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The Development of Science and Technology in Nineteenth-Century Britain

The Importance of Manchester

Donald Cardwell

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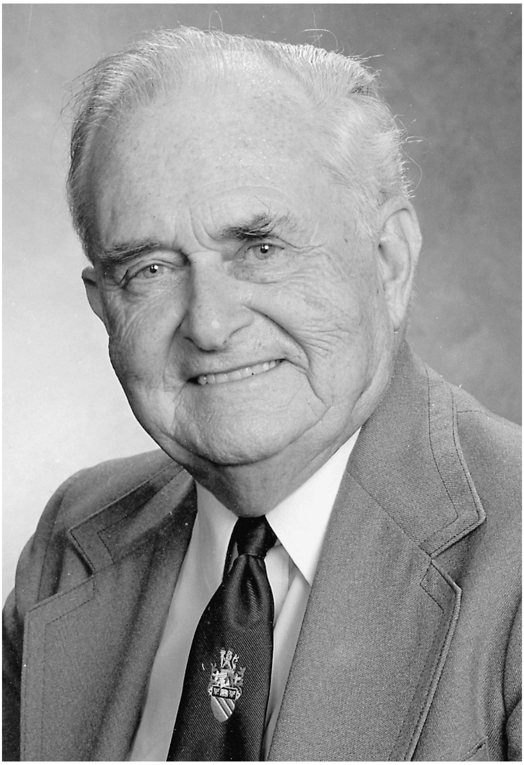

Professor Donald Cardwell

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The Importance of Manchester

Edited by Richard L. Hills

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PUBLISHER'S NOTE

The articles in this volume, as in all others in the Variorum Collected Studies Series, have not been given a new, continuous pagination. In order to avoid confusion, and to facilitate their use where these same studies have been referred to elsewhere, the original pagination has been maintained wherever possible.

Each article has been given a Roman number in order of appearance, as listed in the Contents. This number is repeated on each page and is quoted in the index entries.

FOREWORD

The late Professor Donald Cardwell published five important books on the history of education and science and technology. A full list of these and his published papers is given in the Select Bibliography. This selection of his papers has been chosen to avoid repeating what has been printed in his books. It reflects the wide range of his interests and shows the development of his ideas, both before and after he had completed his research for his books. Therefore these articles make a further contribution to our knowledge and understanding of these disciplines and the subjects contained in them. They also show how Donald changed from starting with scientific and technical education especially in the universities before broadening this after he had moved to Manchester by exploring how this background knowledge was disseminated between people generally. Here he recognized the crucial role of societies like the Manchester Literary and Philosophical Society which became another line of research. This developed into his study of J.P. Joule and his passion for the City of Manchester. But underlying all this was Donald's grounding in physics which resulted in his publications on the development of the electrical industries and, more centrally to his work, the rise of thermodynamics.

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DONALD STEPHEN LOWELL CARDWELL
(4 August 1919 – 8 May 1998)

'He is like a man which digged deep, and laid the foundation on a rock'[1]

It is unusual to start any obituary with a passage from the Bible, but the one quoted above was chosen to be read at Donald Cardwell's funeral and is particularly apposite in both its literal and metaphorical meanings. Donald started life by being born on the Rock of Gibraltar. His family returned first to Greenock (1922–28) and then to Plymouth where Donald attended Plymouth College (1929–36). They then moved to live at Croydon in the family home, designed and possibly built by his grandfather. It was probably at Croydon that Donald's life-long interest in airplanes was first roused. It was through his father that Donald was introduced to the Swiss mountains, so starting a love for rocky landscapes. Walking through those mountains changed later in his life into taking family holidays to ski down them. He was introduced to this sport by Gordon Hesling (1920–89), a life-long friend from Plymouth College days. Donald had hoped to go to Wengen again in the last year of his life That was not to be, but he was delighted with a snapshot of his small grandson on skis, showing that family traditions would be maintained. Donald's family was very important to him and was another bed-rock in his life.

After a skiing holiday, Donald would return to UMIST reinvigorated; his fund of stories would have been renewed – at the lunch table in Staff House, he would have us rocking with laughter at fresh tales of Yorkshiremen, or whatever, for he was a born raconteur, able to correctly mimic accents and other people's expressions. This lighter side of Donald's character often helped to ease an otherwise fraught situation and was the reason why he was for many years the Public Orator at UMIST Honorary Fellowship Ceremonies.

The real foundation of Donald's career and his academic work was his First Class degree in Physics at King's College, London (1936–40 and 1946–49). He would have been influenced by people like Bill Seeds, John Randall and Maurice Wilking who were developing radar at the time. His studies at King's were broken by World War II when he was called up in the Admiralty Signal and Radar Establishment and instructed naval personnel in radar at Scapa Flow and also went to the Middle East and West Africa. Many years later, Donald was ecstatic at finding a 'cavity magnetron', the vital valve which made radar possible and which helped us to win the war, among a box of old radio valves presented to the Museum in Manchester.

Donald once described himself as 'a practising physicist – a "pure" scientist – and as an electronics engineer'.[2] His practical work with radar must have been a stepping stone towards gaining his Ph.D. at King's on 'The Detection of Electromagnetic Waves Generated by Lightning Strikes' (1949). Following this, we see another foundation being laid through a Nuffield Research Grant at King's which stimulated Donald's interest in social aspects as well as the historical background of the teaching of science and technology. While he finished this research in 1954, it was not published until 1957 as *The Organisation of Science in England: A Retrospect.* It proved to be a seminal work, reprinted many times. Donald's knowedge of scientific education spread both to institutions on the continent and to societies in Britain such as the Manchester Literary and Philosophical Society. This background was invaluable when UMIST celebrated its 150th anniversary in 1974 and Donald edited *From Artisan to Graduate.* A glance through the list of contributors shows how broad were his contacts.

While carrying out the research for the Nuffield grant, Donald met Olive Pumphrey whom he married in 1953. My suspicions are that many of Donald's publications would not have seen the light of day without her constant care and loving support. While *The Organisation of Science in England* was in the press, Donald spent a couple of years at Keele University (1955–56) with the economist Bruce Williams. Then, after its publication, came the invitation from Stephen Toulmin to join the Division of the History and Philosophy of Science in the Philosophy Department at Leeds University. Here again another foundation stone for future development was laid with the publication in 1963 of *Steam Power in the Eighteenth Century: A Case Study in the Application of Science.*

That year saw Donald crossing the Pennines to Manchester at the invitation of Lord Bowden, Principal of UMIST, to set up a Department of History of Science and Technology and, if possible, to start a Museum of Science and Technology. Donald remained at UMIST for the rest of his academic career, first as Reader (1963–73) and then Professor (1974–84) until his retirement. The department became broadly based with the addition of the two Drs Farrar. Wilfred (1920–77) was Senior Lecturer and had a profound knowledge of the history of chemistry as well as foreign languages. His wife, Kathleen, also a chemist, contributed a more social background, particularly through studies on the development of medical services in the region. One result of their collaboration was the organization of the conference to celebrate the bi-centenary of John Dalton's birth. Donald edited the papers as *John Dalton and the Progress of Science*, with contributions from many notable scholars.

Dr A.J. Pacey transferred from UMIST Physics Department as lecturer and subsequently published *The Maze of Ingenuity* and other books.[3] With these other members of the department, Donald launched undergraduate courses in the History of Science and Technology for many other departments in UMIST. Post-graduate students ranged over a wide variety of subjects, from Arabic technology to textiles, transport, internal combustion engines as well as history of science. Donald's main work at this period was in the history of thermodynamics, for which, of course, his physics degree provided a firm foundation. His interest in this subject attracted many research students to his department. But it was Donald's own scholarship which formed the basis of another seminal book, *From Watt to Clausius: The Rise of Thermodynamics in the Early Industrial Age* published in 1971. It has proved to be the foundation for the research of many other people and justly merited the award of the Dexter Prize by the American Society for the History of Technology in 1973.

The experiments of James Joule were of course discussed in *From Watt to Clausius*, but Donald continued to study the life of this important scientist and in 1989 published *James Joule: A Biography*. This is the fullest biography available and most likely will remain so through the comprehensive nature of the research and the writing. In addition, Donald drew together the experience of many years teaching in two general books. The first in 1972 was *Technology, Science and History*, which was published in the United States of America as *Turning Points in Western Civilization*. He rewrote it as *The Fontana History of Technology* which was published in 1994.

We will never know the full extent of the foundations which Donald laid in the lives of very many people through his teaching and lectures. There were the undergraduate and post-graduate courses in Leeds and UMIST. As well as these, Donald gave talks to many other groups, societies and just ordinary people. Those of us who attended his lectures owe him a great debt through his help in launching our careers. He also supported the Open University by making educational films while some of his books, such as *The Organisation of Science in England*, became course books, so that his influence was very wide-spread indeed.

Manchester became for Donald another rock or foundation on which to build. With that background of physics and history, Donald realised full well the vital contributions which people in the area had made to the advancement of science and technology, and hence to the industrial civilization which we have today. He would use the phrase, 'Manchester – Centre of Industrial Revolution', emphasizing the effect that Manchester had on other places. He saw that the Manchester Literary and Philosophical Society had been central to the scientific community in Manchester and carried on that tradition through being its Secretary, Editor, Vice-President and President. Finally he was elected an Honorary Member. Donald's interests in Dalton, Joule, steam power and much more were all stimulated through seeing this vital role of Manchester in scientific endeavour. The attractions Manchester held were so great that, although he himself had largely rebuilt a cottage in Pembroke for his retirement, he found he missed the city and UMIST so much that he returned to live in Knutsford where he was to die.

His realization of the important contributions made in Manchester to science and technology and the importance of these contributions to world history, led to his carrying out the other instruction of Lord Bowden, to see if a science museum could be started. It was Donald's vision of the role which Manchester had played, his vision of a Museum which would not only be a place for display of exhibits, but which would both use those exhibits for education and research, and which would demonstrate those exhibits wherever possible, which caused me to turn down the offer of a post at the established Museum of Science and Industry in Birmingham and accept the post of Research Assistant in UMIST with Donald, when there was only the gleam of a museum in his eye. Donald was one of the main instigators of the 1966 Report on the potential of a science museum in Manchester, which became its foundation document, another rock on which to build. Also, Donald proved to be a firm rock against whom I could lean for support in the many vicissitudes of launching that museum, whether trying to get a recalcitrant Newcomen engine to run properly or trying to squeeze a little more money out of reluctant authorities for an important exhibit. Indeed, he was one of the main instigators of the large model Newcomen engine together with Professor Johnson, UMIST Mechanical Engineering Department, which proved to be such an attraction when the Museum was in Grosvenor Street.

Donald was on the governing body from the Museum's first launch in 1968 until retiring about a couple of years ago from the position as Vice-President by which time the Museum had become one of the National Museums. Donald also saw the importance of good museums in the provinces. At my suggestion, he wrote to Jeannie Lee when she was Minister for the Arts which led to setting up a separate national fund for the Preservation of Scientific and Technical Material on which provincial museums could draw. He campaigned against the Capital attracting major exhibits such as H.M.S.

Belfast when these might have been dispersed to help draw visitors to other places.

Donald's recognition spread much further afield than Manchester. With that firm foundation of his physics background, Donald developed his academic career into a sort of outstanding rock, towering above all others. The Newcomen Society awarded him their Dickinson Memorial Medal in 1978 with his lecture on 'Science and the Steam Engine in the Early Nineteenth Century Reconsidered'.[4] This Medal must rank as the highest award for work on the history of technology in this country. That towering rock was recognized internationally also, by invitations to speak to many conferences abroad. But his work was especially recognized by his friend, Professor Ed Layton of Minneapolis, who suggested to the American Society for the History of Technology that Donald should receive first the Dexter prize in 1973 which has been mentioned already and then the award of their highest honour, the Leonardo da Vinci Medal in 1981. Since 1962, only four people have received this double honour of their two highest awards; of these four, there has been only one person from Britain – Donald.

In 1878, a petition to the Prime Minister was drawn up for a civil list pension to be given to James Joule. With one alteration, the citation can be applied to Donald as well.

> Whether we regard the magnitude of the problems on which he has been engaged, or the success and completeness with which he has solved them, he is almost without a rival. Not only in this country, but in the [world of the history of science and technology] generally, he occupies, by common consent, a position accorded only to the greatest names.[5]

Donald wrote in his introduction to his biography of Joule,

> There comes a point when every writer has to decide that he must close the account although he is well aware that there is more to be done.[6]

God has closed Donald's account. However Donald was able to fulfill one wish, which he described in his recreational interests in his entry to *Who's Who in Greater Manchester*[7] as 'Keeping on working' which he did until virtually the end of his life. He was an active member of the North Western Branch of the Newcomen Society, being its Chairman. He was assisting with writing the history of UMIST and was trying to update Axon's *Annals of Manchester*, a record of annual events in the city. We send our condolences to Olive and his daughter Diana and son Stephen. For them and for his many other friends, it seems appropriate to conclude with words which

James Watt wrote on the death of his first wife in 1773, although the feminine has been changed into the masculine.

> In him I lost the comfort of my life, a dear friend, and a faithful husband ... I grieve for myself, but not for my friend Donald, for if probity, charity, and duty to his family can entitle him to a better state, he enjoys it.

RICHARD L. HILLS

Hyde, Cheshire
November 2002

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1 The Gospel according to St Luke, chapter 6, verse 48.

2 D.S.L. Cardwell, *The Organisation of Science in England* (London: Heinemann, 1957), p. xi.

3 A.J. Pacey, *The Maze of Ingenuity; Ideas and Idealism in the Development of Technology* (London: Allen Lane, 1974).

4 *Transactions of the Newcomen Society*, Vol 49 (1977–78), pp. 111–120.

5 D.S.L. Cardwell, *James Joule: A Biography* (Manchester University Press, 1989), p. 283.

6 Ibid., p. ix.

7 *Who's Who in Greater Manchester*, Manchester Literary and Philosophical Society Publications Ltd., 4th edn., 1996, p. 41.

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I

The Development of Scientific Research in Modern Universities: a Comparative Study of Motives and Opportunities

I
Introduction

Francis Bacon, the quatercentenary of whose birth fell this year, was the first sociologist of science; not only by virtue of his descriptions of the social and psychological hindrances to the pursuit of science: the Idols of the Theatre etc., but also by virtue of his clear analysis of the modes of innovation. The mariner's compass, firearms and the printing press are, he says, the inventions which established the supremacy of Europe. An acute observation, no doubt, but Bacon goes further and points out that whereas the introductions of the compass and of firearms depended on the prior discoveries of the properties of the lodestone and of gunpowder — on science, if you like — there was no such precondition for the invention of the printing press. There was, in his view, no reason in principle why the ancient Egyptians or the Greeks could not have invented it.

This distinction between invention which depends on prior science, or discovery, on the one hand, and 'straightforward' invention[1] on the other, is one which runs through the history of technology from ancient times to the present day: it may be commended as a useful framework for prospective historians of technology. In the Baconian scheme the relationship between science and inventions based on science is one good reason for encouraging the advancement of science. Certainly Bacon's ideas helped to inspire the foundation of the Royal Society; but, as I understand him, he went even further in hoping that science, both pure and applied, would become a broadly based national and social institution. This did not begin to be the case until the end of the nineteenth century.

[1] There appears to be no generally accepted expression for this mode of invention.

Bacon has often been criticized for his suggestion that quite ordinary people should be able to undertake scientific research. If we think of science as a succession of 'big names' — Galileo, Descartes, Newton — it is easy to believe that Bacon was wrong. But he was not wrong; he was clearly right: every large modern research laboratory is a confirmatory instance. In fact, the great discovery of the nineteenth century that ordinary talents can be effectively harnessed for the process of discovery is a vindication of Bacon's judgment in this matter.

It was not unfitting that the nineteenth century opened in England with the foundation of the, apparently quite Baconian, Royal Institution. The intention was that it should further scientific investigations and seek to apply them to useful ends,[1] should disseminate scientific and other useful knowledge, and should have a library and museum of models. But it soon became clear that these intentions were not to be fully realized. Davy and Faraday established a tradition of research, but no fertilizing school of science was set up,[2] the lectures were not given to industrial technicians but to the wealthy and fashionable, the museum was never started and the great problems of relating science to industry were not tackled. It would be difficult, therefore, to sustain any claim that the Royal Institution was the first of the modern teaching and research laboratories.

The Royal Institution reflected the temper and traditions of English science: those of the amateur or devotee. In much the same way the Royal Society was, at that time, an association both of men actively and of men passively interested in science. If there were relatively few really professional scientists in the Society, that was because there were few indeed in the country; and if some of the Fellows were influential non-scientists, that was not necessarily to be deplored.

Correlative with the amateurism of English science, the universities took it to be their duty to conserve and to transmit the established liberal education based on classical literature and Newtonian natural philosophy. Interpreted by a man like Whewell this philosophy of education could be persuasive, and

[1] Early supporters of technical education in England favoured the teaching of the 'sciences underlying the arts'. Apparently this meant teaching the 'public', or carefully edited science of learned journals, text-books, etc. This is science at its most general and with little or no reference to the particular instances which may be the main concern of technicians. This possibly impeded the development of scientific technical education.

[2] A proposed "Davy School of Practical Chemistry" did not materialize.

criticism was, in fact, usually restricted either to matters of standards and syllabus or to the exclusiveness of English university education. The intending scientist would be advised, on this philosophy, to postpone his researches until his liberal education was completed, when the clarity of his thought and the soundness of his judgment would be fully developed. Accordingly, university reform in England during the first half of the nineteenth century was devoted to securing the inclusion of modern sciences in the syllabuses and ending unfair discriminations. The roles of scholarship and research in the university were hardly considered at all.

It was different in Germany. From the beginning of the century and more particularly from the foundation of Berlin University, free research was regarded as the ideal means of higher education. Wilhelm von Humboldt, a Kantian humanist, established the doctrine that the autonomy of reason was fundamental to the nature of the university. The university professor is an original scholar whose students help him in his work, acquiring their education and love of learning in the process. Beneath these academic ideals there were strong social currents: in times of defeat and political fragmentation the German universities were the acknowledged strongholds of aspirations for political and social unity: symbols of a common national heritage. This gave them an honoured place in the centre of German public life. The geography and history of England demanded no such role of the English universities. There were enough separate German states to ensure the existence of a number of competing, non-centralized universities. The ideal of free research was from the beginning impartial and non-utilitarian: it applied equally to classical philology and chemistry, to history, law, philosophy and physics, indeed to all branches of systematic knowledge. Engineering and technology were excluded from the university.

The German system was set up in very conscious opposition to the French. In 1808 Napoleon had unified all higher education in France under one central university. This in the event was unfortunate: it established the scientific domination of Paris over the provincial cities, prevented the development of autonomous 'schools' of science and, thanks to the parsimony and indifference of the state, led to the neglect and under-endowment of research and scholarship.[1] Much later, after 1870,

[1] S. d'Irsay, *Histoire des universités* (Paris, 1935) II, 290 ff.

Pasteur attributed many of France's political ills to her persistent neglect of science.[1]

France, in fact, has as good a claim as any country to have pioneered the systematic application of science to industry. The advance of French mathematics in the eighteenth century had been accompanied by advances in application to mathematical engineering — civil and military — to hydraulics and shipbuilding. These were really the main applied sciences of the eighteenth century. During the Revolution heroic efforts were made to develop new industries and to put the older ones on a scientific basis. In 1794 the Ecole Polytechnique was established; here, among other things, training in practical chemistry was given and students were allowed to carry out their own experiments and investigations. This practice was, in fact, copied from the much admired mining college at Schemnitz (Štiavnica, in Czechoslovakia) founded in 1760 during the reign of Maria Theresa — the first of its kind.

Great though the achievements of French and British science were during the nineteenth century, neither country, and this is especially true of Britain, proved able to turn out research students — the rank and file of science — in the numbers and of the quality that Germany achieved. T. H. Huxley put it aptly when he likened British science to any army consisting solely of officers.

In the new century systematic experimental science and later systematic applied science were pioneered in Germany. Early in the century Strohmeyer at Göttingen and Gmelin at Heidelberg had teaching laboratories, but it was really the opening, in 1825, of Liebig's famous laboratory at Giessen that marked the start of organized scientific research as well as the emancipation of chemistry from medicine. Not only was there a constant stream of highly trained scientists from this laboratory, and from that of Wöhler at Göttingen, but new sciences were to be developed: organic chemistry, biochemistry, agricultural chemistry (interest in organic chemistry was much stimulated by its practical possibilities in agriculture and medicine). Following the successes of these laboratories, others were instituted at Marburg (1840), Leipzig (1843 and 1868), and elsewhere in the 1850's. In the 1860's extremely well equipped laboratories were opened at the universities of Berlin

[1] R. Vallery-Radot, *The Life of Pasteur* (London, 1906) 196.

and Bonn. The latter had accommodation for over sixty students.[1]

But the development of the German schools of chemistry may not have been so smoothly effected as a simple chronology of events might suggest. As late as 1840, Liebig himself made a sharp attack on the neglect of experimental chemistry by the six Prussian universities; they offered, he said, no place for the training of the teacher of experimental science, the way was blocked by "an overgrown humanism".[2]

The other sciences followed chemistry in setting up research and teaching laboratories from the late 1830's onwards. (In a sense, of course, physical and biological laboratories have always existed in the forms of observatories, botanical and zoological gardens, etc. And learned academies often carried out specified researches — Victor Regnault's investigations into the properties of steam are a classical example. But in the first case the institutions are very specialized and limited in number; in the second, as soon as the particular inquiry has been completed all research has stopped.) The development of biological laboratories has been closely tied to the medical faculty in which, at the beginning of the nineteenth century, the dominant scientific disciplines were anatomy and botany. But the influence of Liebig and other chemists together with the tremendous development of medicine — first in France and then in Germany[3] — led to the foundation of systematic laboratories for physiology and other sciences related to medicine. Prominent among these were those of Purkinje and Claude Bernard. The Pasteur Institute was opened in 1888.

It was not until the sixties and seventies that physics laboratories were established more or less simultaneously in most universities. It is not clear why physics should have lagged some forty years behind chemistry in this respect. Maxwell gives the rather negative reason: "it will take a good deal of effort to make Exp. physics bite into our university system which is so continuous and complete without it."[4] It might be

[1] For the chemical laboratories at Berlin and Bonn, see: *Report of the Department of Science and Art* (1866). For practical chemistry in the early days of the École Polytechnique, see: G. Pinet, *Histoire de l'Ecole Polytechnique* (Paris, 1887) 366.

[2] *Annalen der Chemie und Pharmacie*, XXXIV (1844) 97, 339.

[3] C. Newman, *Evolution of Medical Education in the Nineteenth Century* (London, 1957).

[4] Lord Rayleigh, *John William Strutt, Third Baron Rayleigh* (London, 1924).

that while the heart of physics — Newtonian mechanics — held little scope for experimental investigations, and while subjects like heat and electricity were regarded as branches of chemistry, the development of the physics laboratory would necessarily be retarded.[1]

II
A particular instance

It will be clear that the development of university laboratories represents a complex and far ranging subject. From this point, then, I must limit my discussion to one important topic only, and I must apologize for the serious omissions this will necessitate.

Only if the admission of students to a laboratory is recognized as a permanent practice can we talk of the laboratory as a systematic research institution. Judged by this standard, systematic research chemistry was taught for the first time in England when, in 1845, the Royal College of Chemistry was opened in London. Public recognition of the importance of Liebig's work for agriculture and medicine inspired the foundation of this institution, and the first principal, A. W. Hofmann, was in fact a nominee of Liebig's. For the first twenty-five years of its existence the College was small — numbers varied between thirty and fifty-two, indicating that the 'need' was not so great as its founders may have hoped — but in that time some 140 original papers were published.

The study of organic chemistry was now being pursued all over Europe. In France, a chair in the subject was created in Paris in 1853, and men like Chevreul, Dumas and others worked in collateral subjects at about that time or earlier; but the main centres of research were in Germany. It was therefore almost an accident when, in 1856, W. H. Perkin discovered the first of the aniline dyes. Perkin, a student of Hofmann's at the Royal College, had been working on his own in the Easter

[1] British chemists who made important contributions to the studies of heat and/or electricity include Black, Cavendish, Dalton and Davy. Through the work of men like Joule, Kelvin and Maxwell these subjects became part of natural philosophy. But public recognition of 'physics' did not come immediately, and a curious example of what we should regard as a misuse of the word is provided by Walter Bagehot's *Physics and Politics* (1873). Here by "physics" was meant natural selection. The word *physics* was imported either from France (cf. Biot, *Traité de physique*) or from Germany.

vacation, attempting to synthesize quinine, when he made this discovery. Realizing its significance and encouraged by the report of a firm of dyers, Perkin, his father and brother launched into manufacture of the new dyestuff. Technologically the moment was propitious: Mansfield, a student of Hofmann's, had just discovered a process for separating benzene from coal tar, Zinin, a student of Liebig's, had reduced nitrobenzene to aniline and Béchamp greatly improved the latter process. But all this hardly detracts from the credit due to Perkin for what Sir Robert Robinson calls "his active, forceful pioneering" in the new manufacturing techniques of coal tar colours.[1] For seventeen years Perkin's firm prospered until, in 1874, he sold it and returned to chemical research.[2]

Thus a number of conspicuous threads from Giessen were woven into the pattern of an important scientific industry. And the new enterprise was not limited to England: factories were set up very quickly in Germany, Switzerland and especially in France where important contributions were made by Verguin, Girard, De Laire and others, and where there was a great tradition not only in scientific chemistry but also in the art of dyeing. In Germany one important firm which manufactured the new aniline dyes had formerly made natural dyestuffs;[3] curiously, in England, no old dye works seem to have taken up the manufacture of the new dyes. Firms were either founded specially to make the new dyestuffs, like Ivan Levinsteins, started in 1864, or else they were like Read Hollidays of Huddersfield, tar distillers started in 1830 who took up aniline dyes in 1860.

Levinsteins and Hollidays were, by the end of the century, the largest and most active manufacturers of aniline dyes in Britain. There were very good reasons why they should be: they were located near their markets, in the textile areas of Lancashire and Yorkshire, raw material — coal tar — was readily available from gas works, and in both areas there was a traditional interest in scientific industry.[4] To these assets we should add the commercial experience of these localities, the

[1] Sir Robert Robinson, in *Endeavour*, XV (April, 1956) 94.

[2] S. Miall, *A History of the Chemical Industry* (London, 1931) 66 ff.; L. F. Haber, *The Chemical Industry during the Nineteenth Century* (London, 1957) 80, 128, 162.

[3] Leopold Cassella & Co.

[4] South Lancashire and the West Riding of Yorkshire strongly supported the Mechanics Institute movement between 1825 and 1851.

availability of capital and, in the case of Levinsteins, who were near Manchester, proximity to the largest and most active school of chemistry in England. It is therefore the more surprising that, by the end of the century, the aniline dyes industry had migrated to Germany, where about 90 per cent of the world's manufacture was carried out. In England the growth of the industry had been very slow indeed.

To revert to the question of systematic scientific education in England: the University Commissions of 1850 and 1854 paid little attention to research training in science, and the ideal continued to be a liberal education. The committee set up by London University in 1858 to consider the inauguration of science degrees similarly paid little attention to research: 'science in education' meant knowledge about science rather than the art and practice of science. An emphasis reinforced by the nature of the written examination system.

The change began in the late sixties and early seventies, and it came through two distinct, although related, movements. Grove, in his Presidential address to the British Association in 1866, asked for Government endowment of research, and this plea was taken up by others in the following years. The aim seems to have been that men of science should have available public research laboratories in much the same way that scholars were provided for by the British Museum Reading Room. This suggests that in the early seventies the typical British scientist was still thought to be an amateur; but now, thanks to the growing complexity and national importance[1] of science, his work was to be officially helped. The second movement began with the criticisms of conventional liberal education and its instrument the written examination by Matthew Arnold and Mark Pattison. An added impetus was given to this movement by a young philosopher, C. E. Appleton, who had studied in Germany and who, by the early seventies, had won the support of a number of men of different disciplines. Thus, of the eight contributors to the volume *Endowment of Research* (1876), only two were scientists (and of these, one, H. C. Sorby, was a distinguished amateur). This movement was therefore inspired by the achievements of German learning and scholarship in general, rather than by the practical achievements of German science. In fact, in 1873 the Devonshire Commission reported

[1] Among the sciences thought to be of the greatest national importance were surveying, meteorology and solar physics. See *Royal Commission on Scientific Instruction and the Advancement of Science* (Devonshire Commission).

that on no subject were academic witnesses so united as on the desirability of research training in scientific education. In America, this same belief led to the foundation of Johns Hopkins in 1876.

How did German and English universities compare in the matter of training in scientific research at this time? At Owens College, Manchester, H. E. Roscoe had built up what was certainly the biggest and very probably the best school of chemistry in this country. Roscoe, trained under Bunsen, believed that research should have a place in scientific education. He was a member of the Appleton group, and a paper he published in 1874[1] contained the observation that German chemical manufacturers were by then insisting that the chemists they employed should have had a research training. Thus the ideals of pedagogy and utility concur! In 1869 new chemical laboratories for Owens College were designed following the best German practice, and by 1871 they accommodated some sixty students. In 1874 the — for England — unprecedented step of appointing a second professor of chemistry was taken, when Carl Schorlemmer was made professor of organic chemistry. In the twenty years the College had existed, 1851–71, some 84 original papers in chemistry had been published.

But Owens College, in chemistry the best that England could achieve, was the only university institution in the great textile areas of the north. In 1865 A. W. Hofmann had returned to Germany, and for nine critical years there was no higher teaching of organic chemistry in England. So when Kekulé's theory came out in 1865 there were few in this country competent to understand it, and very few of these indeed would be in industrial occupations. In comparison with the achievements of Owens College, in the six years 1866–71, some 80 papers were published from the chemistry department at the University of Leipzig (or 89 in the seven years 1866–72). Indeed, some six times as many papers on chemistry were published in Germany in 1866 as in Britain, and this highly unfavourable ratio was maintained until the end of the century: in 1899 some two-thirds of the world's original chemical research came from Germany.[2] As early as 1872 Kolbe had

[1] H. E. Roscoe, *Original Research as a Means of Education* (1874).

[2] F. Rose, *Report on Chemical Instruction in Germany and the Growth and Present Condition of the German Chemical Industries*, *Miscellaneous Series, Diplomatic and Consular Reports* (Cd. 430–16) (1901).

claimed: "Why is German chemical industry now at a higher level than that of England and France? Because in England and France government policy does little for chemical education and Germany has outstripped both countries in that respect. Indeed it is through the scientific laboratories that the chemical industry of Saxony has contributed so much to the wealth and prosperity of the state."[1] A point of view in interesting contrast to Liebig's observation of 1844 that England was not the land of science, for "only works which have a practical tendency command respect, while the purely scientific works, which possess far greater merits, are almost unknown ... in Germany it is quite the contrary".[2]

Statistics can be produced to show not only that Germany endowed its universities much more generously than did England, but also that there were disproportionately more students in Germany than in England. But of course the social arrangements in the two countries were not the same, and great caution would be necessary in interpreting such statistics. Here I will merely observe that in 1890 there were twice as many German academic chemists as there were British (101 : 51), while by 1900 the proportion had dropped slightly in favour of Britain: 16 Germans to 10 British, or, taking account of populations, 12 Germans to 10 British — a rough indication which would suggest that the founding of new universities in Britain was tending to redress the balance. But, as the German technological universities have been excluded from this comparison, the case in favour of Britain has been overstated.

III
The practical consequences

There is, then, a general relationship between the numbers and quality of higher education in chemistry on the one hand and the development of advanced industrial techniques, such as systematic applied science, on the other.

The adoption of the simplest laboratory techniques by British industry began, it seems, only after the Exhibition of 1851. The first step appears to have been taken by a somewhat obscure ironmaster of Dudley, Samuel Blackwell, who was

[1] H. Kolbe, *Das Chemische Laboratorium der Universität Leipzig* (Braunschweig, 1872) xlv.

[2] Letter to Faraday.

responsible for the introduction of analytical, or control, chemists into iron works. It is true that the simple control, or standards, laboratory may develop in the course of time into a research laboratory, as for example the National Physical Laboratory has done. But this process is by no means inevitable and we must ask in which industry and for what reason the systematic research laboratory originated.

Systematic research means the employment of salaried research scientists engaged solely or substantially on research, on a permanent basis and to be replaced by others when they leave. Scientific research, in the form of an attack on one or two definite problems, to be wound up when the problems are solved, has been carried on in industry for a long time; there was, indeed, much of it during the eighteenth century. But this *ad hoc* procedure is not the same as institutionalized research which is a permanent feature, a continuing activity.

On this definition it seems certain that systematic industrial research began in the synthetic dyestuffs industry. The exact date is uncertain, but it must have been about 1870, and the location must have been one or more of the German firms which were, by 1900, to dominate the industry. At the end of the century one of these firms employed scores of graduate chemists on research, while many others worked on production, development, sales and services. There was no such scope for the employment of research scientists in the mechanical engineering industries, such as power generation, machine tools, textiles, etc. It is not evident, on the face of it, why this should have been so. The industries related to the biological sciences, such as agriculture, fishing, etc., were similar to the mechanical industries in this respect.[1]

When Perkin retired, in 1874, from the industry he had founded, it was prospering. He later gave as his reason for retirement his desire to return to chemical research. But, "a much more weighty consideration than this" was, said his son W. H. Perkin, the recognition that the firm could only be carried on successfully if a number of trained chemists could be recruited and employed in the all-important work of making new discoveries. "I remember", he says,

that enquiries were made at many of the British universities in the

[1] Edison's famous laboratory at Menlo Park, set up in the 1870's, and the well-known agricultural researches carried out at Rothamsted by Lawes and Gilbert were both highly individualist enterprises, and can therefore hardly be taken as typical.

hope of discovering young men trained in the methods of organic chemistry, but in vain. There cannot be any doubt that the manufacturer of organic colouring matter during the critical years 1870–1880 was, owing to the neglect of organic chemistry by our universities, placed in a very difficult and practically impossible position.[1]

According to F. M. Perkin, another of Perkin's sons, the original firm was in the early 1870's faced with the problem of rapidly expanding production in order to meet a sharp increase in demand. This meant more research chemists: "research chemists however could not be obtained".[2] True, German chemists were available, but they tended, after a while, to return to Germany, where their English experiences naturally made them much sought after. In the new dyestuffs industry the pressure of rapid change was very great: it was a matter of new colours, of new processes and the improvement of old ones.

This rings only partly true: the inevitable problems of growth in size and complexity are certainly familiar to students of management; but, as regards the absence of suitable scientists, it may be wondered why Perkin & Son were apparently unable to train their own research workers.[3] Surely, the opportunity of training for research under the leading English organic chemist of the time, who was also the founder of a unique industry, should have brought in a number of able young men. In this respect a comparison between Perkin and James Watt is not inapt: both were brilliant scientists and technologists, both founded revolutionary new industries, both had brilliant sons. Nearly a century before, Watt, with his partner, Matthew Boulton, had faced the same problem — shortage of talent — in an even more acute form. But whereas Watt and Boulton succeeded in attracting able men from all quarters — Southern, Clegg, Peter Ewart, Murdoch and others, so that the firm was called the "science school of Soho",[4]

[1] W. H. Perkin (Jr.), "The position of the organic chemical industry", *Journal of the Chemical Society*, CVII (1915) 557 ff.

[2] F. M. Perkin, *Journal of the Society of Dyers and Colourists*, XXX (10 November 1914) 339 ff.

[3] There was a good precedent: R. Calvert Clapham told the Parliamentary Committee on Scientific Instruction (London, 1868) that the alkali works on Tyneside commonly took apprentices into their laboratories for training as (presumably) control chemists. The facilities for scientific training at Durham University were, he said, very inadequate.

[4] Conrad Gill and Asa Briggs, *History of Birmingham* (London, 1952) I, 109.