his work were to be seen as responsible for 'making' the geological system they promoted, and the degree to which they were constrained by the real geological landscape (Rudwick, 1985).

The 1980s and 90s both American and British historians displayed a new concern with the exact sciences in the nineteenth century. Norton Wise and Crosbie Smith's massive 1989 biography of William Thomson (Lord Kelvin), *Energy and Empire,* was a prime example of how to integrate conceptual and social aspects to create a nuanced picture of one of Victorian Britain's greatest scientists. Simon Schaffer and Bruce Hunt examined the work of James Clerk Maxwell and his students, while David Gooding and Frank James produced a number of articles on the work of Michael Faraday. In *Frankenstein's Children* of 1998, Iwan Morus turned attention to the 'other' electricians who populated Faraday's London and who were closely bound up with the commercial and industrial sectors then promoting developments in telegraphy and the railway. Finally, in his *Masters of Theory* of 2002, Andrew Warwick examined the way that the regime of study in the Cambridge Mathematical Tripos was intensified throughout the nineteenth century. Drawing from the work of Kuhn and Foucault, Warwick looked at the culture of training that selected and produced individuals who were able to answer difficult questions in mathematical physics more quickly than their predecessors. A study in the way that one highly specialised group reproduced itself over the course of four or five generations, Warwick showed that the production of theoretical excellence was a practical and institutional achievement that required the disciplining of both the mind and the body.

9. Recent Developments

9.1 Material Culture and Global Science

Since the end of the Second World War, professional historians of science had generally treated Marxist or social-determinist models of science with disdain. From the mid-80s, the most spectacular effects of sociology on the practice of history of science concerned the importance of instrumentation, the importance of skill (rather than 'knowledge') and the pivotal role of the laboratory. One strand of this new concern with things, people and places concerned the equipment that was routinely taken into account by scientists in making observations, and designing and performing experiments. Without deploying increasingly expensive machines for acquiring and (more recently) processing information, most science and astronomy over the last three centuries would be impossible (see in particular Chapman, 1995).

Recognising the overwhelming importance of instrumentation now meant that historians of science could fully integrate the subject with both social and sociologically informed histories of science. Pioneering work in this area was done by Derek de Solla Price, followed by David Bryden, Gerard Turner, Anthony Turner and Jim Bennett. Many of these historians worked in museums, while others had close connections with the thriving trade in scientific instruments. Perhaps the most relevant consequence of this 'turn' in the history of science was the recognition of the pivotal role played by expert craftsmen in facilitating scientific work. In a strong sense, and without implying that historians in this area endorse the ideological baggage that came with it, the renewed respect paid to material culture – and its creators -- represents a stunning victory for the older Marxist view.

If the emphasis of the 1980s had been on local studies, then the following decade witnessed a new stress on the global infrastructures in which scientific knowledge was made, assessed, disseminated and appropriated. Highly influential here was the work of Bruno Latour, whose *Science in Action* of 1987 drew on the work of sociologists such as Collins and Trevor Pinch, as well as his own book on laboratory life (co-authored with Steve Woolgar). Latour argued that laboratories and the practices within them were astonishingly powerful settings for manipulating nature. Objects within them could be controlled and known by being analysed and classified, but local skills and knowledge had to function outside the laboratory before they could become scientific *facts*. Like machines, which required a massive infrastructure and skilled support personnel to make them work on a global scale, so science required universal standards in order that various theories could function everywhere. The activity by which standards like the ohm and the volt worked elsewhere in the cosmos and thus seemed to be fundamental constants of nature was termed *metrology*.

Unsurprisingly, given the sources on which his work was based, Latour's thesis worked particularly well for the exact sciences in the nineteenth and twentieth centuries. Imperial extension was a significant theme in the history of the Victorian exact sciences as well as in the life sciences. The works of Richard Grove, Richard Drayton and John Gascoigne all contributed to a better understanding of the role played by the circulation of seeds, flora and fauna in the consolidation of the Iberian and British empires between 1400 and 1900. Outposts in the British Empire, especially after 1783, engaged in a complex exchange by which indigenous exotic life forms were transplanted to Kew Gardens and other outposts, while hardy British crops were taken out to the colonies. Local knowledge of plants, animals and maps was made 'scientific' and reusable by appearing in printed books and journals. In two books on Joseph Banks, botanist on the first Cook expedition (1768-71) and President of the Royal Society between 1778 and 1820, John Gascoigne showed how Banks made himself the centre of a vast exchange of information and natural and artificial goods between the centres and peripheries of the British Empire. Banks was effectively an arm of the state who dispensed patronage to naturalists and other men who could increase the wealth of the British Empire and ensure agricultural and industrial self-sufficiency in the period of the American, French and Industrial Revolutions.

These histories have shown how both European and non-European objects – natural and artificial – circulated between various networks and were bound up in different value-systems. These goods were valued for their aesthetic worth, their scientific status, and their financial worth as rarities or commodities. A consequence of this has been the forging of a new set of links between history of science, environmental history, economic history, cultural history, imperial history, and military history. Indeed, currently the most fashionable topic in the field is that of voyages of discovery, with attention being paid to the increasing amounts of scientific instrumentation required by the various national undertakings that took place in the wake of Cook. This is of particular interest to historians working on the topic of the Rise of the West, who want to know why and when Western science developed and took on the characteristics that it did (Reill and Miller, 1991; Grove, 1995; Gascoigne, 1994 and 1998; Cook, 2007).

9.2 History of Science and its Wider Publics

For decades history of science has attracted popularisers in different media, notably in Butterfield's *Origins*, Jacob Bronowski's television series *The Ascent of Man* in the early 70s, and Stephen Jay Gould's numerous articles on science and history of science in *Natural History*, which were repackaged as best-selling books in the 1980s and 90s. By this time, the history of science was again pricking the interest of the general public, benefitting from the twin vogues for popular history and popular science. In 1988 publishers had capitalised on the extraordinary interest in the wheelchair-bound physicist Stephen Hawking, and released a best-seller (over 10 million copies sold to this point) on recent cosmology entitled *A Brief History of Time*. In 1991 Adrian Desmond and Jim Moore published *Darwin: the Life of a Tormented Evolutionist*, which built on existing scholarship while expertly weaving an account of the life of Darwin with the history of his development of the theory of evolution by natural selection. More recently, Janet Browne's contextual biography of Darwin demonstrates that the man and his work still hold an unrivalled fascination.

However, the 1995 book *Longitude*, written by the journalist Dava Sobel, caused a sensation in the field. This short book dramatised the efforts of John Harrison to have his chronometer accepted as the best way for determining longitude at sea, in the face of determined efforts (as Sobel would have it) by the Astronomer Royal Nevil Maskelyne to thwart his ambitions. More recently Bill Bryson's *Short History of Nearly Everything* of 2003 showed that even a broad history could capture the interest of large swathes of general readers.

Numerous sorts of historian populate the territory of history of science, including those concerned with the history of scientific instruments, intellectual history, voyages of discovery, science and literature, and the history of science in popular culture. Like general historians, historians of science are now faced with a much more complicated set of problems concerning the sort of audiences they ought to address. The scholarly rationale for publishing specialist monographs is now in serious question, and historians will probably never again be able to devote the bulk of their careers to the internal development of one aspect of a great scientist's oeuvre. As a result, there is increasing interest in producing high quality research and resources online, as exemplified in the Newton Project, the Online Darwin Project and the Darwin Correspondence Project. All of these undertakings are premised on the view that there is extra value to be obtained for both the academic and the general public in making scholarly resources freely available online.

9.3 The Present State of History of Science

Despite the changes outlined the last few sections, more traditional approaches have continued within the field. In the 1970s and early 80s, articles on the religious interests of various philosophers and scientists were common features in journals, though these had fallen out of fashion by the last decade of the twentieth century. Nevertheless, historians have continued to produce important examples of intellectualist history of science, notably in the complex historical relations between science and religion. One signal contribution of recent intellectual history of science has been to show that relations between philosophical, social and religious values were just as significant if much more complicated than was previously believed. In *Science and Religion* of 1992, John Brooke showed that at different moments in the past, religious values at both the individual and institutional levels were supportive of, irrelevant to or antagonistic towards various positions within natural philosophy. Similarly, both lay and elite views of nature play key roles in Stuart Clark's comprehensive *Thinking with Demons* of 1997, which shows how difficult – if necessary -- it was for demonologists to categorise various events as natural, marvellous or supernatural. In different ways, both these works suggest that it is impossible to articulate any overarching grand narrative that can account for the complex historical relations that have existed between religious and scientific values.

Just as science takes many forms, so the history of science constantly reattaches itself to other disciplines in the humanities and social sciences, and embraces a wide range of approaches. These link history of science to history of technology, history of medicine and many other historical disciplines including the history of mathematics (which I have largely ignored in this paper). If the Darwinian metaphor is used for a moment, then certain styles of enquiry and topics seem to have become less fit, and have died out. I refer here to the close relations between history and philosophy of science that largely defined the discipline between the 1940s and the 1960s, and to the related concern with conceptual analysis that was held to be definitive of the enterprise in the same period. As the historians who were active in the post-war period retired, so interest in the early modern period has waned. In Britain, there has been a corresponding expansion in attention paid to the social history of nineteenth century science. Historians also take seriously the point that before the early nineteenth century, there was no such thing as science, but instead there was something called 'natural philosophy', with much broader, ultimately religious aims.

Another factor has been the disappearance of specialist, technical histories of science. Only a handful of historians are now equipped to deal with the technical complexities of the sciences, especially regarding the twentieth century physical and biological sciences which (with the exception of work by Jon Agar and Jeff Hughes) remain largely unresearched areas. Elsewhere, a lamentable gap exists in the borderland between the history of science and the history of technology, in the main because there is little established history of technology in British universities. For many reasons this is peculiar. Britain has long been considered as the major instigator in the Industrial Revolution, and noone can seriously doubt the primacy within our culture of the products of engineering and technical skills. One wonders what would be the case now if universities had been inundated with lectureships in the history of technology rather than in the history of medicine, as they were in the 1990s. Departments that might have been HPS units three or four decades ago now call themselves Departments of Science Studies, or Departments of Science, Technology and Society. This reveals new institutional links with science communication and science policy, but they often contain little history of science, and usually no history of technology.

More than any other historical discipline, throughout its own history the history of science has been forced – or has chosen – to make alliances with numerous other intellectual groupings. It has been prone to fads and fashions, and to amnesia about -- and the rediscovery of -- old topics and theses. In recent years, the maturity of history of science as a properly historical discipline has opened it to charges that it has been too concerned with temporally and spatially local episodes. Some critics have charged that in immersing themselves in footnotes, and by offering rich and thick descriptions of scientific and related activities, historians of science run the risk of losing the audiences it has building up in academia and in the wider population. Some of these critics argue that historians collectively have a sort of moral obligation to produce new grand stories for these audiences, while others believe that historians of science should not lose sight of the old and central messages relating to the universality and rationality of science. The latter group has charged that history of science has recently been spending too much time with the sociology of science, and as a result has imbibed many of the dangerous relativist notions that pervade the latter field. Indeed, for these reasons, history of science was heavily implicated in the tedious and sterile Science Wars that blighted US academia in the 1990s. If these critics misunderstood and systematically misrepresented many of the many positions adopted by historians of science, their concern pointed to the significant role that history of science should still play in the academy and in the wider culture.

Online Resources

The Darwin Correspondence Project

The Newton Project

The Online Darwin Project

Journals

Isis
British Journal for History of Science
Annals of Science
Perspectives on Science and Medicine
Archives for the History of the Exact Sciences
Ambix
Notes and Records of the Royal Society
Studies in the History and Philosophy of Science
Journal for the History of Biology

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