FRANK PHILIP BOWDEN

1903-1968

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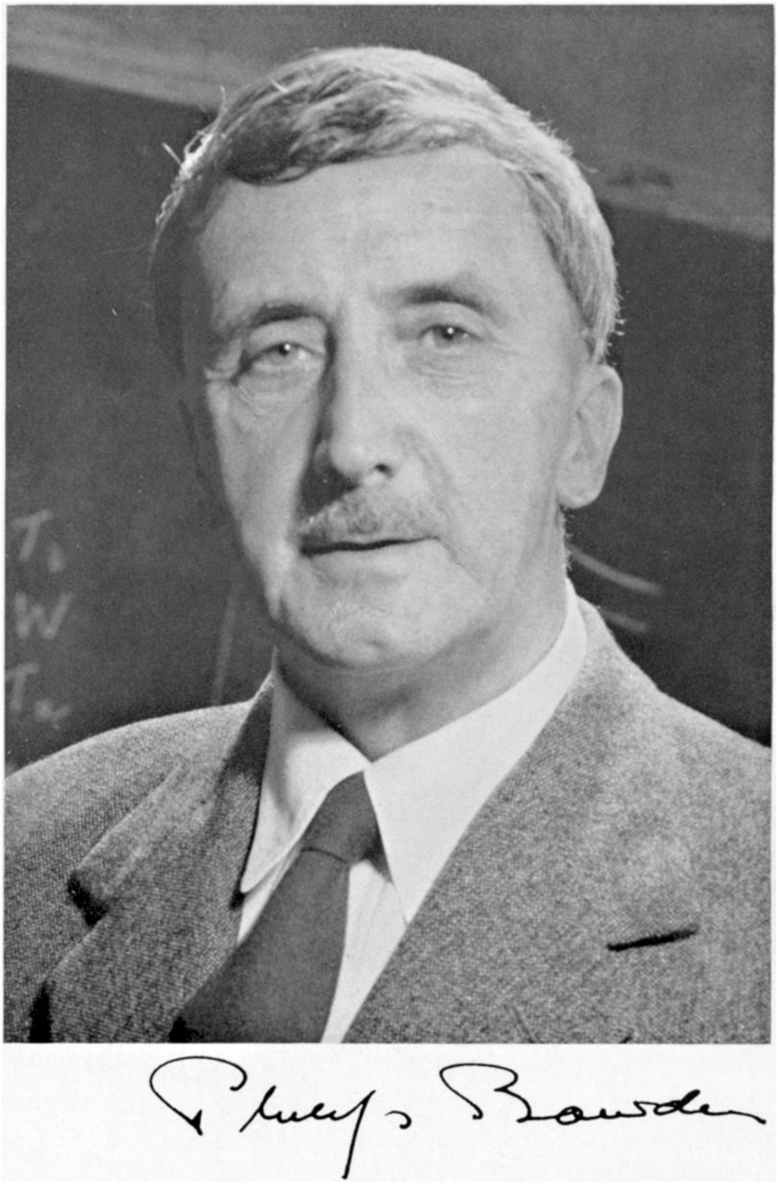
FRANK PHILIP BOWDEN died in Cambridge after a long illness on 3 September 1968 in his 66th year. At the time of his death he was Professor of Surface Physics at the University of Cambridge and Director of Surface Physics, a sub-department of the Cavendish Laboratory. Bowden was a man of many talents and could have made his mark as a writer, as a lecturer, as an aesthete, as a politician and statesman, as an administrator and man of affairs, as a scientist. In a sense he was, in fact, all these but his deepest and most sustained interest throughout his life was his laboratory and the challenge and excitement of scientific research. He was, indeed, an experimental scientist of great originality, perception and versatility.

I. THE TASMANIAN YEARS

Philip Bowden was born in Hobart, Tasmania, on 2 May 1903. He was the fifth of a family of six. Both his parents were born in Tasmania. His father, Frank Prosser Bowden, was an only child, and on account of the early death of his own father was brought up by his mother. Their financial situation was precarious and he left school at the early age of twelve to help support his mother. He was first employed as a messenger boy in the Postmaster-General's Department and subsequently became Telegraph and Telephone Manager for Tasmania. Philip Bowden's mother (*née* Grace Elizabeth Hill) was also an only child who lost her father at an early age. She was brought up by her mother who was a country postmistress. At the time that Frank Prosser Bowden was a Telegraphist, Grace Hill was assisting her mother in the country post office by sending off telegrams. It became a family tradition that the father and mother had their first meeting over a telegraph key.

Grace Bowden was of Irish descent, a kind and affectionate mother and a woman of great beauty. The marriage was a happy one, so that although she died when Philip Bowden was fourteen, her influence in providing a happy family environment was strong. Philip's adolescence was greatly influenced by his father who was a man of remarkable character. In spite of his modest income and the size of his family he was able to provide his children with a full, catholic education. He read poetry to the family, interested them in music, took them camping in the bush, and cultivated



their interest in the theatre. Philip's brother recalls of his father that 'his tolerance, kindness and sense of justice were immense. His sense of humour magnificent. There was a great affinity and indeed a strong physical resemblance between him and Philip.'

Bowden's early education started at the age of seven at a Dame's School and about two years later at the Hutchins School, Hobart. His scholastic record was satisfactory but by no means remarkable. He was extremely conscientious and his achievements were all attained by very hard work. He remained a hard worker with immense stamina and reserves of intellectual energy all his life. His studies progressed adequately until matriculation when he unexpectedly failed in mathematics and therefore failed to qualify for a university place. He was never good at mathematics and this had two important consequences. First in his later researches he always chose an approach which provided answers without the need for mathematics; a quality that is apparent throughout the whole of his career as a research scientist. Secondly he began work as a junior laboratory assistant with the Electrolytic Zinc Company in Tasmania and acquired there his feel for the experimental approach. On their advice he continued his studies under a tutor and, to the great delight of his family, finally matriculated. He entered the University of Tasmania in 1921 as a science student. In his second, year he fell ill and was advised by his doctors to rest for at least six months in a warmer climate. Through some Masonic connexion of his father's he was sent to an Australian inland station in the back blocks of New South Wales. There he rode horses, hunted kangaroos and acted as a jackaroo. But he also read. The main diet, he recalled, was mutton and potatoes. He made a complete recovery and returned to the University to continue his studies. In this he was much encouraged by Dr A. L. McAulay (later Professor of Physics at the University) who already recognized his quality and his potentialities. In 1924 he obtained his B.Sc. degree gaining high distinctions in three and distinction in four subjects. In 1925, working under McAulay in the Physics Department, he submitted a series of papers on problems connected with electro-chemistry and was awarded the M.Sc. degree with first-class honours.

McAulay was extremely keen to secure further support for Bowden's research. The Physics Department had already carried out some investigations of value to the Electrolytic Zinc Company and McAulay decided to approach them for a research grant. As is not uncommon in such circumstances it proved extremely difficult to secure an interview with the Director of the Zinc Company. After many abortive attempts, McAulay finally learned that at 8.30 every morning the great man was shaved at a certain barber's shop in town, and that this might be a good time to find him in a receptive, or rather a generous, mood. The meeting took place and forty years after the event Professor McAulay recalls the sound of the razor scraping away the managerial beard whilst the proposal was made and finally accepted.

Bowden was the first recipient of the Electrolytic Zinc Company's scholarship and this enabled him to continue his work for another year. His first three published papers describe the main body of his work with McAulay on hydrogen overpotential and related topics. In 1926 he was awarded an 1851 Scholarship and left the Antipodes to work under Eric Rideal in Cambridge.

As a young man in Hobart, Bowden greatly impressed his contemporaries and the other members of his family. They recognized his gifts of leadership his toughness combined with kindness and sensitivity. These characteristics remained with him all his life. Two other aspects of the Tasmanian period may be mentioned. He showed little interest in competitive or team sports, but was keen on sailing, walking and ski-ing in the Tasmanian highlands. Ski-ing remained a life-long hobby. Secondly he came under the influence of the Hutchison family. Their home provided a focal point for much of the cultural life of the city. With Hutchison père he spent many hours walking in the mountains learning to love the simple pleasures of the countryside. Forty years later, after a short visit to Tasmania, he remarked that he had rediscovered the beauty and attractiveness of that country.

Amongst the Hutchison children was a young girl, Margot. In 1931 she left Tasmania for Cambridge and became his wife.

II. Cambridge 1927-1939

The early Cambridge days

At McAulay's suggestion (himself a former Caius man) Bowden applied for and was admitted on 14 January 1927 as a Research Student at Gonville and Caius College. At that time Eric Rideal was a lecturer in the Physical Chemistry Department and it was here that Bowden began his Cambridge researches. Australian contemporaries who sometimes found the Cambridge atmosphere and College discipline uncongenial have remarked that Bowden fitted into his new environment as into a glove. He became strongly attached to Cambridge and resisted many attractive offers to go elsewhere: indeed, apart from the wartime separation, he spent the rest of his scientific career there.

Some idea of the early Cambridge years is provided by the following two extracts. The first is from an article Bowden wrote in 1956 in an issue of the *Journal of Colloid Science* in honour of Rideal on his retirement from formal academic life as Professor of Physical Chemistry at King's College, London.

> 'All those who have worked with Sir Eric Rideal agree about his gay and infectious enthusiasm for scientific work and about his conviction not only that all problems are soluble but that it will be great fun solving them. I have been enormously grateful for this from the day of my arrival in Cambridge as a shy student from Tasmania when he set me to work on a table in the basement among the dustbins (the only space available). All difficulties disappeared, and we even solved, after some months of hard work, the problem of the removal of the dustbins.'

By a strange coincidence, nearly thirty years later, Bowden's research group found its home, as a sub-department of the Cavendish Laboratory, in the old Physical Chemistry Department. The dustbins, larger and now on wheels, are still to be found not far from their earlier location.

The second extract is from C. P. Snow who has contributed the following recollections.

'Philip Bowden as a research student at Cambridge showed most of the gifts of mind and personality that came to full development later. In fact, except that he became happier, he didn't alter much with the years. In 1928 when I (C.P.S.) first met him, one could have guessed a good deal of what, with good fortune, he would manage to achieve. A research student at that period was something of an odd-man-out in Cambridge; it was only a few years since the Ph.D. had been introduced and by modern standards the post-graduate schools were small and in some ways amateur. The Laboratory of Physical Chemistry—where Bowden and I were working—was specially eccentric. The Professor was Martin Lowry, a very clever man who had never been accepted in Cambridge (he was a bit of an injustice collector) and who had, with a curious kind of obstinacy, got stuck with researches on optical rotation that didn't attract many pupils. Whereas E.K. (now Sir Eric) Rideal, who was the Humphrey Owen Jones Lecturer, was willing to accommodate research on any topic from pure physics to biology, and his sub-department accordingly became a kind of hold-all for anyone who thought he had a decent problem. Bowden was busy with electrode potentials: Henry was following an idea of Blackett's on specific heat: I wanted to go on with molecular spectroscopy: and so on. The result was that we formed a fairly tight-knit community. We hadn't many undergraduate friends: we were rather too old for that, and we were leading a different kind of life. We worked pretty intensely, longish hours and, of course, most weeks of the year. We talked a lot of science, played poker on Sunday nights, had supper together at the Bath when our college kitchens were closed. It was in that way that Bowden and I formed a friendship that lasted until his death. I think I realized very early that this was a character one wasn't going to meet twice in a lifetime. He was sensitive and also strong-willed, driving himself, either at science or in the physical activities he loved, abnormally hard. He was both charitable and sharp-tongued: I thought, all through our friendship, that though I didn't much like being judged by anybody, if I had to be judged at all I should choose Philip Bowden.

'At that time he was lonely, determined and properly ambitious. It was a long way from Tasmania, and, though he loved his native land, he didn't intend to go back. He longed to do scientific work which would justify his existence: he had the genuine scientific curiosity, and would have been some sort of scientist whenever and wherever he was born. He also meant to stay in Cambridge, for which he formed an affection at first sight and one he never lost. Staying in Cambridge, though, was not so predictable as it would have been a few years later. It wasn't altogether common form, though it did occasionally happen, for research students to be elected to fellowships. I remember waiting in his bleak lodgings for the news of the Caius election. As it turned out, Caius had the good sense to snap him up and from that time Cambridge was his home and his scientific fulfilment lay ahead: although, because of the originality of his mind and the unfashionableness of the subjects that interested him, he didn't obtain full recognition early enough.

'Still, from 1929 he was enjoying a life that most of us envied. He even suddenly became quite well off: a college fellowship on the top of an 1851 Senior Studentship meant financial comfort in those days. His exceptionally happy marriage and family life were soon to follow: but, for a year or so he occupied a handsome set of rooms in college, and there we talked about our hopes, dissatisfactions and desires. He was, even at that age, very cultivated in any art that I knew something about: he was widely read and his taste was firm and very much his own. With politics he was never engaged: he had the normal apprehensions of an intelligent man as we began to see the '30s take shape, but no more than that. He would have been very happy, I sometimes thought, as a nineteenth-century scientist, polishing off pieces of classical physics, mountaineering with Tyndall, a patriarch among his family, entertaining the high-thinking writers and painters of his time.'

Bowden's friendship with Snow was combined with a collaborative research effort. Around 1930 they began to study the photochemistry of vitamins A, B, C and D. The field was new, fashionable and exciting for it combined spectroscopy, photochemistry, and vitamins. Their experiments suggested that vitamin A could, indeed, be synthesized by the ultra-violet irradiation of the appropriate precursor (β-carotene). The work was given considerable publicity in the press as well as in the scientific journals but later studies showed the conclusions to be erroneous; the synthesis, in fact, is effected by enzyme action. This was a source of some embarrassment to Bowden and one of the few occasions on which his judgement was at fault. But his heart was in research and he turned confidently to other fields. Snow, whose interests were different, left science for administration and for novel writing. His account of this episode, somewhat disguised, in *The search* is probably the most authentic novel he has written of University life and of scientists at work. In his later novels of Cambridge life, Snow drew on Bowden as the prototype of Getliffe, the gifted, wise and sensitive scientist.

'The break up of the Papal State'

At the time that Bowden came to Cambridge, Martin Lowry was the Professor of Physical Chemistry, but the titular and administrative head of the Chemistry Department was Sir William Pope. In 1930 the University decided to set up a new department, that of Colloid Science, under the Professorship of Eric Rideal. The research students naturally split up according to the fields they were working in, some joining Rideal and others remaining under Lowry. Bowden had just been made a University Demonstrator under Lowry and decided to stay with him. The discussions and passions at the time of the departmental fission must have been lively and possibly acrimonious. This is reflected in a little laboratory magazine called *The surface* edited by H. Kenneth Whalley with the active support of Dr A. S. C. (Soapy) Lawrence. In the June 1931 issue there is a short anonymous poetic drama entitled 'An old Spanish custom' in which a Court of Enquiry discusses the rights and wrongs of the new Department and the new loyalties evoked. To Bowden is attributed the following:

'My calm and calculating brain is rarely undecided
Yet I wavered (but to no one until now have I confided)
Then Good Fortune—I'm her husband—gave me yet another dowry;
I was made Demonstrator; *ipso facto* I'm with Lowry.'

The phrasing is acrid and uncharitable. But the comment reveals two aspects of Bowden's character that were intrinsic. He always gave long and careful consideration to important issues and because of his wisdom and practical judgement generally made sensible and advantageous decisions. Secondly, he was a man of very great reserve, and except to those who knew him intimately, rarely exposed his innermost thoughts.

The Humphrey Owen Jones lectureship vacated by Rideal in 1930 was occupied by Norrish: in 1937 (after Lowry's death in 1936) he was promoted to the chair of Physical Chemistry and Bowden was appointed to the lectureship, a position he held until the end of World War II. In the meantime his connexions with his College strengthened. He was made an unofficial Drosier Fellow in 1929, Director of Studies in Natural Sciences in 1933 and an Official Fellow in 1934. Because of his over-riding interest in research his college teaching of undergraduates was not an unqualified success. But his college interests were warm and genuine. He strongly supported the creation of additional Research Fellowships. He was an active member of the Wine Committee. He was a witty and engaging conversationalist at High Table. In later years he was something of an elder statesman, bringing his wisdom and equanimity to bear whenever differences arose amongst his colleagues.

Electrochemistry

Bowden's first papers on electrochemistry with McAulay were concerned with changes in the surface tension of mercury associated with the establishment of hydrogen overpotential: later he studied the effect of metallic impurities on this process. In Cambridge, with Rideal, he showed how the charging curves could be used to estimate the real surface area of electrodes. This theme recurs in other papers (e.g. with E. A. O'Connor in 1930), but perhaps his most important contribution to electrode kinetics was his study of the kinetics of electrodeposition of H_2 and O_2. In conjunction with his other papers it emphasized experimental points which are now commonplace, e.g. the use of low-current densities and the need to remove impurities, both depolarizers and 'poisons'. In addition it did much to turn the study of 'overpotential' into a branch of reaction kinetics and to establish experimentally that the electrodeposition of hydrogen and oxygen involved passage over fairly large energy barriers. In retrospect it would seem that the criteria of solution purity were not sufficiently stringent and contamination, perhaps by adsorbed organic molecules, probably accounts for some of the low values deduced by him for double-layer capacities.

In 1931 (and later with L. E. Price) he described an effect which has recently attracted much attention, namely the acceleration of discharge of hydrogen (on mercury) and of oxygen (on platinum) by light of short wave-length. This period marked the beginning of a shift of interest, but he continued to stimulate and direct electrochemical studies until the outbreak of war. The subjects included the overpotential of deuterium (H. F. Kenyon); oxygen and halogen overpotentials (H. W. Keenan); hydrogen overpotential at very low current densities (K. E. Grew); electrode reactions in fused salts and transport processes (J. N. Agar); deposition of the azide ion (H. P. Stout); metal deposition and the behaviour of highly active platinum electrodes (G. C. Barker). Only one of his students (L. Young) continued to work on electrochemical processes in the post-war period and it is typical of Bowden's ability to extend one line of work into another, that this dealt with the effect of interfacial potential on the friction of platinum surfaces immersed in sulphuric acid. The results showed that the friction was a maximum in the range where neither oxygen nor hydrogen was absorbed and a minimum where oxygen was present.

Dr Agar, who has contributed most of the above details, adds the following personal comment showing Bowden's preference for simple concrete physical concepts rather than abstractions:

> 'I remember an occasion in 1938 when we were discussing transients in diffusion-controlled electrode processes. I was pushing the idea of treating such systems as reactive circuit elements characterized by resistance and capacity—an approach which has some merit from an operational point of view but is a long way removed from the molecular processes ultimately responsible. In the course of our discussion, I said "Suppose this thing was a black box with two terminals"; he promptly replied, "But is isn't a black box, it's an electrode".
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> This rather trivial anecdote illustrates the way Bowden's mind worked. It might also give the impression that he was not receptive to suggestions from his students. Nothing could be further from the truth. We were, in fact, allowed a very free hand and could modify our programme of work almost as we saw fit, and often his students got credit for ideas that were really his own.'

The early friction work

Bowden's most active period in electrochemistry covered the first decade of his scientific research. Gradually his interests began to move into other fields. One of the overt influences was that of Sir William Bate Hardy, a senior member of Caius and the only Fellow of the Royal Society ever to have delivered both the Croonian and the Bakerian lecture. Hardy had begun life as a physiologist and after establishing an international reputation on the structure of cell protoplasma and the nature of the cell surface, turned his attention to other surface problems including colloid chemistry, adhesion and lubrication. In 1928 Hardy and Miss Nottage published results showing that a polished steel cylinder placed on a steel plate remained at a distance of 4000 nm above the plate both in air and in the presence of various organic substances. They attributed this to the action of long-range forces. 'To explain this it would be necessary to postulate oriented chains of molecules extending from the surface for several thousand molecules', Bowden wrote in a joint paper with Stewart Bastow in 1931. 'This is contrary to most physical evidence which shows that the effect of a surface on a gas does not extend for more than a few molecular layers. The first molecular layers may be oriented and strongly held by the surface but the degree of orientation decreases very rapidly and becomes inappreciable at a distance of a few molecular diameters from the surface. The point was discussed with Sir William Hardy and at his suggestion further experimental work was undertaken on this anomalous effect.' In this paper which was communicated to the Royal Society by Hardy himself, they showed that for scrupulously clean surfaces from which dust was carefully excluded no such floating occurred. They attributed Hardy's earlier observation to dust. In their view there was no evidence for surface forces extending through air, water or alcohol for thousands of nanometres. The range could not have been more than a few or at most a few tens of nanometres.

This paper represents Bowden's first move towards a study, not connected with electrochemistry, of solid surfaces and marks the beginning of his involvement with friction and lubrication. It was soon followed by an investigation of kinetic friction (with W. G. Beare): of frictional 'hot spots' using the rubbing metals as their own thermocouple (with K. E. W. Ridler): the role of frictional heating and surface melting in the formation of the Beilby polish layers (with T. P. Hughes), the importance of adhesion in the friction of clean metals (with Hughes); the measurement of the real, as distinct from the geometric area of contact between stationary and sliding surfaces (with D. Tabor): and the action of adsorbed long chain molecules as boundary lubricants (with L. Leben). The latter work closely paralleled Hardy's earlier studies on the same subject but the results pointed in a somewhat different direction. Hardy had emphasized the reduction in surface forces by the presence of an interposed separating layer. Bowden thought more concretely in terms of a thin boundary film of low shear strength as the main factor in reducing the friction.

These early experiments laid the foundations for much of Bowden's later work on friction. His electrochemical studies had shown that even carefully prepared metal surfaces were rough on an atomic scale. Coulomb and the followers of the French school who had worked with surfaces that were macroscopically rough held the view that when such surfaces are placed in contact they fit together as parts of a jig-saw puzzle making contact over the whole of the interface. Bowden saw that this could not be so. 'Putting solids together', he said in a popular B.B.C. broadcast some years later, 'is rather like turning Switzerland upside down and standing it on Austria—the area of intimate contact will be small.' He emphasized that the contact pressures could produce local plastic yielding so that the real area of contact would depend simply on the applied load and the yield pressure or hardness of the metals. Consequently the area of true contact would be proportional to the load and independent of the *apparent* size of the bodies. At these regions, particularly if the surfaces were clean, strong intermetallic adhesion could occur. These conclusions provide the basis for the two 'laws' of friction deduced by Amontons in 1699 and by Coulomb in 1781, that the friction is proportional to the load and does not depend on the size of the bodies. Later work has emphasized elastic rather than plastic deformation of the contact regions, as well as a more analytical description of surface contours. Nevertheless, all these models fall back on Bowden's basic view that contact depends primarily on the way in which the individual asperities themselves deform.*

Because the area of contact is small the frictional energy is released over a very small volume of material. This readily leads even at modest sliding speeds to very high temperature flashes, perhaps even to local melting, although the bulk of the bodies may remain quite cool. The idea of frictional melting played a vital part in Bowden's view of the formation of the polish layer. It also hinted at the suggestion that with lubricated surfaces frictional hot spots might produce local breakdown of the lubricant film precisely at those regions where lubrication was most important. This received analytical development by Blok and independently by Jaeger and acquired experimental support a little later from my own experiments in the Britannia Laboratory.

The art and science of ski-ing

Bowden's researches on frictional hot spots had a direct bearing on one of his most cherished hobbies—ski-ing. The early background is described in Bowden's typical style in a short article he contributed to the *British Ski Year Book* in 1956. The episode described took place about 20 years earlier:

> 'It was on our third snowbound day in the Concordia hut that we found a battered copy of *Daisy's aunt* by E. F. Benson. There was neither food nor fire in the deserted hut and we were cold, hungry and

* An independent study along the same lines had apparently been carried out a little earlier than this by Ragnar Holm at the Siemens' Laboratory in Berlin.

infinitely bored. Outside the blizzard continued to sweep down the Aletch Glacier and pile the snow in a great drift against the window. It was obvious that we were stuck for at least another day so that a novel to read was indeed a treasure. Meticulously we divided it in half and tossed. I lost the toss—but settled down contentedly enough to read the second half. It was finished all too soon and, eager to know the beginning of this enthralling story, I turned to my companion to swap. He was still reading, and it was with growing impatience I watched him linger over every sentence and slowly turn each page; clearly I would have to think of something else.

'We had ski-ed down to the hut from the Jungfraujoch on a very cold day. Although the temperature of the snow was below −5 °F our ski, once we got them moving, had run reasonably well. It is curious that the friction on ice and snow can be so low—it may be less than a tenth of that observed on other crystalline solids. This problem had been considered by Osborne Reynolds about 1900. He suggested that the low friction of skates (in those days no Englishman ski-ed) was due to a water film formed by pressure melting. A similar suggestion had been made earlier by Professor Joly. If a substance expands on freezing, and this is so with ice, it can be melted by pressure. Some calculations made on the back of *Daisy's aunt* showed, however, that it was most improbable that a pressure sufficient to melt snow at −5 °F could ever be reached on ski—even by an elephant. On the other hand calculations of the amount of the heat liberated by the frictional rubbing of the ski suggested that this might (even when they were moving quite slowly) be sufficient to melt a very thin layer of water at the points of contact of the snow crystals. At this moment my companion relinquished his half of the novel and speculation ceased. Later there was a break in the weather and we made a get-away.

'An experimental scientist is necessarily chained to the laboratory. If he is wise he will invent a problem which takes him to some desirable place. It is also refreshing on occasion to turn one's attention to the physics of some simple everyday processes. Scientists in the nineteenth century were better at this than we are and there is much to be said for maintaining a tradition. With these supporting excuses I carried out in 1938, in collaboration with Dr T. Hughes, an experimental study of the friction of ice and snow. We joined Mr Gerald Seligman at the Jungfraujoch Research Station. He, with the same laudable motives was studying the mechanism of glacier flow. In the mornings we climbed the lovely peaks of the Bernese Oberland and in the evenings conducted our researches. These experiments, which were made on a laboratory scale with miniature sliders, supported the view that the low friction of snow and ice was due to a surface melting. They showed, however, that pressure melting is significant only at temperatures very close to the freezing point and with slow moving surfaces. If the temperature is below freezing the local surface melting is produced not by pressure melting but by frictional heating of the sliding surfaces. In ski-ing and sledging it is this frictional heating and melting which is primarily responsible for the low friction. Apparently the calculations on *Daisy's aunt* were right. . . .'

In post-war years after he had noticed the extremely low friction and poor wettability of polytetrafluoroethylene (P.T.F.E. or Teflon) he suggested that this material should prove beneficial on ski or sledge surfaces since its friction is low and depends little on the condition of the snow surface. In his Royal Society paper describing this work (1953) he acknowledges his thanks to his wife Margot who carried out some of the model ski experiments with him at the Jungfraujoch Experimental Station.

Cambridge and the outbreak of war

Bowden's work in the first dozen years of his Cambridge career achieved considerable recognition, first in electrochemistry which he was beginning to leave, secondly in friction and lubrication which he was beginning to enter. In 1928 he received a Rockefeller Foundation Fellowship for Research in Physics and Chemistry; in 1933 the Tasmanian D.Sc. degree; in 1938 an award from the Beilby Memorial Fund for research on the physical properties of surfaces; in 1938 the Cambridge Sc.D.

Bowden's work on friction and lubrication received support and encouragement from Hardy and from Dr David Pye both of whom were members of the Lubrication Research Committee of D.S.I.R. Very soon it attracted the attention of the oil companies and of other industrial concerns in England, Holland and America. Bowden began to be consulted by Shell, and in 1937 they established, under his supervision, a small research unit to deal with wear and lubrication. It was called the Britannia Laboratory and was situated at the north end of East Road near McKay's Engineering premises. Its founding member was Dr H. F. Kenyon who carried out some beautiful experiments on wear between lubricated surfaces. I joined the group in 1939 and it was here that I observed the reversible breakdown of boundary lubricants with temperature as the surface films were melted or desorbed. This work was continued by Dr J. J. Frewing after I had left to join Bowden in Australia.

The Air Ministry, the Fuel Research Board and the War Office showed increasing interest in the work and in Bowden's approach. Members of the Air Ministry Research Laboratory at Farnborough came to Cambridge to follow his methods and to discuss ideas of mutual interest. Bowden himself made a number of visits to Farnborough to study the type of wear, lubrication and corrosion problems that might arise in the event of war. During the early months of the War whilst Bowden was abroad and his return uncertain, Dr T. P. Hughes took over responsibility for the University group which then consisted of W. Hirst and G. C. Barker. Gradually the group disbanded, its members going into Government or industrial research laboratories. The Britannia Laboratory was also closed down and Kenyon continued his work in the Shell Laboratories at Thornton.

III. World War II. The Australian laboratory

In the summer of 1939 Bowden went on a lecture tour to America, his first visit to that country. He decided to return via Australia to visit friends and relatives there. His wife and their first child had meanwhile arrived direct from England. While in Australia the war broke out and Bowden was much concerned with the part that he could play in the war effort. The question was whether he could be more effective back in England or in Australia. After some consideration he came to the conclusion that he could be of more value to Australian industry at that phase of its development. As a result he approached Sir David Rivett, Chief Executive Officer of the Council for Scientific and Industrial Research (forerunner of the Commonwealth Scientific and Industrial Research Organization) and discussed with him the possibility of doing research work, preferably in Melbourne, to assist the Australian war effort. His memorandum to Rivett in September 1939 is a typical Bowdenesque document: a forthright statement of what his work had already achieved at Cambridge, its implications for the testing and improving of lubricants, for the development of bearing materials, for the selection of driving bands for shells, and finally a modest but firm statement of requirements. There was strong support from the University of Melbourne, from Mr L. P. Coombes (Head of the C.S.I.R. Aeronautical Laboratory) and from the aeronautical industry in general, as well as from Rivett himself. Some industrialists, however, were not sure that Bowden's work in Cambridge was practical enough for their use in Australia and suggested that it might be more useful for him to return to Cambridge to continue his fundamental studies there.

The Minister, the Rt Hon. R. G. Casey (now Lord Casey), who had been trained as an engineer, took a special interest in the case and was rather inclined to the view at first that it might be better for Bowden to return to his fundamental research work rather than to stay in Australia at that time. After having, at the suggestion of Sir David Rivett, discussed the whole matter with many industrialists, Heads of Commonwealth Departments interested, university scientists, and others, Casey was not certain that he should accept the earlier view: he therefore called Dr Bowden to an interview with him.

In his interview with the Minister, Bowden was so effective in presenting his case that he convinced the Minister completely of the practical promise of the work he could do in Australia on lubrication and bearings. On 25 September 1939 the Minister wrote to Rivett saying, *inter alia*, 'I asked Dr Bowden to come to see me and after hearing his story I take a different view. I believe that his work has a definite practical value—(a) in recommendations as to bearing metals, and (b) as to the most suitable lubricants for specific jobs.' Casey then went on to say that in common with most of those who had considered the matter and who had a right to make a judgement, he now favoured the idea that Bowden should be invited to stay on in Australia to carry out the kind of research work which he had suggested in his memorandum. He said further that provided co-operation with the University of Melbourne was assured and provided the Executive Committee were satisfied, he would authorize an appointment for Bowden in C.S.I.R.

In the event, Bowden was appointed to the staff of C.S.I.R. as from 1 November 1939 as Officer-in-Charge of a section which was given the title 'Lubricants and Bearings'.

For some months in early 1940 this section was housed in the Engineering School of the University with the full support of Professor Burstall and then transferred to the new Chemistry Building then just completed, where the Professor of Chemistry, Professor Hartung and his staff welcomed the team and co-operated to the full with them in the most cordial and effective manner.

The group began as a very small team, but by the time I joined it in 1940 it was a going concern. Gradually it grew and by the end of the war had a research staff of nearly a score. Its contacts with the University Departments, especially Chemistry, Engineering, Metallurgy, Physics and Mathematics were warm and friendly and a fair amount of interaction with men and ideas ensued. Work of specific value to the war effort included the evaluation of special lubricants for machine tools and for aircraft, the development of satisfactory casting techniques for the production of aircraft bearings (with H. W. Worner and R. W. K. Honeycombe) and the successful formulation of flame-throwing fuels. There were other lines of work not originally planned which proved to be highly successful. In studying the penetration of sheet metal by bullets and the possibility of increasing penetration by lubricating the bullet nose, it became necessary to determine the bullet velocity. At that time a young Tasmanian engineer had joined the laboratory. His name was J. S. Courtney-Pratt and he proved to be the most unusual, the most fabulous and probably the most original worker ever to be associated with Bowden's researches. He soon devised a simple apparatus for measuring bullet velocities which attracted the attention of the military authorities. With their encouragement he turned his attention to the construction of semi-portable equipment suitable for calibrating large guns on naval vessels. The principle was simple. The shell passed over the bed of the apparatus, interrupting daylight entering through two narrow slits about 3.50 m (10 ft) apart: the time between each interruption could be measured with an electronic counter. With great energy and drive (working an average of fourteen hours a day) and with the assistance of Dr (later Professor) A. E. Ferguson, he produced equipment which could measure shell velocities to one part in two thousand. Courtney-Pratt used it to calibrate the main guns of the larger ships of the Australian Fleet. He then travelled with the equipment across half the globe (my impression is that he slept with it all the way for fear of getting it mislaid) in order to carry out a similar mission for the British Navy: but by the time he reached England the end of the European war cut this programme short.

Courtney-Pratt continued to develop apparatus of great originality and effectiveness. In the Cambridge laboratory which Bowden re-established after the War, he constructed a high-speed camera using an image converter tube: in its modern form it can take individual pictures at an exposure time of 10^{-9} s. He also constructed a completely original high-speed movie-camera using a fly's eye lens system. In its simplest form the only moving part was a Nipkow disk capable of changing the angle of attack of the light beam. A single photographic plate could then hold all the information of *ca.* 300 successive frames in apparatus scarcely bigger than a conventional plate camera.

Initiation and detonation of explosives

Another line of work which soon attracted Bowden's attention was the initiation of explosion and the growth to detonation of explosive materials. He was particularly concerned with the occasional unpredicted explosion which could completely destroy an explosives factory: by its very nature it also destroys all the evidence as to its cause. Bowden had shown that rubbing surfaces could generate very high local temperatures even though the bulk temperature remained low and it seemed to him that heating by impact or friction during the handling of explosives might well be the cause of factory explosions.

Whilst still housed in the Engineering School he began experiments using the well of a staircase as a convenient height from which to drop steel balls on to droplets of nitroglycerine placed on a heavy brass anvil. Since no explosions were obtained the technique was changed and brass slugs were fired from an air-gun at glancing incidence on to the nitroglycerine. Again explosions were rare. At this stage, in 1942, A. D. Yoffe joined the group (now housed in the Chemistry Department) and suggested that the reason for the rare occurrence of explosions was simply that the nitroglycerine escaped during impact. When cavities were placed into the nose of the impacting solids the sensitivity to impact was extremely high and a 40-g weight dropped from a height of only 1 cm or so gave explosions. The results obtained were still erratic, however, until Bowden suggested introducing tiny gas bubbles into the cavity (order of 0.1 mm diameter). The sensitivity then became extraordinarily high and reproducible. This at once suggested that explosions were initiated by hot spots produced by the adiabatic compression of trapped gas bubbles. The explosives work now began 'to make sense' and acquired a role of increasing importance in the laboratory. Those engaged in the early stages of the work included A. D. Yoffe, M. F. R. Mulcahy, R. G. Vines and Dr F. W. Eirich who had been brought from Britain to Australia in the *Dunera*, and was later extracted from an internment camp in Victoria. Their work showed that viscous heating during the extrusion of liquid films between heavily impacting surfaces could also provide a means of initiating explosion. Quite apart from its importance in the explosives field this work stimulated a major theoretical study by Professor (later Sir) T. M. Cherry of the 'Flow and generation of heat in compressed films of viscous liquids' (1945). A simpler version was published by Eirich & Tabor (1948). Yet a third form of hot-spot initiation could be provided by frictional heating. By introducing grits of known melting points between rubbing solids, Bowden (with Stone & Tudor) was able to show that the temperature of the hot spot necessary to produce explosion was of the order of 500 °C.

These studies, apart from their scientific importance, had other consequences. From the point of view of the safe handling of explosives they showed the importance of avoiding trapped air-bubbles; they also suggested that low melting-point alloys should be used in manipulating explosives since this would limit the temperature of frictional hot spots. Finally the work greatly stimulated the development of high-speed cameras by Courtney-Pratt as a means of studying in much greater detail the initial stages of initiation and detonation.

Friction, lubrication and other studies

The laboratory produced a good deal of rather important and fundamental work in other fields. It was during these years that Bowden developed (with Tabor) his theory of metallic friction, attributing it to two main processes: the shearing of junctions formed by adhesion at the regions of real contact and the ploughing of hard asperities of one body through the surface of the other. Together with Gregory & Tabor he studied the mechanism of boundary lubrication and showed that fatty acids were generally more effective than paraffins or alcohols because they reacted with the metal surfaces to form a thin soap film. This lubricated effectively up to its melting point and provided some lubrication even beyond this temperature. Again with Greenhill, Gregory & de Kadt he showed that extreme pressure lubricants were, in fact, extreme temperature lubricants. They contained a labile group which, when conditions produced a dangerous rise in surface temperature, could react to form a protective surface film capable of preventing metallic seizure. With Courtney-Pratt & Tudor he measured the breakdown of lubricant films in an internal combustion engine by measuring the electrical resistance between piston-ring and cylinder wall, a technique that has been widely used since then by other workers.

Finally we may mention an unexpected outcome of some researches by R. W. K. (later Professor) Honeycombe which arose from his work on the casting procedures and properties of bearing alloys. He observed deformation characteristics produced by repeated heating and cooling. Together with Dr W. Boas of the Metallurgy Department of the University, this led to a classical piece of work on the internal stresses produced in pure metals and alloys by thermal cycling as a result of anisotropic thermal expansion.

A change of name

The laboratory had achieved a fine reputation both for its fundamental research and for its practical work. It was functioning in great style when the War in Europe ended. Bowden resigned in 1945 and returned to Cambridge. I was acting head until the Section was taken over by Dr Stewart Bastow, himself a Tasmanian and a former Cambridge colleague of Bowden's. In 1946 the title of the section was changed to the 'Section of Tribophysics' and this continued until in 1948 it was given the status of a full Division—The Division of Tribophysics—with Dr Bastow as its first Chief. A few years later Bastow became an Executive Officer and Dr Walter Boas was appointed Chief. During all the subsequent years the Division has continued, to a large extent, to maintain the traditions which Bowden established.

A few words may be added about the origin of the new name. When Bowden left he suggested that, for the sake of the future of the laboratory, it might be desirable to find a more scientific and romantic name than 'Lubricants and Bearings'. The idea was to find a name which sounded scientific but which was so ill-understood that it would not prevent us from continuing to work in any unconventional field that we fancied. I finally produced the word tribophysics (from tribos meaning "rubbing") and tried it out on Professor Cherry. His first guess was that it had something to do with tribes and the measurement of cranial characteristics. Bastow was delighted when he heard of this on his arrival and was happy to sponsor the new name. The recent developments in this country of Tribology have, alas, removed all mystery from the official appellation of the Division.

A war-time memorandum

Bowden paid an official visit to England early in 1944 to discuss war work of mutual interest. He renewed contacts with old colleagues and began to explore the prospects of resuming activities in Britain when the war would be over. In March 1944 he prepared a memorandum (a copy of which was deposited with the Royal Society) entitled 'A study of the physical and chemical phenomena associated with the rubbing and with the impact of solids'. In it he described briefly his earlier work in Cambridge and his current work in Australia; he then went on to discuss the desirability of continuing similar research in Cambridge as soon as it would become possible to leave the Australian laboratory. The research work was to deal with friction, boundary lubrication, and chemical decomposition induced by stress. The approach was to be fundamental, but another aspect was stressed, 'the application to practical problems and practical developments'. Bowden then added: 'War experience has shown clearly that, in pre-War days the academic and practical research were too widely separated and suggests that, in future, contact between the two should be maintained. The action of joint committees and frequent meetings and discussions can do something towards this. It is obvious that one method of making this contact more real is by the interchange of personnel, and it is suggested that in this case arrangements might be made for certain members of the Service and Industrial Research establishments to come to Cambridge and work for a time, normally for not less than a year, and conversely for members of the Cambridge group to go into the Research establishments . . .' The Cambridge laboratory did in fact initiate such a policy and collaboration of this sort, particularly with government laboratories, continues to be actively pursued.

Bowden also raised another point—collaboration with Dominion Research Establishments. He saw this as a two-way exchange and expressed the hope that 'certain members of the Lubricants and Bearings Section in Australia will come to Cambridge to work, and to take a research degree, and that others will go out from Cambridge to Australia . . .'. This, too, has proved to be a successful and durable relationship.

Bowden concluded with two very typical points that reflect his empirical approach:

'It is not suggested that any elaborate research plans or organization plans need to be made at this stage. What is necessary now is some plan which will make it possible to start again at Cambridge a research group working on the problems in the way that has been outlined.' And again, 'It is not proposed that the group should be large, in fact, it should be reasonably small, and it can grow or die according to its use and the work it turns out. The work would fit into the framework of the Physical Chemical Laboratory.'

The memorandum was warmly supported by Sir Ralph Fowler who wrote: 'I hold most strongly that he should be given every encouragement and opportunity to develop such a project on the scale contemplated, and am confident that all parties concerned, in Cambridge, in the Services, in industry and in the Dominions will benefit greatly from its establishment.'

Bowden's proposals also won the approval of Sir Henry Tizard, Dr Owen Wansborough-Jones and Sir Ben Lockspeiser.

IV. Cambridge 1945-68

When Bowden returned to the Physical Chemistry Department in Cambridge he set to work to recreate his research group. For this he needed financial support, men and accommodation. Funds available for research from University and Departmental sources were negligible. Even in later years they were never large. His main support came in the form of a grant from the Ministry of Supply (Air) and this enabled him to acquire basic equipment, take on workshop personnel and engage research staff. This grant was maintained throughout Bowden's lifetime and there is little doubt that, without it, the beginning of the laboratory and its subsequent growth would have been immeasurably more difficult if not impossible. Active interest and help were also provided by Sir Edward Appleton and the Department of Industrial Research.

Courtney-Pratt had already preceded Bowden with his muzzle velocity measuring equipment but at that stage he was involved with Service matters at Shoeburyness. Bowden's first students were A. D. Yoffe and A. J. W. Moore from the Australian laboratory and they were followed by Courtney-Pratt and E. B. Greenhill; from England came L. Young, J. E. Young, L. Burns, R. Whitehead and P. Gray. Early in 1946 I left Australia and returned to work under Bowden once more.

Bowden was given an office on the top floor of the Physical Chemistry Building: later he moved to a long narrow room on the ground floor overlooking the rear end of the Mond Laboratory. For his research he was given part of the old Anatomy Building facing Corn Exchange Street. The runnels in the floor for washing away the blood of the dissections remained as a perpetual reminder of the original purpose of the area. One portion was set aside as a small workshop, another corner as a glass-blowing unit, another as a dark-room. Later an outside coal-store was cleared out and converted into a high-temperature room. The last stage of expansion at this phase was the occupation of a corridor of the old Zoological Museum. Bowden gave the laboratory a special name: Research group on the Physics and Chemistry of Rubbing Solids (P.C.R.S.) and it became effective very quickly. Its three main areas of work in accordance with Bowden's memorandum of March 1944 were friction, lubrication and the initiation and detonation of explosives, though other themes gradually were added.

The explosives work—continued

The explosives work continued to prove exciting and fruitful. Experiments were extended to solids and both initiating and secondary explosives were studied. These investigations showed that, broadly speaking, the conclusions derived from the study of liquids could also be extended to explain the initiation of explosions in solids. The basic concept was that initiation develops from a centre of energy that must be capable of producing more energy by further decomposition than is lost by dissipative processes (e.g. thermal conduction). This was supported by experiments using a variety of methods to initiate explosion; heat, light, ionizing radiation, high-speed particles, laser illumination. The problem of spontaneous explosions was also actively studied, as well as the transition regions from initiation to burning and from burning to detonation. The problem of the propagation of explosion and low velocity detonation still exercised Bowden's mind. He was particularly challenged by the way in which weak shocks could initiate explosions in condensed systems under conditions where self-heating of the liquid or solid by the shock wave was negligible. The mechanical break-up of the liquid or solid by the shock wave was an attractive idea, which was supported, at least in part, by a study he made with McOnie of the collapse of cavities trapped in a liquid. With a new high-speed camera he was able to show that the collapse is accompanied by involution of the cavity and the formation of microjets (resembling the Munro jets observed in shaped charges). These provide another possible means of initiating explosion—

they may also play some part in erosion. Bowden was always ready to apply new techniques if their use was really justified. For example, he used electron microscopy to investigate decomposition on a microscale: in particular he found that the scanning electron microscope provided a means of studying the decompositon of crystals whilst decomposition was actually taking place (with N. Gane, P. G. Fox, & R. F. Walker).

Bowden's contribution to our understanding of explosives was widely and internationally recognized. He strongly favoured the hot-spot mechanism of initiation but his mind was receptive to other ideas. In this field of study the main workers were A. D. Yoffe (of the original Australian team), O. A. Gurton, P. Gray, H. T. Williams, J. D. Blackwood (who provided the first satisfactory explanation of the decomposition of gunpowder), K. Singh, A. McLaren, J. H. McAuslan, B. E. Evans, S. K. Deb, A. M. Yuill, M. Camp, H. M. Montagu-Pollock, J. Soria-Ruiz, M. M. Chaudhri and P. G. Fox. The outstanding figure in this work was A. D. Yoffe, a gifted physical-chemist from Melbourne whose grasp and ability in experimental research acquired increasing strength and independence. The greater part of the explosives work was written up by Bowden and Yoffe in two monographs published in 1952 and 1958. Yoffe's interests gradually moved away from the mechanism of explosion to another fundamental question: why some substances are explosive and others are not. This led him, around 1960, to study the bonding in various azides and similar compounds and this in turn soon led to an interest in solid-state physics. Today Yoffe runs, within the laboratory, a group working on the electrical and optical properties of crystalline and amorphous solids; this work has won wide recognition for its elegance and perception.

The Bowden tradition, however, continues, and a small but active group in the laboratory is still engaged in unravelling some of the unsolved problems of initiation and detonation.

The work on friction and lubrication

The frictional work continued unabated. The main workers in this field included E. D. Tingle, J. W. Menter, J. V. Sanders, E. Rabinowicz, A. J. W. Moore, J. E. Young, A. C. Moore, J. S. McFarlane, L. Young, K. V. Shooter, G. W. Rowe, K. E. Eldredge, J. A. Chapman, E. Eisner, J. R. Whitehead, W. R. Throssell, R. W. Wilson, R. F. King, M. Pascoe, J. B. P. Williamson, J. A. Greenwood, J. Harris, P. H. Thomas, R. F. Deacon, E. H. Freitag, M. Camp, H. G. Scott, J. F. Goodman, M. Seal, P. A. Persson, R. G. Lord, C. A. Brookes, A. E. Hanwell, P. J. Harbour, J. H. Greenwood, W. O. Winer, K. C. Ludema, M. Imai, P. Barnes, T. C. H. Childs and others. I was very closely involved with a major part of this work and together with Bowden wrote two monographs (Part I in 1950 and Part II in 1964) describing our main contributions to the field of friction, adhesion, lubrication and related surface studies. Some of the main lines may be worth mentioning. The new work showed that, broadly speaking, the frictional mechanism derived for metals was applicable to non-metallic solids. The two main differences were that in some cases elastic deformation of the contacting surfaces was far more important than plastic deformation; secondly, that the ploughing or deformation term could often be described in terms of elastic hysteresis losses in the ploughed surface. With single crystals marked anisotropy in deformation and friction were observed during sliding. With clean surfaces where there was strong interfacial adhesion the application of a tangential stress could lead to appreciable junction growth before gross sliding occurred. This readily caused gross seizure but a whiff of oxygen or some other reactive gas could greatly reduce junction growth and the associated tendency to seizure. Later work also showed that junction-growth is much less with solids of limited ductility. These studies proved to be of direct relevance to the problem of friction in space vehicles.

During a visit to the United States Bowden had been shown the magnetic ultra-centrifuge developed by Beams and he saw in this a very simple way of studying friction at extremely high speeds. With Freitag he developed a technique of suspending a steel ball magnetically and then spinning it at about one million revolutions per minute. The ball could then be trapped between three pads and the deceleration measured. Alternatively the ball could be dropped on to an inclined surface (Bowden & Persson) and the angle of deviation from the plane of impact determined after rebound. In both ways it proved possible to measure the friction between surfaces at sliding speeds of up to 600 m/s. The results with metals such as copper showed that at these high speeds the frictional heating produced appreciable melting of the interface and the friction and surface damage were small. The behaviour indeed resembled that of ice. As Bowden remarked—'if only one could move fast enough one might perhaps ski on copper mountains' (the Forty-first Thomas Hawksley Lecture). On the other hand, with metals such as bismuth the high rates of shear at the interface produce considerable fragmentation of the surface layers. With typical originality Bowden showed (with Lord) that this apparatus could also be used as a means of studying aerodynamic resistance under conditions which are not otherwise easily susceptible to investigation.

The lubrication studies made use of electron diffraction techniques to study the orientation of boundary layers and the effect of temperature. This work showed that the first stage was one of surface melting, the second of surface desorption: these changes in state with temperature are accompanied by progressive stages of lubrication deterioration. The action of lamellar solids as high-temperature lubricants was also investigated.

Work in the field of friction and lubrication had many practical implications and was widely appreciated by industry. One particular example may be quoted. Soon after Bowden had observed the low friction of Teflon further work in the laboratory suggested that its incorporation into a porous metal backing would provide a material combining the strength and high thermal conductivity of a metal with the low friction of Teflon. This suggestion stimulated the modern development of 'unlubricated' composite metal-polymer bearings.

There are many other examples of the applied value of Bowden's work and there is little doubt that his was the primary influence in bringing a scientific approach to bear on a whole range of tribological problems.

Among our first research students in this effort was Dr J. W. Menter who, following a study by electron diffraction of the orientation of surface films, became interested in the microstructural world opened up by other emerging electron optical techniques, particularly electron microscopy. He converted our early transmission electron microscope to image surfaces at glancing incidence and later in collaboration with P. B. Hirsch and A. Kelly of the Cavendish made a detailed study of the microstructure of cold worked gold foil by transmission electron microscopy and diffraction. This was the precursor of two significant developments outside the laboratory. Hirsch, working in the Cavendish, discovered that dislocations and other defects could be imaged by transmission electron microscopy of thin metal films through contrast effects associated with a strained lattice. Menter, who had by this time moved to the T.I. Research Laboratories at Hinxton Hall, had the happy idea of studying thin crystals of platinum phthalocyanine in a high resolution transmission electron microscope. The molecular layers some 1.2 nm apart could be directly imaged to reveal dislocations. These parallel and complementary developments appealed very much to Bowden's penchant for the direct approach to physical knowledge through experimental observation. It was natural that he should seize on to them and stimulate his staff to put them to work in his own laboratory.

Impact, erosion and fracture

Bowden's interests were always very catholic and other lines of work continuously emerged. One of these was connected with the damage to solids produced by rain drops travelling at supersonic speeds. With J. H. Brunton he developed an extremely elegant, simple and inexpensive method of projecting water droplets of reproducible shape and size against a stationary target at speeds of up to about 1000 m/s.

The first studies of impact were made with a Cranz-Schardin system of high-speed photography originally devised by Courtney-Pratt. Later, as a result of Bowden's efforts, the laboratory obtained as a loan-gift from Du Ponts, a Beckman and Whitley camera which enables twenty-five successive frames at microsecond intervals to be recorded. This opened new possibilities in the impact studies. The work showed that compressible deformation of the drop occurred and that this produced extremely high pressures at the region of impact. These pressures were able to deform the hardest materials whilst the high rates of strain tended to shift the deformation properties of the target into the more brittle range. Finally the extremely rapid radial flow following the impact could produce further erosion of the surface surrounding the central damaged region.

The next stage of the work with Brunton & J. E. Field was concerned with the role of stress-waves in the deformation of brittle solids. These could be observed by using transparent targets and polarized light. In this way it proved possible to explain the various complex failure patterns induced in the specimen in terms of the reflexion and interference of stress waves generated by the impact process itself. Recent work by Field has shown that stress waves play a much more important role in brittle failure than has generally been considered, for example, in rock blasting.

Another problem was that of repeated impact of liquid droplets at velocities insufficient to cause damage by an individual drop. This work (with Hancox and with Thomas) is of direct relevance to the drop-impingement erosion damage of turbine blades. Although the mechanism is by no means unequivocally established the evidence is that it is the result of a fatigue process. On the other hand the micro-jetting of collapsing bubbles observed by McOnie may provide another way in which damage can be produced by relatively gentle impacts.

Bowden continued to encourage and direct work on high strain-rate phenomena. With Cooper he studied the twinning-mechanism of calcite induced by pulse loading, whilst similar studies on metals were carried out with Wilson and Brunton. His most recent work was with Field on the fracture of brittle solids induced by liquid impact or explosive charges or by the tension of a notched specimen. Apart from the intrinsic contribution this work has made to our understanding of crack-propagation, crack-bifurcation and other failure mechanisms it led to a typical practical proposal. Bowden had been impressed by the high rate at which a relatively large specimen could be separated into two parts by a rapidly moving crack. He suggested that this might have applications as a rapid circuit breaking device. A preliminary investigaton with Brunton, Field, and Heyes produced very favourable and promising results.

Fracture studies remain an active part of the laboratory's researches and are being conducted by Dr J. E. Field.

Other areas of work

Bowden was very interested in the properties of matter in the finely divided state. From the point of view of their chemical stability, finely divided solids are clearly far more reactive than the bulk. For example, he showed (with Walker & Gane) that fine crystals of azides decompose at an immensely high rate. This work led Gane to study the mechanical properties of very small crystals and very small volumes of solids. With a most ingenious mechanism he was able to carry out and observe, *in situ*, in the electron microscope the micro-indentation of metals. Under suitable conditions it was found that these manifested a resistence to indentation comparable to the ideal strength of the metal.

Sometimes two different areas overlapped in a successful and felicitous way. An example of this (with Fox & Soria-Ruiz) was a beautiful, original method for determining the temperature at the tip of a crack during its propagation. A crystal of azide was subjected to controlled fracture in a high vacuum and the amount of decomposition products determined using a mass spectrometer. A parallel measurement was made of the thermal decomposition of the material as a function of temperautre. Knowing the time that it took for the crack to cross the crystal it was thus possible to estimate the effective temperature of the crack tip.

Other areas of work included further studies on the range of action of surface forces using the molecularly smooth cleavage faces of mica as the solid surfaces (with Courtney-Pratt, Anita Bailey & Susan Kay). The most recent work in this field (published after his death) demonstrated for the first time by direct measurement that normal van der Waals forces dominate where the surfaces are less than 15 nm apart, retarded van der Waals forces when the separation exceeds 20 nm (Tabor & R. S. H. Winterton). Another field dealt with the mechanical and structural properties of high temperature solids such as the carbides and borides of the transition metals (L. M. Fitzgerald, C. A. Brookes, A. G. Atkins, M. J. Murray, M. E. Packer). The laboratory is still actively engaged in the growth of single crystals of these materials and in the study of their properties. Two other areas may be mentioned, radiation damage (with L. T. Chadderton, D. Van Vliet, D. V. Morgan, I. McC. Torrens) and laser damage of solids (with T. J. Bastow).

Contacts with industry

Bowden had always shown a keen interest in co-operating with industry. On the one hand the problems of industry would often suggest fruitful lines of basic research in fields that were relevant to his own interests. On the other he felt that the fundamental investigations generated within the laboratory itself could in some cases have practical applications. Some typical achievements in this direction have already been mentioned, for example the use of Teflon in dry polymer-metal composite bearing materials: the application of brittle fracture to electrical contact-breaking devices. He also felt that he might be able to influence industry in its approach to its own research problems.

Bowden's involvement with industry provided an additional outlet for his energies and ambitions. He had little time for University politics in the broader sense and little interest in the more formal aspects of University teaching or Departmental duties. His main concern was research. The University provided a congenial centre, whilst industry offered a further field for research and action. In addition there was the personal satisfaction of being associated in a fairly direct way with the world of affairs. All these interrelated factors found fulfilment, in greater or lesser measure, in his association with T.I. and English Electric.

T.I. and the laboratory at Hinxton

Late in 1953 Ivan Stedeford, Chairman of Tube Investments Ltd, who happened to be a member of the Council of the Department of Scientific and Industrial Research, told B. Lockspeiser, then Secretary of DSIR, that his company was thinking of establishing new central research facilities and would like the advice of a distinguished scientist with an interest in the application of science to industrial problems. On Lockspeiser's recommendation Bowden was appointed as consultant by Stedeford.

The next step was to find a suitable site and to set up a research facility. Bowden's right-hand man in the initial stages of this work was Courtney-Pratt who brought his customary indefatigable zeal to the task. Early in 1954 a site for the Tube Investments Research Laboratories was chosen at Hinxton Hall, a country estate, ten miles from Cambridge, and by the end of the year the first scientists were installed in the converted mansion. Very soon plans were made for the erection of modern premises in the grounds. This early stage was one of euphoria within the T.I. Laboratory and the elation and enthusiasm spilled over into the Cambridge Laboratory. We were all, somehow, involved. For this and for other reasons the break which occurred later between Bowden and Courtney-Pratt was a wretched period for those of us close to the events. It led to Courtney-Pratt's resignation and ultimate migration to the United States. Today he holds a distinguished position in the Bell Telephone Laboratories in New Jersey.

As the laboratory at Hinxton took shape Bowden began to impose on it the pattern he had in mind. He saw in it the opportunity for breaking new ground by experimenting in Britain with an approach to industrial research similar to that of some of the large American corporations like General Electric at Schenectady and Bell Telephone Laboratories at Murray Hill, which he much admired. He encouraged a number of active young scientists working in areas of science of potential commercial interest to Tube Investments to come to Hinxton Hall with a freedom to choose their own research problem not unlike that found in university laboratories. This approach, aided and stimulated by frequent interchange of views, discussions, etc., with colleagues in various departments of the university established a very lively scientific atmosphere. The output of published papers was remarkable, over two hundred and fifty in the first six years, including several in the *Proceedings of the Royal Society*. Later two members of the laboratory (J. W. Menter and D. W. Pashley) were elected to Fellowship.

Notable advances were made in the understanding of the exceptional strength properties of whisker crystals and brittle materials generally (J. E. Gordon), in the beneficial effects of electron irradiation on polymers (A. Charlesby), in the study of the microstructure of crystals by electron-microscopy (J. W. Menter and D. W. Pashley).

The design and manufacture within the laboratory of the first fully engineered electron probe X-ray scanning microanalyser by P. Duncumb and D. A. Melford was a particularly striking example of the fruit of the close interaction of industry and university which Bowden envisaged from the beginning. The need to solve an urgent problem of steel quality in one of T.I.'s steel works took Melford to the Cavendish Laboratory in search of a new technique for point-by-point non-destructive chemical analysis of surfaces. Duncumb's experimental apparatus provided the answer and led on to the design of a major new metallurgical research instrument which was subsequently licensed for manufacture and sale by the Cambridge Instrument Company.

During the period of build-up the laboratory operated under Bowden's personal guidance. It became clear, however, that with a staff of well over one hundred, including thirty to forty research scientists, a full-time director was required, particularly to coordinate the work of the laboratory with the development, manufacturing and commercial aspects of Tube Investments. T. P. Hughes, a former colleague of Bowden's in the pre-war Cambridge days, came from the Rocket Propulsion Establishment at Westcott to take charge. Thereafter Bowden continued to be intensely interested in the work of the laboratory, making frequent visits to hear about the latest experimental results and giving stimulating suggestions for new lines of enquiry.

Bowden always saw the laboratory in a dual role as contributing to the pool of scientific knowledge and to the commercial needs and benefit of T.I. because he believed the one fed the other. Furthermore, as a pragmatist he held that the simultaneous pursuit of these objectives was the only way of encouraging more of the better scientists to work in industry. This reciprocal relationship between the industrial and research side gradually developed and proved successful. Some of the research lines, exciting in themselves but not of close interest to T.I., were gradually dropped: research men moved from the T.I. laboratories into industrial management. Gradually the confidence of the practical men, the technologists and the developers was won. The future of the laboratory was now secure.

The research needs of the company became more clearly defined. Under the leadership of Hughes, his successor J. W. Menter, and more recently D. W. Pashley, a more stable balance between applied and pure research has been achieved with somewhat more emphasis being placed on the applied side. Nevertheless, the duality of purpose built into the ethos of Hinxton Hall at the outset remains to this day. Although the stabler form of the laboratory may not be what Bowden had originally envisaged he gradually adjusted himself to the newer pattern. It is characteristic of his flexibility of outlook that his own views on what was desirable for industrial research within the U.K. changed as a result of his increasing involvement with industrial affairs and his growing appreciation of the immense complexity of the industrial environment.

He took particular pleasure in two things. The laboratory survived its early vicissitudes and is firmly established as an essential element of the Tube Investments organization. Secondly, he had provided a cadre of staff from his Cambridge laboratory and they had played a vital part in the development of Hinxton Hall. In addition he was delighted and highly gratified at the fact that one of his former research men, Dr J. W. Menter, went on to become Research Director of Tube Investments Ltd.

The English Electric Company

Bowden's experience with T.I. gave him a taste for applied research, particularly with that deriving from science-based industries. Through a number of contacts he was approached by the English Electric Company and in 1958 was elected a Director. His main task was to study the research activities of the firm and to represent its forward needs and research function at Board level.

When he joined the Company the various groups had their own research establishments, and often worked in isolation from one another. His first major recommendation was the establishment of a Research Council comprising laboratory directors and senior executives from headquarters and the operating units. This was to play a vital role not only in the co-ordination of a number of large, diverse laboratories, but also in increasing the respect held for research throughout the Company. He took over the chairmanship of the Council in 1962, and was its driving force until his death.

Under Bowden's guidance a distinguished scientist, Dr Eric Eastwood, was appointed as Director of Research, and a comprehensive research organization was evolved, combining the advantages of central co-ordination and direction with those of involvement of the operating units. He was steadfast in his advocacy of a strong, central activity as the best method of achieving excellence in industrial research and of assuring its continuity in times of financial stress. On the other hand, he fully recognized and actively encouraged concentration of the research effort into the areas of greatest potential commercial significance. Thus he commanded support from scientists and managers alike.

A notable feature of the outlook he brought to industry from university research was the importance of identifying gaps in the research programme. He saw this as being far more rewarding and worthwhile than seeking directly to identify and correct real or apparent duplication.

Bowden recognized the danger of allowing the research activities to operate remotely from the development and manufacturing groups. It was not so much that he feared the ivory tower complex in the scientists as the generation of suspicion in the minds of the factory teams that the research people were being accorded privileged treatment and did not have to bear any of the 'toil and heat of the day'. He did not formulate any special philosophy as to the nature of industrial research. In his own words 'he was not that sort of man'; he was essentially a pragmatist and empiricist with a wonderful flair for realizing what was possible. As Dr Eastwood has remarked: 'We were content to derive solid advantage from the calm good sense and excellent advice which he was always ready to give. Above all, he brought to English Electric the great personal quality, both as a scientist and man, of complete integrity.'

Governmental committees

Apart from his numerous commitments in industrial affairs Bowden also played an active part on a number of committees of the Scientific Research Council, as well as on several Ministry Research Boards. He was Visitor to the Electrical Research Association (1963) and a member of the Advisory Council on Scientific and Technical Development (1964-67). Perhaps his most responsible and certainly his most long-termed involvement was as Chairman of the Executive Committee of the National Physical Laboratory (1955-62). This was a period of transition in the life of N.P.L. On the one hand it faced criticism from University personnel, many of whom felt that government laboratories were too well off and were in a relatively privileged position. On the other hand it faced increasing pressure from the Government to accept the pattern of control imposed on other parts of the Scientific Civil Service. Bowden showed great fairness in understanding the needs of the N.P.L. and when he was convinced that a certain course was justified would support it with firmness and determination. The N.P.L. regarded him as a most helpful and sympathetic Chairman.

Cambridge. The last decade

Bowden had been made a Reader in Physical Chemistry in 1946. Ten years later when the Physical Chemistry Department began its move into new accommodation in Lensfield Gardens, Bowden changed his affiliations. He became a Reader in Physics (1957). His laboratory became part of the Cavendish, acquired the shortened title of Physics and Chemistry of Solids (affectionately known as P.C.S.) and took over, in part, the former premises of the Physical Chemistry Department in Free School Lane. Geographically, Bowden was back, more or less, where he had started as a research student thirty years before. The laboratory which was given the status of a sub-department was happy with the new arrangement and there were especially fruitful contacts with the Electron Microscopy and Metal Physics groups at the Cavendish. In 1966 he was appointed to a personal chair (*ad hominem*) in 'Surface Physics', an appointment which gave him great pleasure and the laboratory took on its final, present name.

This was a period of intense activity and fruitful work. Bowden was still busy and creative in the Cambridge laboratory, involved with T.I., initiating new policies in English Electric, accepting responsibility on governmental and Ministry committees. In the midst of all this he also managed to find time to serve as President of the Cambridge Philosophical Society (1957) and as President of the Cambridge Alpine Club (1965). He also served as Vice-President of the Faraday Society (1953-56). There was no flagging of energy—no drying up of original ideas in research.

Awards and Honours

Bowden was made a Fellow of the Royal Society in 1948 and awarded the Rumford Medal in 1956. He received the Redwood Medal of the Institute of Petroleum in 1953, the Elliott Cresson Medal of the Franklin Institute in 1955 and the Medal of the Société Française de Métallurgie in 1957. In 1954 he delivered the Hawksley Lecture of the Institution of Mechanical Engineers and in 1967 the Kelvin Lecture of the Institution of Electrical Engineers. In 1968 he was awarded the Glazebrook Medal and Prize of the Institute of Physics and the Physical Society and in the same year the Bernard Lewis Gold Medal of the Combustion Institute. He received the C.B.E. in 1956.

Bowden: the scientist and the man—a summary

Bowden's researches were characterized by simplicity and elegance. His approach was direct, his conclusions clear and uncomplicated. He was recognized as an experimental scientist of great originality and in almost every field that he touched he provided some germinal idea of value and importance. He had no shortage of ideas. He made original contributions in the fields of electrochemistry, surface studies including friction and lubrication, the mode of action of explosives and the deformation of solids at high rates of strain. It is significant that during the post-war period he organized three Royal Society Discussion Meetings, one on 'Friction' in 1951, the second on 'The initiation and growth of explosion in solids' in 1957 and the third on 'Deformation of solids by impact of liquids' in 1966. Although his approach was highly individual he was able to establish a research school not once but thrice: at Melbourne in the Division of Tribophysics; at Hinxton in the T.I. Laboratories; and at Cambridge in Surface Physics, which to his great pride had achieved the status of a sub-department of the Cavendish Laboratory.

In the Cambridge laboratory he was a marvellous head to work under and with, and this was reflected in the atmosphere: it was not only a stimulating place to work in, it was also an extremely happy laboratory. In addition he had the knack of enabling his students, almost without exception, to become successful research workers: often they seemed to be able to do better work within his laboratory than they would have done elsewhere. This was largely because (as he wrote about Rideal) he communicated his conviction that all problems were soluble and that it would be great fun solving them. He recognized two approaches—theoretical analysis for which he had little stomach and the direct experimental approach which he greatly favoured, the more direct the better. He laid great emphasis on physical reality. Indeed the most devastating criticism he could make of a piece of research was to say that 'It was, physically, not realistic'. Members of the laboratory soon learned to distinguish between the ingenious indirect deductive approach and the Bowden-type experiment. He conveyed his enthusiasm for this method of research to his team and they responded.

Quite apart from his gifts as a scientific leader Bowden showed a great personal interest in the well-being of his staff and everyone held him in affection as well as respect. He won the loyalty of all the members of his group who turned to him for advice on many issues. With a few well-chosen words, sometimes with a witty aside, he could often solve some tricky staff problem or suggest a way out of some research difficulty. Probably his most individual contribution was his success in forming a research group within a University Physics Department with a positive attitude towards industrial problems, a willingness to help industry, an eye for the interesting fundamental issues within the practical problem and a feeling for applying fundamental work to practical affairs. This gave an individual flavour to the work of his laboratory. In some ways the Cambridge group may well be considered as a pioneer type of university laboratory devoted to applied science and in this sense is well ahead of its time.

Bowden was not successful as a lecturer to undergraduates—probably his heart was not in formal teaching—but as a lecturer on a research topic he was superb, whether addressing schoolboys at some Christmas function, the audience of a learned society or an assembly of international scientists.

There were certain contrasts in his character. He was shy and reserved and, on the deeper issues reticent to a remarkable degree; he was tough and ambitious; yet he was a warm and affectionate and human person. He was in many ways 'down to earth'; yet he had a breadth and spaciousness about him. He always had time. This earthiness and breadth had their intellectual counterparts. He was both shrewd and wise in his judgement of men and affairs. As a result his advice and guidance were in great demand in Ministry circles, on Government committees, on the councils of learned bodies, in College affairs, and of course in industry.

He greatly enjoyed his contacts with industry and with the broader world of affairs, as well as the rewards that these contacts provided. But professionally his real interest throughout his life was his laboratory and the challenge and excitement of scientific research. One might say he 'raised research above his chief joy' and it was this, amidst the distractions and temptations of the outside world, that preserved his integrity as a scientist. Or maybe this was just part of his integrity as a man.

Bowden's marriage was extremely happy and harmonious. He had three sons and one daughter and greatly enjoyed family holidays, with friends, and with his children's friends, climbing, walking and ski-ing in the Alps. He took immense pleasure in the increasing size of his family circle as each of his children married. He enjoyed company and played the host with great charm and vivacity, conversing with equal individuality on science, art, literature or the theatre—and as often as not on the joys of mountaineering and ski-ing.

Bowden was a man of complex character. He was also a man of very many gifts. As one who worked with him for over thirty years and whose debt to him is beyond words, I cannot do better than conclude with some quotations from the address I gave at the Memorial Service held in Caius College Chapel on 20 October 1968.

> 'One of the qualities that most completely describes so much of Philip Bowden's many-sided personality was his keen appreciation of the aesthetic side of life. It was apparent in his speech, in his subtle and discerning taste in literature and art, his love of ballet, his palate for wine, even in those gay firework displays at Christmas which exhibited not the slightest trace of vulgarity. It was part of his style. Those of us who worked with him felt the influence of this quality in his scientific research. The choice of subject, the elegant original and direct experimental approach, the forthright language, the uncluttered conclusions—all these reflect his aesthetic sense. His best scientific work is at once art and science and bridges many cultures in an effortless and unselfconscious manner . . . I shall greatly miss him as a scientist and as a man. All of us who knew him, in one capacity or another, will miss his wisdom, his charm and his humanity.'

Apart from some war-time documents Bowden left no biographical notes with the Royal Society. I am indebted to his brother John G. Bowden of Sandy Bay, Tasmania, for the early biographical details, to Dr J. N. Agar for comments on the electrochemical research, C. P. Snow (Lord Snow) for his notes on the early Cambridge days, Sir Frederick White and Sir George Currie for details concerning the first few months preceding the establishment of the Melbourne laboratory, to Dr J. W. Menter for his review of Bowden's association with T.I. and to Dr Eric Eastwood for comments on his role in English Electric. I am also grateful to some of the more senior members, past and present, of the Cambridge laboratory for various suggestions or reminiscences. I would especially like to thank Dr J. N. Agar, Dr J. W. Menter and Dr A. D. Yoffe for some critical and helpful comments on the final manuscript.

The photograph is from a three-quarter length portrait taken by Edward Leigh, F.I.I.P., F.R.P.S., in 1960.

D. TABOR

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