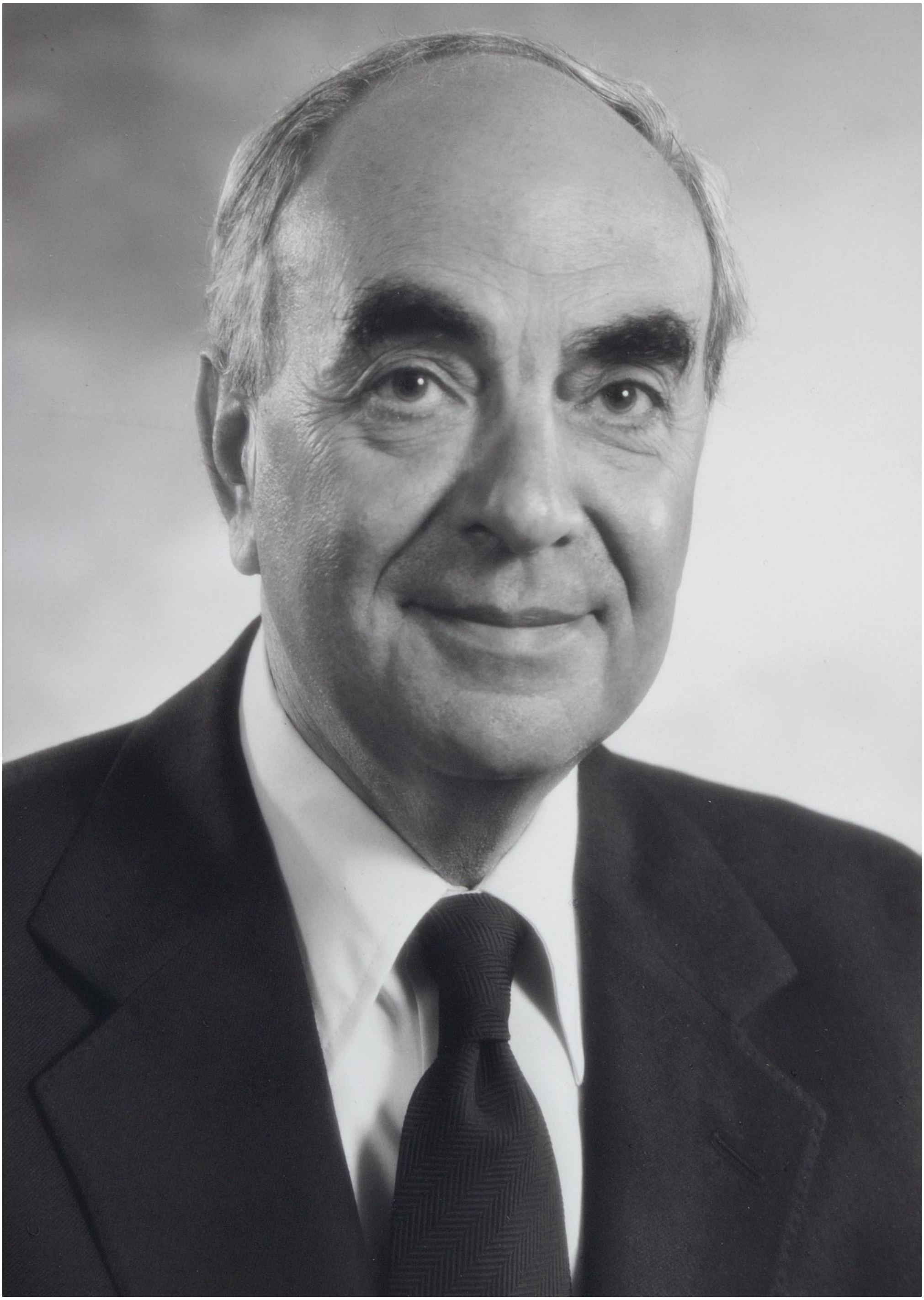
DAVID TABOR

23 October 1913 — 26 November 2005

Biogr. Mems Fell. R. Soc. **54**, 425–459 (2008)



David Tabor.

DAVID TABOR

23 October 1913 — 26 November 2005

Elected FRS 1963

BY JOHN FIELD OBE FRS

Physics and Chemistry of Solids Group, Cavendish Laboratory,
J. J. Thomson Avenue, Cambridge CB3 0HE, UK

David Tabor died in Cambridge on 26 November 2005. At the time of his death he was Emeritus Professor of Physics and an Emeritus Fellow of Gonville and Caius College, Cambridge. He was a humane and gentle man, yet despite his modesty he was a formidable and respected scientist. With Philip Bowden (FRS 1948), he laid the foundation for understanding friction and lubrication and the way in which contacting surfaces interact. Both Bowden and Tabor had great physical insight and were empiricists who favoured the most direct and simple approach to problems. They built up a research group that was interdisciplinary and motivated to basic research but with a flair to see the practical advantages of their research to industry. As individuals they would both have achieved much, but the way in which they integrated produced an exceptional 'team'. There are many examples of the benefits of their collaboration. Their research started in Cambridge; during the World War II period they ran a laboratory in Melbourne, Australia, with both returning to Cambridge after the war. Initially, they were part of the Chemistry Department but left Physical Chemistry to become part of the Department of Physics in 1957, when Nevill Mott FRS was Head of Department. After Bowden's death in 1968, Tabor became Head of the Physics and Chemistry of Solids (PCS) Group as it was called, and remained Head until he retired in 1981. Tabor continued as an Emeritus Professor and researcher. In 1992, a new group (Polymers and Colloids) was founded and worked in the 'Tabor Laboratory'. A visiting Professorship at Imperial College, London, continued and papers were produced up to 1998. He was, indeed, a scientist of great originality and versatility.

doi:10.1098/rsbm.2007.0031

Background family history and education (Michael Tabor and Daniel Tabor)

(This account is based on tape recordings made in 1983 and 1985 by Daniel Tabor with additional information from Tabor's written reminiscences and a family biography written by his brother Henry.)

David Tabor was born David Tabrisky to Charles and Rebecca Tabrisky in Notting Hill Gate, London, on 23 October 1913.

David's father was born Ezekiel Tabrisky in 1871 in Smorgon, Russia, and was one of eight brothers. Although of a scholarly family, poverty meant that the children were not able to receive much of an education. At the age of 13 years, Ezekiel started work as an apprentice metal worker. When he was 21 years old he went into the Imperial Russian army and became an armoury officer (non-commissioned) at the military barracks at Kazlov near Tambov. This was an unusual achievement because the official anti-Semitism of the time barred Jews from holding any positions of rank. According to family history, a display of weapons in the form of a Russian Eagle so impressed the Czar's uncle during an inspection in 1896 that Ezekiel was summoned to meet him. However, the Prince, immediately deducing that he was a Jew, told his commanding officer (a Colonel Gororev) that if this man wished to stay in the Russian army he would have to join the Russian Orthodox Church. This Ezekiel declined to do. After a brief spell working on the railways, he set up a private gunsmith and metalworking business in Kazlov. His business was rather successful because the officers from his former regiment continued to use his services.

With the help of a testimonial from Colonel Gororev he obtained an exit visa in 1904 enabling him to travel to England, where one of his brothers already lived. He set up a small metalworking business (specializing in customized fittings and designs) and after four years had made enough money to send for his wife, Rebecca, and their three young children, who had been born in Russia before 1904. At some point in the early 1920s he applied for and received British citizenship and anglicized his name to Tabor, and became known as Charles Tabor.

David's mother was born Rebecca Weinstein in Vilna in 1877. Although her grandfather had been a successful merchant, her father considered work beneath his dignity and devoted his time to scholarly pursuits, supported by handouts from his father. As a result, Rebecca grew up in considerable poverty. Rebecca had an uncle by marriage who had become a successful civil servant and had a permit to live in St Petersburg (Jews were not allowed to live there without one). All three of his sons had died in an influenza epidemic and, desiring to have another child in their house, he and his wife arranged for Rebecca to live with them. In later life, Rebecca recalled her time in Petersburg (from 7 to 15 years of age) as the happiest of her life. She had a governess (a Sophia Davidovna), learned Russian as well as some French and Hebrew, enjoyed reading Russian literature and had a circle of Russian friends. This exposure to sophisticated living set the standard for the rest of her life. As a result of permit problems, she had to leave St Petersburg and return to the miserable circumstances of her parents' home in Vilna. At age 21 years she married Ezekiel Tabrisky.

Childhood and early school years

David's earliest recollections begin when he started attending the Portobello Road Primary School at the age of five years. Overall, he recalls those days as being happy and that he enjoyed learning.

At the age of seven or eight years he went to the upper school (at Portobello Road). In March 1923 (aged 10 years) he was awarded the school prize for being the top boy. The prize was Rider Haggard's *King Solomon's mines*, which suggests that he had become quite a sophisticated reader by that age. At about the age of 11 years he got into the Regents Street Polytechnic, where his older brother John was already a pupil.

David remembered that as a boy he enjoyed collecting magnifying glasses and liked making models of things 'that worked', such as a cardboard house with a door that opened. He believed that this early predilection for the simple and practical influenced his approach to research in later life.

Despite their very limited means, his parents spared no effort in bettering the education of their children. In addition to private Hebrew lessons (described below), music lessons were arranged. David recalled having violin lessons but after a while his teacher, formerly of the Russian Conservatory, told David's parents that their son was not destined to be a violinist and that it was not worth continuing with the lessons. (However, his teacher's son became a successful concert violinist so perhaps he set rather high standards for his other pupils.)

At the age of 13 years, David was struck down with osteomyelitis. This resulted in his having to spend almost a year in hospital. In an age before antibiotics and anaesthetics, he was lucky to survive. Major surgery saved his leg but resulted in arresting its growth with the consequence that, as he continued to grow, one leg ended up slightly shorter than the other. This necessitated the use of a surgical boot and walking stick for the rest of his life. Nonetheless, he overcame his disability with a characteristic grace and quiet determination, and he enjoyed physical activities such as hiking, swimming and even the occasional game of tennis.

Family life

The seven children divided into two groups: the three older children, Simon, Bessie and Alfred, who had been born in Russia, and the four younger children, Esther, John, David and Henry, who were all born in England. David, John and Esther, with whom David had his closest attachment, formed a tightly knit group, whereas Henry, the youngest, was a little too young to join in their games (which included desert island adventures under the dinner table fuelled by an exotic beverage termed rumba made of desiccated coconut and lemonade). The eldest, Simon, who helped run the family business, was 13 years older than David and already living away from home, as did Alfred, who was generally considered to be the 'cranky' member of the family. Bessie, the eldest daughter, played the role of deputy mother and was a much-loved figure in the family.

Family life followed a pattern typical of so many immigrant families of that (and every) era. The central figure was their formidable mother, who oversaw, and apparently made all the decisions about, virtually every aspect of family life and, in particular, the education of the children. Their father worked six days a week to support his wife and children. He would leave home early in the morning and return in the evening in time for dinner, and was usually too tired to spend much time with his children. The exception to this was on Sundays, when he would tell them stories, of his own fabrication, which would often run on for many episodes; and sometimes he would take them on family outings and picnics to such places as Wembley and Richmond. Despite the limited contact with their father, all his children regarded him with the greatest affection.

The language around the house was a bilingual mix of English and Yiddish, and his parents would reserve Russian for use as a secret language (which spurred David into learning Russian). His mother spoke better English than his father, or—at least in David's opinion—

was less self-conscious about her accent, and she would be the parent to visit the schools and interview the teachers (the word 'interview' here being well chosen).

David's father had two brothers also living in London, but he had no recollection of meeting them. However, one brother (who was euphemistically described as having 'disappeared'; in other words he had left his family) had a wife called Sonia. David recalled his Aunt Sonia as a well-educated, strong-willed woman who had an opinion on all matters great and small. She had several children (all first cousins to David). One of them, who affected an exaggerated English heritage, styled himself Denis Charques and became a successful literary critic and minor author (including a well-regarded *A short history of Russia*). As far as is known, David's mother had no relatives in England; however, members of her family did emigrate to the USA, and subsequent generations, still bearing her family name of Weinstein, have met with much professional success.

Jewish education and community life

David grew up in a tightly knit community of Eastern European Jewish emigrants. Jewish tradition dominated their family and social lives. Throughout his life David felt himself to be the proud and unconflicted product of two cultures: Jewish and English. He also noted that Jewish family life in his community was much more about tradition than about religious practice, although as a young man he became quite devout in his observances.

The community was composed mainly of Lithuanian Jews, and each different national group felt superior to the others. Most of the immigrants were of limited education and their first language was Yiddish, although some, like David's parents, spoke Russian. They were mainly working-class people who were shopkeepers and tradesmen, although some in their past lives had been scholars and now performed menial jobs to support their families. A few, like David's father, were craftsmen such as metal workers and watchsmiths. There was also something of an ideological divide in the community between those whose activities centred on the life of the local synagogue (Notting Hill Synagogue in Kensington Park Road) and those with left-wing leanings who formed a Jewish Workers Circle (known at that time as a Friendly Society) and who were interested in social and political issues of the day and helping Jewish families in need. However, the community as a whole was, and felt itself to be, separate from the Anglo-Jewish establishment. David did not become acquainted with this group until he was a student.

After World War I, the children all had Hebrew lessons—which tended to have a secular, as opposed to a religious, flavour. The first Hebrew lessons were given by two Palestinian Jews who had served in the Jewish Brigade in World War I and had settled in England after the war. David particularly recalls one of the teachers, a Mr Razili. After a few years, Mr Razili returned to Palestine (and subsequently became a senior figure in the national bus company Egged) and a new teacher had to be found. This was a cause of a family conflict because his strong-willed Aunt Sonia felt that the job should go to a relative of hers. However, the family chose Israel Herman. He was a considerable scholar (although he supported his family with a small cigarette-rolling business) who had a strong influence on David's interest in Hebrew and Hebrew literature. His son, Abe, was one of David's closest childhood friends. In later life, as Abraham Harman, he became Israeli ambassador to the USA (1960–67) and later President of the Hebrew University of Jerusalem. Other close childhood friends included Sydney Black, the son of the local grocer, who became a Rabbi, and Nathan Goldenberg, who became a food chemist and a director of Marks & Spencers.

In his teens David became quite religious, and also very interested in Zionism—which in those days was an idealistic cause untainted by the political slander from both the right and left of more modern times. In 1930, he and some of his teenage friends started a Young Zionist Society, which he describes as being ‘very serious’, as manifested by the exclusion of girls, whom he and his friends thought were too frivolous for such a serious undertaking!

He was now an avid reader. Particular influences on him at this time were the two-volume history of Zionism by Nacham Sokoloff (whose funeral he attended in 1936; David, as part of an old Jewish tradition, sat with the corpse before the funeral) and the *Ethics of the Fathers*, a work that, in his mind, epitomized the religious humanism of Judaism that he embraced for the rest of his life. He also read the works of Ahad Ha’am in a translation by the eminent Anglo-Jewish scholar and senior civil servant (Sir) Leon Simon. The dominant figure in the Anglo-Jewish community was the long-serving Chief Rabbi Joseph Hertz. He was a fine scholar and charismatic individual, whom David admired greatly.

Student years in London

David won a scholarship to what is now Imperial College, London, where he studied physics. He recalls attending lectures by G. C. Thompson and by Dr Archer, Dr Mann, Dr Moon and Dr Dingle. The latter taught spectroscopy without quantum mechanics. This lack of instruction in the new physics was a source of regret in later years. He also recalls mathematics lectures from a Professor Levy, who was assisted by Bertha Swirles (who became the wife of Harold Jeffreys FRS). He graduated with the top first of his year and stayed on at Imperial College to work with G. C. Thompson on some problems in electron diffraction. However, he recalled that when he expressed a desire to pursue further studies in mathematics, Thompson discouraged him from doing so. He described this as one of the worst pieces of academic advice he ever received. His friends at the time included Richard Beeching (later Lord Beeching, author of the eponymous railway report) and Morris Blackman, who went to work with Max Born (FRS 1939) in Germany.

The equipment used for the electron diffraction studies in those days was quite primitive. Vacuum-proof metal/glass seals had not yet been developed, and David recalled that after the apparatus was evacuated they used to paint lacquer quickly over the various seals to sustain the vacuum. He tried to study clean surfaces of Na–K alloys but found that he could see only the diffraction patterns of the oxidized surface. It was, perhaps, in the course of this work that he started to develop his interest in surface properties and the ways in which molecules are adsorbed on surfaces. His various projects also led him to try to reproduce some experimental work of Sir William Hardy* FRS related to friction. He could not do so, and Dr Moon suggested that he visit a Philip Bowden in Cambridge, who had apparently encountered the same problem. This proved to be a pivotal piece of advice. After meeting with Bowden, the latter invited him to join his group in Cambridge as a research student. He started in the late autumn (some surgery delayed the start, which was to have been earlier) and thus began the professional collaboration that lasted until Bowden’s death in 1968. Indeed, Bowden’s influence on David’s professional career was immense and, in latter years, he described Bowden as having had a remarkable knack of being able to find and ‘skim the cream off’ interesting problems and, indeed, being able to find cream where nobody else could.

* In David’s obituaries it is suggested that he considered working with Hardy. However, this has to be erroneous because Hardy died in 1933. This misconception probably arose as a result of his attempts to reproduce some of Hardy’s results.

While a student, David also taught himself Russian and could read, write and speak Russian quite fluently. He would often write letters to his mother in Russian—something that was a great source of pleasure and pride to her.

Cambridge, 1936–39

David's research with Bowden resulted in their first joint publication (1)* in 1939, which discussed the contact between surfaces and established the simple but crucial idea that the real area of contact was generally much smaller than the apparent area—a concept that was fundamental to much of their subsequent works on friction and lubrication. David received his PhD in 1939 and started work as a research scientist at a small laboratory in Cambridge—the Britannia Laboratory—that Bowden had started with support from Shell. He also recalled attending lectures by Lord Rutherford FRS and Lawrence (later Sir Lawrence) Bragg FRS, and occasionally seeing Sir Joseph John (J. J.) Thomson FRS wandering (in an apparently somewhat bemused state) around the streets of Cambridge.

Through Bowden, David had an affiliation with Gonville and Caius College but for the most part did not participate in college life. He lived in digs in Collier Road. His landlady was a Mrs Searle, whose son, Ronald Searle, found fame as a cartoonist. David always spoke of Mrs Searle in the fondest terms and how well she looked after him. (She also received his mother's seal of approval after she came down from London for an official inspection.)

Outside his research, David was deeply involved in Jewish and Zionist affairs and, like so many of his generation, debated the momentous issues and causes of the age: communism, the rise of fascism, the Spanish Civil War, and anti-Semitism. The focus of his social life was the Cambridge Union of Jewish Students (CUJS). He was the secretary, and his closest friend at the time, Aubrey Eban, was the president. (Abba Eban later went on to become a leading figure in Israeli politics, serving as Foreign Minister from 1966 to 1974.) Their circle was quite remarkable and included the economist Richard (later Lord) Kahn, who was treasurer of the CUJS, the applied mathematician Sydney Goldstein FRS, the physicists Cyril Domb (FRS 1977) and Sammy Devons (FRS 1955), and many others who became leading figures in the Anglo-Jewish community. David also recalls a friendship with the daughter of Leon Simon and the extreme innocence of relationships between young men and women of his set at that time (although he believes that their parents may have met to discuss the possibility of a match between their children). Another friend and quasi-paternal influence was Henry Dagut, who was head of Hillel House (the Jewish house at the Perse School). A dominant intellectual figure in David's circle was Herbert Loewe, the University Reader in Rabbinics.

With the outbreak of the war, the School of Oriental Studies in London evacuated to Cambridge. David enrolled in a course in modern Hebrew offered by Isadore Wartsky, and he recalls how much he learned from the course and also that as the war gathered momentum the class size dwindled until he was the only student left.

The war years in Australia

In 1939 Bowden had made a visit to the USA and returned via Australia to visit his family. This visit coincided with the outbreak of World War II. The Australian government invited Bowden to set up a research group in Melbourne (the Lubricants and Bearings Laboratory)

* Numbers in this form refer to the bibliography at the end of the text.

to work on problems related to the war effort. Bowden invited David to join him. Accepting this invitation was not an easy decision given concerns about his father's declining health, and he recalls seeking the advice of Henry Dagut, who reassured him that he would be doing the right thing by going. The long journey to Australia by ship, without military escort, went via Aden, Bombay and Singapore. He remembered travelling by third class and that he was the only passenger in his class to request kosher food. Only one other pair of passengers, a Dr and Mrs Landau, made the same request. A friendship was struck up with this couple, who turned out to be related to his younger brother's wife.

David arrived in Melbourne in early 1940 and quickly found a boarding house, where he stayed for several years. The house was near the university and the zoo. He made the decision that during his first year in Melbourne he should minimize his extracurricular activities and concentrate on his research. His constant companion that year was Joos's *Theoretical physics*, which he studied from cover to cover. Nevertheless, he would sometimes attend talks at the University Labour Club and go on the club's Sunday hikes.

After David had settled down in Melbourne, he again took up his activities in the Zionist movement, in which he played a leading role, and pursued his Jewish studies. He was a founder of Habonim (Zionist Youth Movement) in Australia and it was through this that he met Hannalena (Hanna) Stillschweig, a beautiful young refugee from Germany. She lived in Sydney; they met only occasionally and mainly exchanged correspondence. However, it was by all accounts love at first sight, and they married in 1943. Apart from lectures and discussions, Habonim activities included frequent hikes and camping trips, and David liked to claim that he became quite proficient at pitching tents, lighting campfires and tying knots.

Melbourne had a small but vibrant Jewish community that included some notable Menshevik exiles from Russia; these included a Dr Patkin, who had been the private secretary to Litvinov, and Dr Sternberg, who had briefly been Minister of Justice in Lenin's first cabinet. (Sternberg was somewhat sceptical of the Zionist movement and worked hard to persuade the Australian government to consider giving up some desolate territories as a Jewish homeland.) Among the other members of the community was a Dr Goldman, who was a polyglot and from whom David learned some Arabic and with whom he later, with his new wife Hanna, had some private Hebrew lessons. David also recalled a pleasant friendship with a Shlomo Lowe, who worked for the Jewish National Fund, and his wife.

David was fond of telling the story of the occasion when he was under suspicion of being a Japanese spy. Among the immigrant community it was common to refer to the wartime refugees as 'newcomers', and in a letter to a friend in the UK he used this phrase frequently when describing his circle of Jewish acquaintances in Australia. The letter was read by the censors, who apparently thought that this word was a code name for Japanese agents. He was interviewed at some length by the Australian Secret Service. As David liked to describe it, the agents were clearly decent men trying to do their job, yet woefully ignorant of the world and had no knowledge, let alone understanding, of the world of Zionism and Jewish interests. Eventually, of course, he was able to persuade them that he was not a threat to national security and was able to continue with his contributions to the war effort.

David's research colleagues in Melbourne included Abe Yoffe (who became a long-time colleague at Cambridge after the war), the metallurgists Alan Moore, Robert (now Sir Robert) Honeycombe (FRS 1981) and June Collins (who became Honeycombe's wife), and Jeofry (Jeof) Courtney-Pratt. David considered Courtney-Pratt one of the most gifted people he had ever worked with. The latter also had rather eccentric work habits, which included working till 4 or 5 a.m. and then sleeping until midday. For a while David tried to emulate this pattern, perhaps attracted by the runs for ice cream in the middle of the night, but found that he could not sustain it and reverted to more normal work habits.

World War II and the Australian laboratory

There is a very full account of this period written by David Tabor in his Biographical Memoir of Bowden ((32), pp. 12–17) and in Greenwood & Spink (2003). The account below is based on remarks made by Tabor at the PCS laboratory's 50th anniversary.

In the summer of 1939 Bowden went on a lecture tour of America and combined it with a trip to Australia. Almost immediately, World War II broke out and Bowden offered his services to the Australian Government to set up a group to help with the Australian war effort, particularly on problems of friction, lubrication, bearings and wear. Detailed discussions were, of course, in private, but after a gap of 50 years some of the documents have been released. One Australian industrialist who was consulted thought it a bad idea—Bowden was 'too airy fairy'; the proper place for him was back in Cambridge, and if anything useful emerged that could help the Australian war effort, no doubt it could be communicated to them through normal channels.

Fortunately, not all Australians had such a negative attitude, Bowden was appointed head of a research group as part of the Australian Council for Scientific and Industrial Research (now Commonwealth Scientific Industrial Research Organisation; CSIRO). The group was called Lubricants and Bearings, and it was housed in the newly completed Chemistry Building.

The laboratory was in a university department; it was on the university campus in Melbourne and had close connections with the physics, chemistry, metallurgy and engineering departments, all of which were friendly and cooperative—but it was not a university department. There were no formal lectures, no supervisions, no examining (this suited Bowden); it was an industrial research laboratory. This had important consequences: if Bowden wanted to study the behaviour of piston rings in an internal combustion engine, he could take on mechanical engineering graduates; if he wanted to study the action of lubricants, he could take on chemists; if he wished to help the development of aircraft bearings, he could appoint metallurgists. Such flexibility of choice would not have been easy to practise in a conventional university department.

At the end of 1939, when Tabor was still in Cambridge, he received a cable from Bowden inviting him to join his laboratory in Melbourne. It was still the period of the phoney war. Like many people in Britain at the time, Tabor thought that France, with its fortified Maginot Line, was impregnable and that Britain and France together would defeat Hitler in a couple of years. He decided to accept Bowden's offer and told his parents that he would be back in two years' time. In retrospect, he was very glad he went—it was a new country, and so many aspects of it seemed different: the flora and the fauna, and of course the people themselves.

Tabor's task was to study friction and lubrication. Bowden and Tabor observed that if clean surfaces were pressed together, there was strong adhesion; to produce sliding, you had to shear the surface layers. This was the main cause of friction. They found that if you deposited a thin film of a softer metal on one of the surfaces, the substrate supported the load but shearing occurred in the softer metal. For lead on brass, for example, the friction was decreased fivefold or tenfold. Tabor remembers going to a munitions factory where they were drawing brass shell cases and asking the operator to clean all the lubricant out of his die. He then produced a brass shell case on which he had deposited a thin film of lead, and asked him to draw it through the clean die. 'You can't do that,' said the operator, 'because if you draw metal without a lubricant you will burst the die.' Tabor told him he had authority to do so. With incredulity, the operator put the piece in the die and activated the plunger. It went through like a dream.

This practical experiment had very positive spin-offs. First, it confirmed the ideas of the mechanism of friction. Second, it influenced the choice of the type of copper–lead bearing most suitable for aircraft. Robert Honeycombe and Alan Moore played an active part in this, and the laboratory cooperated with a bearing manufacturer in producing effective bearings for the newly developing aircraft industry in Australia.

Return to Cambridge

As the war wound down and Bowden prepared to return to Cambridge, he asked David to take charge of the laboratory and also to find a more scientific and romantic name for it. David came up with the name 'tribophysics'. He asked the mathematician Professor Cherry what he thought of the new name and was told that it suggested anthropological studies. This rather appealed to David who liked the idea of having a name that nobody would really understand—thereby giving the laboratory licence to work on anything they found interesting. The laboratory went on to thrive as the Division of Tribophysics from 1948 to 1978, when its name was changed to the Division of Materials Science.

In 1946, David and Hanna took a troop ship back to England to rejoin Bowden's group in Cambridge. Among his fellow passengers was the applied mathematician George Batchelor (FRS 1957), who became the long-time head of the Department of Applied Mathematics and Theoretical Physics (DAMTP) at Cambridge. David recalled that conditions during the six-week voyage were primitive and that by the end sanitation was virtually nonexistent. One passenger was mentally deranged and after threatening other passengers with a razor was put in solitary confinement. However, by the end of the voyage the joke was that he might have been the smartest man on the ship because he had the use of his own bathroom.

At the end of the war, Bowden returned to Cambridge and became a lecturer in the Physical Chemistry Department. He brought with him some of his more experienced scientists from Australia, including Abe Yoffe, Courtney-Pratt (who died in 1995) and Alan Moore, and was soon joined by some graduates from Cambridge and other universities, namely Peter Gray (FRS 1977), Jim (now Sir James) Menter (FRS 1966) and Eric Tingle. The programme was very much a continuation and development of work in Melbourne, and it is significant that Bowden called it the Physics and Chemistry of Rubbing Solids. Papers published during that period had 'PCRS' on them, not 'PCS'.

Laboratory accommodation was very cramped. Bowden acquired the dissecting room of the old Anatomy Department, with runnels at the bottom of the walls to carry away the blood spilt in the dissections. The entrance housed the main milling machine, the lathe and the welding machine; these were manned by Roy Moss, Colin Naunton and later glassblower Arthur Stripe, and photographer Alan Peck. The technical administration was undertaken by Bertie Dean in the face of competition from the Australian laboratory, who were rather keen to recruit him. The research men had to wind their way through the workshop to get to their benches.

As a result, all the researchers and workshop technicians got to know one another rather well and fitted together as an integral unit.

Within five years or so, the laboratory was beginning to develop its own character. Alan Moore was developing taper sectioning; Brent Greenhill was studying lubrication with chlorine and sulphur-based additives; Eric Tingle was looking at the protection offered by oxide films; Ernst Rabinowicz introduced radioactive tracers to study adhesion and transfer; Jim Menter used the earlier type of electron diffraction equipment to study surface films, and later modified an existing electron microscope in reflection to product the first direct observation of dislocations; Anita Bailey and Jeof Courtney-Pratt introduced mica as a model surface; Peter Gray demonstrated the role of the vapour phase in the detonation of liquid explosives; and Abe Yoffe extended his study of explosive reactions to the structure of azides and later to the electronic properties of layered solids. In the background, there was increasing use of Jeof Courtney-Pratt's cameras to study high-speed phenomena in detonation and in mechanical erosion.

The general scene in Cambridge was also changing. At the end of World War II, Alexander (later Lord) Todd FRS was appointed as Head of Chemistry, and with outstanding financial backing was able to build a department in Lensfield Road to house all parts of chemistry: organic, inorganic, physical and theoretical. Tabor spent many afternoons planning the location of the group within the area allocated to Physical Chemistry.

Suddenly this involvement with the Chemistry Department came to a stop—because in the early 1950s the Cavendish Professor (Lawrence Bragg) retired and the new Cavendish Professor, Nevill (later Sir Nevill) Mott, was appointed. Nevill Mott was the father of solid state physics in the UK. Although a theoretician, he had close contacts with industry and had been involved in many wartime activities. Not only did he solve the problem of how photographic processes occur, but he also worked on dislocations and wrote seminal papers on fracture and indentation hardness. He understood Bowden's interests and approach. He was also a Fellow of Gonville and Caius College. These discussions led to Bowden's resigning from Physical Chemistry and being taken on by the Cavendish. Moreover, Nevill Mott gave strong support to the laboratory and encouraged its involvement in the study of the electronic properties of layered materials.

Bowden and Tabor's laboratory was the first university group that was interdisciplinary in its research, long before that word was used with its present connotation. There were problems, particularly in relation to teaching, supervisions and examining, but these were resolved. Strangely, there were few difficulties about funding, but this was largely because of Bowden's wise choice of research themes and because of the confidence that industry had in the projects.

Friction of metals (K. L. Johnson FRS)

Bowden had become interested in friction and lubrication in the decade before the war, and his work had already attracted the attention of the Department of Scientific and Industrial Research (DSIR). His basic view was quite clear: all solid surfaces are rough compared with the range of molecular forces. Placing two such surfaces in contact is like 'like turning Switzerland upside down and standing it on Austria—the area of intimate contact will be rather small'. Further, the local pressure will be high enough to deform the true contact spots plastically at the yield pressure (hardness) of the material. Thus the real area of contact, at which friction is generated, will be proportional to the compressive load. These two postulates explain the two elementary 'laws of friction': the force of friction is (i) independent of the apparent area of contact and (ii) proportional to the load. Bowden was not interested in the lubrication of bearings in which the solids were lubricated by a film thick enough to completely separate the surfaces, which he regarded as a problem in engineering fluid mechanics. His concern was with so-called boundary lubrication of the small areas of 'real' contact, where friction is generated by very thin, even monomolecular, lubricant films. It was recognized that, during sliding, local 'hot spots' could be generated.

Tabor came to Cambridge in 1936 to join Bowden's group in the Department of Chemistry as a research student and published his first papers in 1939: 'On the area of contact between surfaces' (with Bowden; (1)) and 'The influence of temperature on the stability of mineral oils' (with Bowden and L. Leben; (2)).

As described above, Tabor joined Bowden's Australian laboratory in 1940, and his research on friction and lubrication continued there until 1946 (3–10). The laboratory was also much concerned with work directly relevant to the war, for example the penetration of metal sheets by bullets and whether this could be increased by lubricating the nose of the bullet. This led to an ongoing study of high-speed impact (3). The work on high-speed events and impact was accelerated by the recruitment of Courtney-Pratt, whose skill at devising and constructing apparatus to measure high-speed events was to become legendary. His simple apparatus for measuring the speed of bullets was adapted to measure naval shell velocities to 1 part in 2000, and used to calibrate the large guns of the Australian fleet. After Bowden's group returned to Cambridge, Courtney-Pratt designed a range of high-speed cameras, which provided the wherewithal to study both liquid and solid impact and fast reaction in explosives. It remained a strong line of research in the laboratory under Tabor's leadership and was carried out by John Field. After Bowden's return to Cambridge, Tabor became head of the Melbourne laboratory, which he renamed (with Bowden's approval) the Tribophysics Laboratory, from the Greek *tribos*, meaning 'rubbing', a name that led to interesting repercussions 20 years later.

Tabor moved back to join Bowden in 1946. He set to work to refine their view that plastically deforming surface asperities were the source of metallic friction. Attempts were made to find the real area of contact between rough surfaces experimentally by measuring the electrical resistance and acoustic impedance of the conjunction. Both methods failed on the same ground: the constriction resistance of a single asperity contact is proportional to its diameter rather than its area, so that the total area depends on the unknown number of contacting asperities ((34), with K. Kendall). Crushing of a single asperity has strong similarities to indentation by a hardness indenter, which led Tabor into a basic study of the measurement of the hardness of metals, culminating in a brilliant book, *Hardness of metals* (12), recently reprinted. The hardness work showed that the indentation hardness could be related to a simple tensile test of the material. His famous formula (that, for a fully strain-hardened material of yield stress Y, the hardness is approximately $3Y$) received support from the developments in the theory of perfectly plastic solids by L. Prandtl and R. Hill (FRS 1961). Tabor showed that the same result held for strain-hardening materials if Y was taken to be the flow stress at an equivalent strain of about 0.08.

The frictional energy loss in sliding was divided into that due to the 'ploughing' of the softer surface by the asperities of the harder, referred to as the 'deformation loss', and that due to shearing at the interface, referred to as the ‘adhesion loss’, following the claim that the regions of intimate contact formed cold-welded ‘junctions’ between the surfaces. Friction arose from the force required to shear these junctions. Contaminant in the form of oxide or lubricant served to weaken the adhesion. The theory encountered a certain amount of scepticism arising from the common experience that surfaces fall apart when the load is removed. Perhaps a phrase such as ‘interfacial force’ would have been better. However, experiments with ultra-clean hard metals showed coefficients of friction greater than unity; that is, interfacial shear stresses apparently greater than the yield pressure of the material. Tabor recognized that this conclusion was not consistent with plasticity theory and suggested that the area of contact would increase with the friction force, such that the yield criterion under the combination of pressure and shear would not be violated. He called this process ‘junction growth’, which was demonstrated by experiment (17) and confirmed later by K. L. Johnson (FRS 1982) in a rigorous plasticity analysis.

Tabor became fascinated by the source of rolling friction. A classic paper by Osborne Reynolds (Reynolds 1876) attributed the energy loss in a rolling wheel or ball to slip at the interface. Tabor felt intuitively that this was wrong and pointed his finger at the deformation losses in the highly stressed contact zone, even with hard solids such as ball bearings in which the stresses are elastic. Assuming that a fixed small fraction of the stored elastic energy is dissipated by hysteresis, Tabor predicted the dependence of rolling friction on load and ball diameter, which was fully substantiated by experiment ((13), with K. R. Eldredge). The investigation was extended to plastically and viscoelastically deforming materials in a paper with Greenwood and Minshall (18). This led to a patent for highly hysteretic rubber tyre treads to reduce skidding on wet roads.

The controversy about adhesive junctions stimulated a career-long interest in adhesion between solids. Experiments in pressing a smooth, clean, hard steel ball into the surface of a softer material revealed adhesion only with indium, gold and lead when the load was removed. The failure with harder materials was attributed to the release of elastic stresses in the asperities on the surface of the ball. More sophisticated experiments were performed on cleaved mica sheets, which were smooth to molecular dimensions and adhered strongly. The section ‘Surface force measurement’ describes in more detail the use of mica to measure van der Waals surface forces.

In the course of a practical investigation into the friction of car wiper blades, a student, Alan Roberts, experimented with a sphere of smooth soft rubber in contact with plate glass. He observed that the rubber adhered to the glass and required a tensile force to remove it. A fellow student, Kevin Kendall (FRS 1993), suggested that the effect might be governed by a balance between the elastic energy of the deformed solid and the surface energy of the interface. A visit to the Engineering Department recruited K. L. Johnson to provide the required contact mechanics, resulting in the so-called JKR adhesion theory (Johnson *et al*. 1971). Characteristically, Tabor did not add his name to the paper. At the same time, Derjaguin and colleagues in Russia had published an apparently conflicting theory (Derjaguin & Toporov 1974). This gave rise to a slightly acrimonious correspondence, which Tabor eventually decided was creating ‘more heat than light’. He promptly showed that the two analyses applied to opposite ends of a spectrum governed by the ratio of the magnitude of the deformation produced by the surface forces to their range of action, now known as ‘the Tabor parameter’. This work lay comparatively dormant for 30 years until nanoscience bloomed with the appearance of instruments such as the atomic force microscope and computer-based molecular dynamics.

The questions that exercised Tabor in the 1950s and 1960s, such as the relationship between adhesion and friction, are now being revisited on the molecular scale.

Although Tabor's work concentrated on metals, no doubt because of their engineering preponderance, friction studies were conducted on other materials, namely diamond, ice and plastics.

Bowden was a keen skier and so took an interest in the friction of skis on snow and ice, where experiments could be performed in congenial surroundings. Tabor's leg injury prevented him from taking part in these pleasures, but he worked on the creep of ice, with relevance to the flow of glaciers.

Extension of the Bowden and Tabor theory of metallic friction to polymeric materials is not straightforward. Their lower elastic moduli give rise to a larger deformation loss. Their strength properties are sensitive to temperature and pressure, which means that the real area of an asperity contact is not given directly by the hardness. It remained to Archard (1957) and Tabor's former student, J. A. Greenwood (Greenwood & Williamson 1966), to demonstrate that, in a multi-asperity contact having a statistical variation in heights, the coefficient of friction is constant, independent of the law of asperity compression (elastic or plastic) and the relation between the local shear stress and pressure. Most clean polymers sliding at light loads on metals show a coefficient of friction of 0.3–0.5, except for PTFE (Teflon), whose coefficient of friction can be as low as 0.05. Like most polymers, repeated sliding produces a transfer layer to leave the polymer sliding on itself. PTFE has low self-adhesion and its linear molecular structure enables the polymer chains to align with the direction of sliding and to offer minimal resistance ((41), with C. M. Pooley).

In the early 1960s, the newly commissioned continuously cast steel rolling mills in south Wales were suffering from repeated shutdowns caused mainly by problems of friction and lubrication. A Department of Trade and Industry committee, of which Tabor was a member, was set up to investigate this under the chairmanship of H. Peter Jost. It was recognized that research into friction, lubrication and wear was an interdisciplinary activity that draws on several academic subjects: physics, chemistry, materials science and engineering. In the Jost Report (Jost 1966), it was proposed, among other things, that this area of applied science should be brought together under the name 'tribology'. After a little reluctance, the name has been adopted worldwide.

Metal machining (J. A. Williams)

Tabor had been awarded a Science Research Council (SRC) grant in 1977 to investigate the role of lubrication in metal machining. He recruited John (now Professor) Williams, who had spent the previous 18 months at the Tube Investments Research Laboratory at Hinxton Hall. Williams's immediate supervisor was Dr Nicholas Gane. The idea was to try to isolate the lubricating, as opposed to cooling, effects of a lubricant, by performing a simple planning operation within a chamber whose environment could be carefully controlled. The only preceding work of a similar nature had been conducted some years earlier by Dr Geoff Rowe at Birmingham—and in fact he and Dr Tom Childs (both previously of PCS) produced a very perceptive review article only a few months after the start of the project.

Some of the preliminary design of the rig had been completed and the large stainless steel vacuum chamber with its associated bellows, which permitted a stroke of about 300 mm, had been put out for manufacture. While they were waiting for the pieces of the rig to be completed, they instrumented a microtome to measure cutting forces and looked at the effect of crystallographic orientation on cutting. There were indeed hard and soft directions, and these could be correlated with the orientation of active slip systems.

They began the experimental programme proper in 1973 at just about the time when the then 'new' Cavendish building at west Cambridge opened, and the rig was one of the first to be installed. In 1974, Gane went to a job with CSIRO in Melbourne. At that stage Tabor took over supervision of the project. It had been expected that the project would involve modelling the cutting process, either analytically with slip-line-field techniques or with the then rather new methods of computer-aided finite elements, and that the experimental research would provide useful input data, especially on the frictional or interfacial conditions that these models needed. However, the more 'look and see' experiments they did, the less they understood. While some workpieces—ferrous materials—behaved as the books said they should, alloys of copper and aluminium behaved rather differently and with some tool materials gave an apparently improved cut in vacuum or, at least, in the absence of oxygen. They had several visitors to the laboratory including Dr (now Professor) Tony Atkins, then at Ann Arbor, and Dr Peter Oxley from the University of New South Wales, whose specialism was machining. In true PCS style, Williams tried to simplify the process to its essentials until one day Gane and Williams found they were cutting a gold specimen with an iridium cutting insert! They had also had the idea of using a transparent tool of synthetic sapphire so that they could look at the interface from the inside. These images suggested that there were several zones of contact up the tool rake face with different local contact conditions and resulting shear stresses. None of the classical continuum models of cutting could cope with this degree of complexity.

Rather than being an exercise in high-strain-rate continuum mechanics, the analysis of the results turned out to involve the surface diffusion of the active molecular species at the interface between the chip and the tool. The model that Williams and Tabor developed (43) explained the results and to some extent also some of the apparent paradoxes in the literature. In 1975, while Tabor was on sabbatical leave in Australia, he talked about the work, and in the following couple of years the laboratory had two visiting postdoctoral researchers from the Southern Hemisphere: Dr Paul Wright and Dr Derry Doyle. Wright had done his PhD in Birmingham with Edward Trent, who was a leading UK expert on machining and was a colleague of Geoff Rowe's (previously of PCS).

While Tabor was away, Williams was supervised by Dr Mike Stobbs, who did some very elegant electron microscopy of copper chips cut in air and *in vacuo* that confirmed some of their suspicions about what was happening within the zone of intense contact between chip and tool. In September 1975 Williams returned to Tube Investments at Hinxton Hall; the PCS cutting work continued with Dr Greg Horne and Dr Paul Wright, who picked up and much extended the work with transparent tools.

Williams records that Tabor's supervision was low-key but he had a wonderful knack of seeing to the heart of the matter and asking the key question, often deceptively simple, which highlighted what really might be going on.

Surface force measurement

Professor S. Tolansky FRS, of Royal Holloway College, had demonstrated that relatively large surface areas of mica could be prepared free of cleavage steps; that is, molecularly smooth. Bowden asked Courtney-Pratt to study the technique. The attraction for Bowden and Tabor was that mica produced a surface without asperities. If two mica surfaces were brought into contact, the 'real' contact area would be the 'apparent' contact area. Anita (now Professor) Bailey joined forces with Courtney-Pratt. They produced contacts between two curved sheets of mica at right angles (the so-called 'crossed' position). The sheets were silvered on the rear surfaces and multiple-beam interferometry was used to observe the contact area. Bailey and her student, Susan Kaye, then showed that by moving one of the mica sheets, the shear strength of water monolayers could be measured. In other experiments, two equally thick layers of mica were peeled apart, allowing surface energies to be measured.

An ingenious extension of the peeling experiment provided a method of deducing surface forces and the way in which they vary with separation. It was based on the shape of the peeled strips. If there were no surface forces, the shape would be that of a classical cantilever; however, because of surface forces the shape is modified. Multiple-beam interferometry enables the profile to be determined with extremely high accuracy, and from this it is possible to calculate the force across the gap.

Work on mica in the laboratory remained quiescent for several years until Richard Winterton joined the laboratory as a research student in 1965. He found that it was possible to glue the mica sheets to cylindrical glass mounts without producing wrinkles in the mica. Tabor and Winterton realized they had surfaces that were molecularly smooth, of fixed curvature and relatively rigid. This was in marked contrast to the floppy sheets that had been used in the early studies on mica and they were now able to perform experiments that had not previously been possible. In particular, they decided to make a direct measurement of the forces between two such rigid surfaces. One surface was supported on a leaf-spring and its deflection was measured as the surfaces were brought together. An instability was reached when the rate of increase of the surface forces was greater than the rate of increase of the restoring force of the leaf-spring, in which case the surfaces flicked together. A beauty of the system was that the stiffer the leaf-spring, the smaller the distance at which the flick-over occurred! By shortening the leaf-spring, the jump distance was reduced to 5 nm. By lengthening it, the jump distance reached more than 30 nm, but beyond this the system picked up vibration from the building. Nevertheless, the first definitive results were obtained, showing by direct measurement that normal van der Waals forces operate for separations of less than 10 nm and retarded forces for separations greater than about 20 nm.

This research was continued, and the technique greatly improved, by Winterton's successor—Jacob Israelachvili (FRS 1988). In particular, he exploited the vibrations that occurred in the suspension and measured, to great accuracy, the change in frequency with separation, which is a measure of the force gradient at that separation. His results are for separations of 2–100 nm. Israelachvili then modified the apparatus so that it could operate with surfaces immersed in a liquid. The jump method was replaced by a far more rigid measuring device. Soon it became possible to study the effect of ions in an aqueous medium and so provided a direct method of measuring the repulsive forces due to the electrically charged double layer. At about the same time, Jacob Klein showed that polymer chains adsorbed on the mica surface could produce entropic repulsion.

Attractive forces and repulsive forces are the essential elements of colloid stability. For this reason, the mica apparatus soon became an essential piece of equipment in laboratories concerned with surface forces and colloids. A little later it proved possible to coat the mica with monolayers that present a hydrophobic outer surface, thus increasing the range of studies available. More recently, the apparatus has been used to measure the shear forces produced in liquids when the surfaces are slid over one another, and also the relaxation processes involved. This, in turn, provides a possible approach at the molecular level to the problem of slip at the liquid/solid interface.

The rigid mica surface with its highly reflecting rear surface has become a servant of surface science.

Low-energy electron diffraction (LEED), 1963–66 (based on an account by Brian Dicker)

Bowden, Tabor and subsequent heads of PCS gave high priority to having good technical help in their mechanical and electrical workshops. For sophisticated equipment, specialist technical help was employed, as occurred with LEED. Brian Dicker had been working at the Guided Weapons Research Establishment (English Electric, Luton) as an electronics engineer. It was these skills that he was asked to employ at PCS to build and instrument a LEED apparatus to study the properties of surfaces. The project was led by Tabor with funding by the SRC.

The technique had been revived by L. H. Germer when he left Bell Labs and returned to university, as he revisited his 1926 work on nickel but now taking advantage of the latest ultra-high vacuum (UHV) technology and development of the television tube and electron optics. In the research at PCS, use was made of the electron guns from the local Cambridge firm, Pye.

The basis of the apparatus was an expensive non-magnetic stainless steel large-bore UHV system to study the back-diffraction pattern from the interaction of a low-voltage electron beam with a clean and filmed surface. As this was the first such system in the UK, Dicker and Tabor were often in demand for visits, and even had several from representatives of Bell Labs itself, who were complimentary about several innovations introduced. Dicker and Tabor also consulted for Birmingham University, who began work on a similar project about two years into their project.

The UHV system was conventional for the day. It used an ion-getter pump, with liquid-nitrogen traps to maintain cleanliness in the work chamber. The last stage of pumping used a liquid-nitrogen molecular sieve trap. The whole system could be encased in a huge electric oven (almost 1 m^3) to out-gas the system at 300 °C for several hours. Of special interest was the large-bore work chamber to house the electron gun, specimen holder and manipulator, and a concentric analysis and viewing screen. The diffraction pattern was displayed on a fluorescent screen deposited on the inside of a spherical shell of stainless steel 90 mm in diameter. Inside this at 5 mm separations were two other hemispheres of gold-plated molybdenum gauze for the energy-loss analysis. The sizes were determined by the diameter of Skefco ball bearings used for pressing the gauze. Everything was state-of-the-art, even the larger 120 mm UHV window and seal. One important feature was the ability to change the voltage of the electron beam and hence the beam wavelength, and Dicker designed a variable-voltage 50–1000 V power supply to facilitate this. However, there were snags too, because the focusing electrodes required a derived nonlinear relationship to maintain focus. Various experts in the engineering laboratories and Professor Eric (now Sir Eric) Ash (FRS 1977) at University College London were consulted over this.

This innovation proved very useful, and the data collected were examined with interest by the theoreticians in the Cavendish. It is difficult to get a good focused spot current (mA mm^2) at low voltages because of beam spreading; the choice of cathode emitter also needed attention. There had been some recent work by the RCA labs in the USA on low-work-function cathodes and the use of lanthanum hexaboride, and this was incorporated successfully.

During the development of LEED, Tabor was dedicated in his support and persistent in his questioning. His contacts were widespread and in this way Tabor and Dicker developed an understanding of the equipment's specification and requirements.

Surface physics, 1980 to present (Dr W. Allison and Professor J. E. Field)

The study of surface physics was given a boost when Tabor recruited Roy Willis to head the group working in this area. Willis had been a PhD student of Tabor's and had worked on thin-film lubrication (31, 33). After his PhD, Willis had researched at the European Space Laboratory at Noordwijk in The Netherlands. He returned to Cambridge when Tabor obtained a Royal Society award for surface studies. This grant also allowed Dr Bill Allison and Dr David Joiner to be funded. The Tabor–Willis collaboration proved particularly productive, with the development of many new techniques. When Willis took up a Professorship at Penn State University, Dr Allison took over as leader of the surface group.

The study of surfaces under controlled UHV conditions expanded enormously during the 1970s. Electron diffraction, as developed in Tabor's group, was one of the driving influences. With other techniques such as photoemission and electron energy-loss spectroscopies having an important role, the field now known as 'surface science' had finally arrived. Tabor's interests evolved and became extended through the study of surfaces in UHV. New methods were developed with low-energy electrons, with energies between 1 and 50 eV, as a probe of surface vibrations. Photon probes were also becoming more flexible through the availability of synchrotron light sources, and these added enormously to our understanding of the electronic properties of surfaces. In addition, the technique of atom surface scattering, using supersonic molecular beams with millielectronvolt energies, was introduced. As always, the laboratory was quick to promote new developments and was one of the first in the UK to undertake work in scanning tunnelling microscopy (STM) and atomic force microscopy (AFM). The latter method was closely related to Tabor's work on interatomic forces and benefited immensely from existing expertise in Cambridge.

At the same time as new developments in instrumentation, there was also a significant change in the kind of surface system that was of scientific interest. Experiments showed that surface properties varied markedly with the surface morphology. Not only did properties change with the crystallographic plane, but they also depended crucially on atomic-scale features such as vacancies, steps and kinks. The heterogeneity of surface behaviour became a key that allowed sites with special properties to be exploited so as to create new structures.

Thin films of materials could be grown by using techniques such as molecular beam epitaxy so that new surface materials could be created. For example, the production of ultra-thin magnetic films, with novel properties generated through thin-film geometry and film–substrate interactions, became an important and a long-standing research interest in the Cavendish Laboratory.

This period of rapid development led to many of the technical developments that created the field of nanoscale science. It became possible not only to observe features on a nanometre scale but also to control the methods of sample preparation that created particular surface structures. Many areas of nanotechnology now rely on the techniques developed at that time. Fundamental questions remain to be answered, and several can be traced back to Tabor's early research. For example, the dynamics of an adsorbate moving across a surface is determined by the potential-energy landscape, and the nanoscale friction by its coupling to the thermal reservoir of the substrate. The surface group continues to explore these issues with new methods and today it is possible to address these questions on a genuinely atomic scale. A recent and unique development in the group is that of atom spin-echo spectroscopy. Here, a dynamical surface scatters a beam of atoms and the motion of the surface creates a distinct signature in the scattered flux. Answers to detailed questions on the motion of adsorbates hopping from one site to the next, the extent of their interatomic interactions and the nature of the atomic friction are beginning to emerge.

Diamond research

The Diamond Research Scheme was the brainchild of Sir Ernest Oppenheimer (De Beers) and Franz (now Sir Francis) Simon FRS of Oxford, and will celebrate its 60th anniversary in 2009. Sir Francis realized that diamond is a material of great scientific interest to many branches of science. However, there was little that could be done without a supply of crystals. De Beers undertook to provide samples and to support research students at various universities in the UK. Each year there is a conference where the diamond researchers meet to discuss their latest research. Following Sir Francis, Professor Robert Ditchburn FRS, Sir William Mitchell FRS and now Professor John Field FRS have been chairmen. Philip Bowden and David Tabor were involved from the start. Tabor made a presentation on hardness at the first conference and was delighted to be complimented by Professor M. Born FRS, the Nobel laureate.

Much of the research produced by the universities scheme has been presented in three books (Berman 1965; Field 1979, 1992). In the first of these, Bowden and Tabor wrote a chapter on the friction of diamond (23); in the second, Tabor wrote the chapter on adhesion and friction (48); and in the third, Tabor and Field covered recent research on the friction of diamond (50). In addition to the friction and adhesion studies, Tabor and his student Paul Lurie and J. M. Wilson also used LEED to study diamond surfaces.

There has been considerable controversy about the mechanisms involved in the friction and polishing of diamond. Tabor took the view that even if the friction of diamond rubbing on diamond is very low, it is still important to understand the mechanisms that account for the friction. Essentially, friction involves adhesion, although on unloading the elastic energy stored in the asperities causes them to separate easily. Diamond is very anisotropic in its frictional properties. In the polishing of diamond, recent research (van Bouwelen 2003; Hird & Field 2004) has shown that the so-called 'soft' direction is accounted for by a pressure-induced transition to graphite. Tabor's presentations at the conferences were models of clarity (and humour). On one occasion, when the experimental data differed from his expectations he commented, 'since I am not an astronomer, an error of 10^8 is regarded as significant!'

The friction and creep of polycrystalline ice (D. Goodman)

David Tabor was always interested in what appeared at first sight to be very basic physics but usually turned out to have intriguing challenges of explanation. And so it was with his research into the properties of ice. His first paper on the topic was published with Raraty (15). Raraty and Tabor investigated how ice adhered to metal and polymer surfaces. With metals, a very strong bond could be made. Deformation or fracture occurred in the body of the ice when a shear stress was applied. A transition from brittle to ductile behaviour was observed as the temperature was reduced. With polymers the adhesion was lower and the failure was at the interface. Tabor always related his results to practical applications, pointing out the relevance of the results to aircraft de-icing and icebreaker design. He noted that PTFE showed particularly low adhesion with ice, a result that could be of relevance today for reducing the hazard of icing of helicopter blades in freezing rain.

His next collaboration on ice was with Paul Barnes (27, 28). With Barnes, Tabor investigated how ice deforms under an indenter at different temperatures. Above −3 °C appreciable pressure melting occurred but below this temperature the behaviour was similar to that of metals, oxides and carbides. The hardness decreased with loading time as a result of creep in the ice below the indenter. The experiments were undertaken in a cold room not much bigger than a telephone box, in a stairwell in the Cavendish and with the help of Gordon Robin, the then Director of the Scott Polar Research Institute in their much bigger cold rooms. The results were important for understanding glacial sliding and skiing.

These experiments were extended by John Walker (35). Walker prepared cylindrical polycrystalline ice specimens for compression tests between loaded platens and studied the primary, secondary and tertiary creep of the ice. Plotting the secondary creep rates as a function of stress showed that at low stresses the creep rate was proportional to the third power of the stress (known by glaciologists as Glen's flow law), but at higher stresses the exponent increased. Recrystallization was observed to increase the flow rate. Temperature dependence was observed to be an Arrhenius relationship. Walker also continued the ice friction experiments started by Barnes by sliding polycrystalline and single-crystal ice cones across different surfaces. The friction was determined by deformation in the body of the ice where ice adhesion was strong; this had important implications for glacial flow. The results of the work with Barnes and with Walker were brought together in a much-quoted paper entitled 'The friction and creep of polycrystalline ice' published in *Proceedings of the Royal Society* series A (36).

Finally, in 1973 a six-year collaboration with Dougal Goodman began. Goodman was interested in how to extend the work done by Barnes and Walker in combination with fieldwork to observe and understand the creep and fracture properties of ice in nature and to undertake theoretical work on dislocation motion and grain boundary diffusion (in collaboration with Mike Ashby (FRS 1979) and Harold Frost) (Frost *et al.* 1976). Again Gordon Robin at the Scott Polar Research Institute played an important role in helping Goodman visit Greenland and the Antarctic to observe the break-up of ice shelves and sea ice. Laboratory experiments were undertaken to measure the fracture toughness of ice (47) (later extended to the fracture toughness of single crystals of ice) and surface strain changes under natural loading from sea waves made of ice floes off Greenland and on a floating ice tongue in the Antarctic (Goodman *et al*. 1975; Wadhams *et al*. 1988). On one occasion off the Greenland coast (in a joint experiment with Peter Wadhams) just after the instruments had been set up on an ice floe it calved in half, with the crack running right through the strainmeter, providing a direct observation of ice failure caused by wave action.

Working with Ashby, Goodman combined the results of Barnes and Walker with others to construct a deformation mechanism map for ice. This showed that ice is surprisingly creep resistant when compared on a relative basis with other materials (Goodman *et al*. 1981), because dislocations moving in ice are held up by the random arrangement of hydrogen atoms. Thermal rotation of water molecules is required to avoid the formation of Bjerrum defects (two protons on a bond or no protons on a bond) when a dislocation moves. In addition, a polycrystalline aggregate does not allow slip only on the easier hexagonal planes; ice grains cannot stay together unless slip occurs on non-basal planes. As a result of the high creep resistance, the fracture toughness is much lower than in other materials because the creep zone at the head of the crack tip is small and little energy is lost as the crack propagates. Increasing the hydrostatic stress component can suppress crack propagation as under an indenter or at the base of a glacier or at depth in an ice shelf. Crevasses have a finite depth (except when filled with water) because the hydrostatic component increases with depth in a glacier.

Goodman went on to join BP, where he used the work undertaken with Tabor and the work of Barnes and Walker to estimate the design load for structures to be built off the coast of Alaska and Canada for oil production. Because of the low fracture toughness of ice, the loads turn out to be much lower than first expected (Palmer *et al*. 1983; Ponter *et al*. 1983; Sanderson 1988). There is a resurgence of interest in the work done in Tabor's group on ice as BP and other companies explore for oil and gas in Siberia and offshore Sakhalin in Russia. Tabor would have enjoyed knowing that his work on ice has been applied in a very practical way to a problem of high economic impact.

General topics

This section comments on other research topics, not discussed above, which Tabor was involved in. Papers are listed in the full online bibliography.

With J. S. McFarlane, Tabor studied the adhesion of solids and the effect of surface films. Jim Menter spent time developing electron microscopy in PCS and with Tabor researched the orientation of fatty acid and soap films on metals; this research was continued with J. V. Sanders. The frictional properties of polymers were studied with several students (K. V. Shooter, R. F. King, M. W. Pascoe, E. Wynne Williams, K. C. Ludema, S. C. Cohen, A. A. Koutkov, P. R. Billinghurst, B. J. Briscoe, C. M. Pooley, D. Allan, A. K. Pogosian and S. Bahadu). J. A. Greenwood, who was later to show, with J. B. Williamson, that the independence of friction of load could also be explained by a distribution of elastic asperities, wrote papers with Tabor on the properties of model friction junctions and hysteresis losses in rolling and sliding friction. This later research led to collaborations with the Road Research Laboratory and tyre manufacturers.

Research with a Canadian, D. Atack, on the friction of wood eventually led to new ways of pulping wood. There were also practical implications from studies of lubrication with W. O. Winer, R. F. Willis and C. R. McClune. Hardness studies at high temperatures were made with A. G. Atkins, who used mutual indentation techniques.

A fundamental study of the effect of pressure on the viscoelastic properties of polymers was made by Emyr (now Sir Emyr) Jones Parry. Research by Jones Parry, Jacob Klein and A. R. Rennie gave useful practical data on 'reptation' studies in polymers in support of theoretical studies made by Pierre-Gilles de Gennes (ForMemRS 1984) in France and Sir Sam Edwards (FRS 1966) in Manchester and Cambridge.

There were studies on fibres such as those with I. C. Roselman on the friction and wear of carbon fibres, and important physical insight was gained into the way steel fibres could reinforce cement and concrete with D. J. Pinchin.

Elegant and important research on the nano-scale using atomic force microscopy was done with M. D. Pashley and J. B. Pethica (FRS 1999; now Professor).

Papers in the late 1980s with former Cambridge colleagues, Professor Anita Bailey and Professor B. J. Briscoe, were written when Tabor had a visiting Professorship at Imperial College, London.

David Tabor and teaching

Tabor was a very good lecturer to students. Apart from short specialized courses, he covered two major topics. The first was to the first-year physicists essentially on 'Properties of matter'. Because there were typically 400–450 IA (first-year) physics students, these lectures were shared with another lecturer. For many years, this was David Shoenberg (FRS 1953), so by coincidence the two best Russian speakers on the staff shared this load! After a few years teaching this course, Tabor produced his text *Gases, liquids and solids* (30). As noted elsewhere, this was a popular book because of its clarity and the physical insight it gives. It was subsequently updated in two further editions (published by Cambridge University Press). The second course was a major option on 'Materials' in Part II, with a typical audience of 30–40 students. This was a new course and had a great deal on polymers and colloids, material which was added to his textbook. Significant parts were on topics that Tabor and his students had pioneered (such as diffusion in polymers, van der Waals force measurement, colloids, dislocations, plastic deformation, hardness and fracture). The present author was delighted to take over, and further develop, the course when Tabor retired.

David Tabor and the books on friction and lubrication (Professor E. H. Freitag)

In 1950, an unusually wide-ranging summary of research on the friction and lubrication of solids (11) was published by the Clarendon Press. Its co-authors, F. P. Bowden and David Tabor, at that time headed the PCS Laboratory in Cambridge, where most of the experimental studies described had been conducted. The book won immediate acclaim far beyond the English-speaking world from the engineers, metallurgists and physicists, among others, interested in this ancient and mundane subject. In the words of the reviewer of *Metallurgical Abstracts* it marked 'the beginning of a new epoch' in this field. The manner in which it was written reveals much about how the evidently successful symbiotic partnership between Bowden and Tabor functioned: having decided to share the task and agreed on a deadline, Tabor went ahead quickly and reached his deadline before Bowden had started. Tabor then tackled further sections of the book until, to meet the publisher's demand, Tabor completed the manuscript, leaving Bowden to write the preface. Forever after, Tabor's role in PCS remained that of its scientific conscience and scientific communicator, whereas Bowden provided the resources and the connections to industry and politics, and also frequently injected ideas for future projects. With their unflagging enthusiasm they created together a heady atmosphere that attracted research students from almost every industrialized country. Whether they were formally supervised by Tabor or not, and whatever their scientific intellect, his vast store of knowledge and his generous praise of laudable efforts were evident. When critique was called for, it was delivered not as a rebuke but rather as a nudge with a hint of how a better result might be obtained.

The weighty monograph on friction and lubrication, part I, did not dampen Tabor's zest for scientific exposition. In 1951, with the slim volume *The hardness of metals* (12) he rendered a great service to all those engineers and scientists engaged in hardness measurements, either in manufacturing industry or in research and development, because he succeeded brilliantly in explaining the results obtained in terms of physical concepts and mechanisms, depending on the method of measurement used. Even now this gem of a practical book based on a sound scientific foundation has not been superseded, and as it became increasingly difficult to get hold of a copy Tabor gave the impression that this achievement pleased him more than anything else he had written. Throughout his career he remained a prolific writer, not averse to extracting theories from experimental data. He was admired by his fellow tribologists all over the world for the clarity and precision with which he presented the PCS output at conferences and in review articles.

In 1956, Bowden and Tabor published a shorter account of their research in *The friction and lubrication of solids* (11), which remains a readable and informative account for students and researchers entering the field. Part II of *The friction and lubrication of solids* (19), again written mainly by Tabor, appeared in 1964.

A book co-authored by Tabor with H. G. Howells and K. W. Mieszkis (*Friction in textiles*) was published in 1959 (16).

The quality of his writing perhaps owes much to his interest in languages and literature. He was a discerning reader, not only in the realm of science. Thus, he recognized the greatness of the Australian novelist Patrick White long before the latter was awarded the Nobel Prize. Being at ease with texts in German and, to a lesser extent, in French he made himself familiar with the other schools of thought in his field and reacted to them with sympathy, praising openly what he found interesting, and leaving the aspects he considered questionable or wrong to be discussed in a smaller circle on a suitable occasion. No one ever was the target of a provocative intervention that could have resulted in an abrasive confrontation.

It would be wrong to see him only as a desk scientist and not also as an experimental physicist endowed with creative imagination. He was at his most cheerful when he made the pioneering measurements of rolling friction by means of the simplest possible apparatus, which he had designed for the purpose. He seemed to know what he was going to observe, and when he obtained some freak results in the early tests he shrugged them off with a chuckle. In the second and much larger volume *The friction and lubrication of solids*, part II (19), the chapter on rolling friction runs to 42 pages, and the chapter on the wear of solids, from a technological point of view one of the most important areas of tribology, to a mere 14 pages. Undoubtedly, he could have contributed more than he did to that field but he was clearly aware of the complexity of wear phenomena, and the range of topics he worked on was already quite exceptional.

Tabor's death leaves a huge gap in his chosen field of endeavour and evokes strong feelings of sorrow and gratitude in numerous former friends, pupils and colleagues.

The Tabor Laboratory (Athene Donald FRS)

In 1992, a £3 million grant was awarded to the Cavendish, as part of a major DTI–industry grant also involving Bristol University (Chemistry) and Imperial College (Chemical Engineering) plus Unilever, ICI (and subsequently Zeneca after the demerger) and Schlumberger, for a programme in colloid technology. This grant enabled a substantial amount of new equipment to be purchased, together with the hiring of many new postdocs, as well as the appointment to a five-year post of David's former student Adrian Rennie. It also provided an impetus to set up a new group within the Cavendish as a 'spin-out' from David's old group of PCS. This group became known as the Polymer and Colloids (P&C) group, and its initial head was Athene Donald.

To give this new group and space a clear identity, it was suggested by Sir Sam Edwards that it should be known as the Tabor Laboratory, in recognition of David's myriad and innovative contributions to the field of colloids and surfaces. David himself was typically humble about the idea and needed some persuasion, but he was eventually convinced to lend his name to the area. A formal opening was held in November 1992, at which a plaque was unveiled by the then minister Edward Leigh MP in the presence of the Vice Chancellor, Professor David (now Sir David) Williams, as well as Tabor himself.

Tabor took an enormous amount of interest in the activities of the new group. He used to visit regularly to discuss how things were progressing and attended seminars within the group right up to the time he stopped coming into the Cavendish. He used to sit at the front and look full of innocence until it came to question time, when he would regularly ask extremely penetrating questions, sometimes to the consternation of the speaker. It was obviously a pleasure to him to see this strand of his work and interest in colloid science flourishing at the Cavendish. It was equally a pleasure to the P&C group members to have David among us, with his experience and wisdom always available.

Tabor at Gonville and Caius College (Sir Sam Edwards FRS)

Bowden, a Fellow of Gonville and Caius College, introduced Tabor to the college. Neither held a major college office because they were so involved with their laboratory, but they brought many visitors from around the world to partake of the excellent table kept by the college. Caius has a rule that after 20 years as a Fellow you become a Life Fellow. The Life Fellow physicists traditionally share room L1. A few years ago the list of names (including Mott, Shoenberg, Tabor and Edwards) was shown as a page in a Cambridge calendar.

I found that Tabor could speak fluent Russian and keep up with Professor David Shoenberg, another member of L1 whose native language was Russian. I recall Rudolf Peierls FRS visiting, whose wife was Russian and could speak fluently, and they left me completely behind because my other language is French and they had no use for that.

Tabor's wife, Hanna, was an accomplished artist and L1 had one of her paintings as a centrepiece, which she changed periodically.

His entry in *Who's Who* has as his recreation Judaica. This was a non-trivial entry. He cherished his Jewishness and believed in the power of prayer.

Tabor's ideas in physics were always both fundamental and simple, and he made massive contributions to studies of friction. My field at this time was polymer science and I recall his comment on the tube theory in which any one polymer can be thought of as sliding along a tube made of the others. Tabor thought that if this is true it must be possible to slow or hurry a polymer by exerting pressure on the material, which would squeeze the tube. And indeed his student at the time, Jones Parry, found this to be true. Another simple but effective reasoning said that if a polymer was given a 'head' that responded to infrared, the movement of the infrared signal could be used to follow the diffusion. Some brilliant experimental work by Jacob Klein (now a professor at Oxford) worked this out.

Tabor's involvement with the Institute of Physics Tribology Group, the journal *Wear* and the International Conference on Wear of Materials (I. M. Hutchings)

The inaugural committee meeting of the Tribology Group of the Institute of Physics (IOP) was held at the Cavendish Laboratory on 30 July 1980, and at that meeting David Tabor was elected chairman. The vitality of the new group is evident from the large number of meetings it organized—no fewer than eleven in less than three years—and the interdisciplinary nature of the subject is illustrated by the collaboration that was set up, even so early in its life, between the group and the Royal Institute of Chemistry, the Metals Society (later to be absorbed into the Institute of Materials, Minerals and Mining) and the Institution of Mechanical Engineers. Although Tabor handed over the administrative task of chairman, he continued thereafter to be an active supporter of group meetings, and the informal and thoughtful discussions that often followed presentations were enriched by his participation. He gave the introductory paper 'Tribology and physics' (49) at the three-day international conference organized by the group in April 1991 to celebrate its tenth anniversary.

Although Tabor was never a member of the Editorial Board of the journal *Wear*, which was founded in 1957, he published many papers in it, including the very first paper in the first issue, entitled 'Friction, lubrication and wear of synthetic fibres' (14). His contribution to the subject of wear also included the opening paper at the first International Conference on Wear of Materials, held in St Louis, Missouri, in April 1977, entitled 'Wear—a critical synoptic review' (45). In that paper, in his characteristically self-effacing style, he suggested that he had been invited to speak because although he had studied surface interactions for many years, he had never studied wear itself. He then proceeded to give a very thoughtful and comprehensive review of the current theories of wear. The biennial conference, like the journal and the IOP Tribology Group, continues to thrive and is now celebrating its 30th anniversary.

The Bowden–Tabor collaboration

Bowden and Tabor both had great physical insight and were sources of inspiration to their students. Tabor regretted not knowing more mathematics but collaborated well with those, such as J. A. Greenwood and Professor K. L. Johnson, who had mastered contact mechanics. Both Bowden and Tabor were exciting lecturers on their research. Bowden was not happy with formal lectures; in contrast, Tabor was, and wrote excellent textbooks (11, 12, 19, 30). Bowden found book writing a chore and was lucky to collaborate with someone as skilled in writing and linguistics as Tabor. Without Tabor, the now classic texts on friction and lubrication, and separately hardness (11, 12, 19), would probably not have been completed. Similarly, with the two texts on explosives, by Bowden and Yoffe, Abe Yoffe took on the main burden of writing. Tabor was no doubt happy to leave the grant acquisition and financial matters to Bowden. In his lifetime Bowden set up three laboratories: PCS, the Melbourne laboratory and the Tube Investments laboratory at Hinxton; the last of these now houses the Human Genome Project. Bowden was a research director of General Electrics and was on the board of the National Physical Laboratory. He also founded a government explosives advisory committee, now chaired by John Field. Tabor was also influential on committees, particularly those that related to tribology. Both saw the advantages of interdisciplinary science and both saw the value of having good technical help. So far, 11 former PCS people have been elected Fellows of the Royal Society. To my knowledge, the Bowden and Tabor relationship was never acrimonious because each had a mutual respect for the other's talents. No assessment of PCS can be made without mentioning their colleagues Abe Yoffe (the third Head of PCS) and Jeof Courtney-Pratt. Mott had such a high impression of Yoffe that he moved to an office in PCS when he retired from being Cavendish Professor and Yoffe was Head of PCS. Courtney-Pratt was an exceptional 'hands-on' experimentalist with a series of achievements in developing optical and high-speed photographic techniques. To have worked with these four inspirational scientists was a great privilege.

Recollections of David Tabor

Kevin Kendall FRS

I first met David Tabor in early 1966 when I visited the PCS Group in the Cavendish Laboratory in Cambridge to discuss my starting a PhD project under his supervision later that year. Tabor impressed me with his quiet friendly style and with his lucid explanations of friction. He also showed me a special recreation of Galileo's experiment of rolling balls down an inclined plane, but with rubber exerting an interesting resistance to rolling. The interview turned into a scientific discussion. I thought his technique of demonstration and logic to be superb and decided on the spot to join him.

A vivid memory is that Tabor also seemed to be impressed by me, which surely must have been impossible considering the gulf between a callow new graduate and a masterful scientist who had just published his second large volume on friction and lubrication with Philip Bowden in 1964. Later, I found that he always approached people with humility and respect, no matter what the outward appearances.

About 12 months later, I had started the three-year PhD project and showed Tabor the marvellous new friction apparatus that I had designed to be built by the workshop. It was truly novel; it did not work! The reason was my error in allowing the normal force to be influenced by the friction force, when I should have made the two forces independent. Tabor patiently explained this simple mistake in a way that did not make me feel stupid. His sympathy and humour added a memorable dimension to my error.

As the PhD project proceeded, Tabor allowed me lots of freedom to do my own thing, visiting a couple of times a week around midday to chat and 'borrow' cigarettes. After a further 12 months the project stalled. The results looked random and I could not find a rational way forward. He took me into his capacious office in the old Cavendish Building and asked in detail what I was trying to do. He then guided me through the Hertz theory of friction and allowed me to propose experiments to analyse the difficult problems. It was a revelation to listen to his penetrative questions and feel his cool rationality. After that session I progressed rapidly and completed a successful PhD within three years. That meeting had a marked effect on my subsequent approach to research.

I did not realize at that time what he had been going through. His long-time colleague and mentor, Bowden, had just died in 1968 and that must have placed a great strain on his life and work. Tabor put together a memoir to celebrate Bowden's life and achievements. I remember him going round the department and personally giving us each a copy of the biography with a few words on his admiration for Bowden. I still look at the memoir from time to time and reflect on the loyalty and dedication that Tabor had for his colleague and friend.

Later, having left Cambridge to join British Railways, I was trying to write up the thesis results for publication but got into serious difficulties as I attempted in the evenings, after work, to condense a thick thesis into a compact 10-page journal paper. Tabor gave me time to produce a draft, but the document did not look good and I had to admit defeat. Tabor then rewrote the paper completely and made an excellent job of it. I began to understand that he had a sure feel for the important elements of the story. But, despite my incompetent write-up, he still put me down as the first author, which seemed significant at the time. His generosity of spirit was unusual because most academics strive for priority and recognition in the competitive world of science, and students often come last.

In conclusion, David Tabor had a substantial effect on me, both in science and in personal contact. Without him, I think my life would have been much diminished.

Tabor as my PhD supervisor (Jacob Israelachvili FRS)

When I received a good mark in the Experimental Physics Tripos in 1968, it seemed natural for me to try to continue as a PhD student at the Cavendish Laboratory. At a brief meeting with Nevill Mott, then Cavendish Professor, I was told to visit Professor Bowden's department because he had lots of unrestricted money from industry. Apparently, as a non-British subject, I was not eligible for a government grant. A few days later I turned up for my interview with Professor Bowden but was told that he had taken ill and that Dr Tabor would see me instead. I never met Bowden, but I would have if my interview had been scheduled for the previous week. He died shortly afterwards. Tabor seemed pleased that someone who apparently was a good experimentalist was seeking to do his PhD in PCS, which, I quickly gathered, was not regarded as being a 'real physics' department at the Cavendish, but more of a department of 'rubbing and scrubbing'. The then glamorous world of exploding galaxies and solid state physics was to be closed to me, but I was welcomed to the world of 'squalid state physics'. At that short interview, Tabor introduced me to a problem and area that I have worked in ever since, for all of 40 years, and I have never looked back.

Quietly, softly, gently, patiently, he told me of research currently being done by Richard Winterton to measure the van der Waals forces between surfaces as a function of their separation (in air) at the ångström resolution level, and asked whether I would be interested in continuing this work in some new direction; he asked semi-apologetically, as though the problem must sound mundane and old-fashioned in comparison with the glitzy physics that everyone else was then excited and talking about. For some reason, I really liked this 'problem' and lunged straight into it. I also warmed to the idea of working under this kindly-looking professor who talked to me as if I were his intellectual equal, with no sign of impatience with what must have been my many silly questions.

I was given a lab in which I was told J. E. Lennard-Jones FRS had worked, and opposite one used by P. L. Kapitza FRS. It had a large Victorian salon-style fireplace, and an old chemical cabinet containing bottles older than 20 years with labels that showed them to contain enough poisonous chemicals to kill a town the size of Cambridge.

During my first summer, Tabor sent me to my first international conference in Karlsruhe. This was an excellent start and introduction to my new field. I wrote down everything, as if I were still an undergraduate student receiving the gospel from the unquestionably wise. I felt overwhelmed. How could I ever know all that stuff? Reporting back to him, he glanced at my notes and told me not to worry—most of 'that stuff' was wrong or not important. With the papers he gave me to read and the conference behind me, I had learnt enough to plan my research.

He gave me a more or less free hand. We chatted a few times a month, and he guided me mainly by alerting me whenever it looked like I might be going off the rails. I designed a new apparatus to measure van der Waals forces over a larger range (in both directions) than previously done by Winterton and Tabor (34), and also stayed on for one year of postdoctoral research during which I modified the apparatus to measure frictional (shear) forces. The experiments were successful. The apparatuses worked well, as planned (39, 40, 42–44).

Tabor had a tremendous influence on me—mostly, as I slowly realized, in a subliminal way: by example rather than prescription. I now see myself treating my students and postdoctoral researchers the way he treated me; I like it, and they like it. The 'Tabor style' is close to the 'Socratic method'—the aim, however, being more to enquire and get at the truth than to win an argument. It has served me well. But times have changed, and the high-speed, high-stress, world we scientists now live in does not easily accommodate people of David Tabor's gentle nature. But I still always try to aspire to that state of being.

Tony Atkins

My own memories (no doubt felt by all who were taught by Tabor) are of patience, kindness, tolerance of daft or ignorant ideas, and especially the marvellous ability to realize the important physical parameters in a problem and get 90% of what is probably a horrendous solution with simple (but not simplistic) maths. Interestingly, the paper for which Tabor, Arnaldo Silvério and I received the Wilson Award from the American Society for Metals was about creep in hardness, for which Arnaldo's data on tin at room temperature and mine on carbides, borides and nitrides at 2000–3000 °C, all agreed with Tabor's modelling, showing that it is the homologous temperature that counts (20–22, 24–26)!

Jacob Klein

In 1973 I received my Cambridge BA in physics and was looking for a research group in which to do my PhD. In those days solid state physics and radioastronomy were the hottest fields in the Cavendish and were a natural choice for an aspiring graduate student. However, going around the different groups, I was charmed by Tabor's kindness and accessibility, and decided to 'go for the supervisor'—and for what Tabor himself warned me were ostensibly unglamorous research directions—rather than for the fashionable topics of the day.

The problem that Tabor suggested for my PhD arose from the following very practical issue: the polymeric material (polyethylene) used for making rolls of folded bags, such as for waste bin liners, had incorporated in it a small amount (parts per million) of surfactants such as stearamide or oleamide, which had an alkyl tail of some 18 $–CH_2–$ units attached to a polar headgroup. These molecules migrated to the surface of the polymer sheets, creating a surface boundary layer that suppressed adhesion and prevented 'welding' of the bags when they were tightly folded and rolled together into a cylindrical roll. David wanted to understand more about the process by which these surfactants migrated to the surface, and my project was to investigate this. I soon realized that the diffusion of C_{18} chains through polymers lay somewhere between the well-studied case of diffusion of small gas molecules through polymers, and the *terra* almost *incognita* (as it was then) of self-diffusion of polymers. I therefore focused on the latter, work that led in time to the first experimental demonstration (Klein 1978) that long, flexible polymer chains (with hundreds or even thousands of monomers), moving within a polymer matrix with which they were entangled, diffused in a snake-like fashion. This snake-like mechanism had earlier been proposed by the French physicist Pierre-Gilles de Gennes, who termed it 'reptation', and was to prove a very fruitful concept in polymer physics. Thanks in large part to the work of Sir Sam Edwards and his co-workers, the reptation idea now underlies modern molecular understanding of polymer rheology, and was noted prominently in de Gennes's Nobel Prize citation many years later.

I mention this demonstration of a rather fundamental and important idea—reptation—arising from what might have been considered a somewhat mundane problem—stick-prevention in bin-liners—to recall that Tabor had a marvellous facility for asking questions that sounded innocent but could lead to much deeper insights. Another of these simple-sounding (but far from innocent) questions that he often posed when considering tribological problems, his dominant research interest, was: how is energy dissipated during frictional sliding? This came back to me after two decades of working in polymer physics (work that developed from my original PhD with Tabor and Brian Briscoe), when my interests also shifted increasingly to friction and lubrication. I will always remember the long and intimate tutorial Tabor gave me, in 1997 (he was then 84 years old), when he described to a newcomer to the field his views on the essential problems in tribology, where the importance of understanding frictional dissipation was central. And indeed, focusing on this issue in recent years has proved immensely fruitful in providing insights in my own research on boundary lubrication in soft and aqueous systems (Raviv & Klein 2002; Raviv *et al*. 2003; Briscoe *et al*. 2006; Klein 2007).

How to write a scientific paper (Alan Roberts)

One of my most enduring memories of Tabor was the morning he taught me how to write a scientific paper. After struggling for some time to produce useable optical interferograms of the contact of rubber against glass to see the squeezing of water films, he sensed that the time had arrived for a first publication on the topic. I well remember going down to his Free School Lane ground-floor office, armed with photos and data. After the customary warm welcome, we settled down to the task of writing a paper. He asked me various questions about the technical details of the work and then, before my eyes, began to write in small neat longhand. Quickly the blank paper acquired value as he drafted the introduction, methodology, results and broad conclusions. Although it was only a short paper (28), it had a touch of class, and needed very little rewriting before being handed to the lab secretary for 'official' typing before submission. I guess the manuscript was completed in a couple of hours, and this practical demonstration taught me for life how the matter is done. In my turn I have written subsequent papers on this and other topics, and each time I start I recall his good guidance.

The development of the so-called JKR equation was noted in the section 'Friction of metals'.

Yet another surprise came when looking at rubber–glass contacts lubricated with soapy water, essential for wiper/windscreen cleaning. Solids placed in an aqueous electrolyte solution often absorb ions at the surface to form a charged double layer. If two such surfaces are brought together they will repel if the charges are of similar sign. In most colloidal systems it is the repulsion between double layers of similar charge that stabilizes them. The direct measurement of double-layer repulsion forces is not easy, but it turned out to be possible (38) by using optical rubber–glass or rubber–rubber contacts because forces and gap thicknesses could be measured directly—which is far easier than, for example, making observations on suspended draining soap films. It was even possible to look at stable films of saliva and synovial fluid (Roberts 1972) by using the 'soft' contact afforded by rubber.

Brian Briscoe

I still remember vividly my first meeting with David Tabor in the spring of 1970. I was in Cambridge to visit the late Denis Haydon (FRS 1975) with the plan to join him as a research fellow to study cell membranes. Haydon was busy in the morning, so he sent me to see Tabor in his magnificent office in Free School Lane; I recall the old-fashioned lamp standard, desk and an offer to become the 'group chemist' on a (then) SRC grant to study metal cutting in controlled environments. The room, I learnt, had belonged to Philip Bowden, and Tabor was reluctant to use it but Tabor did follow the Bowden multidisciplinary or interdisciplinary group structure that was to become fashionable many years later. In total, the group must have hosted or employed over 60 people at its height*. The support technical service was truly remarkable—the group could build almost anything. I left for Imperial College in 1978, as an Assistant Director of Research, having had many very productive interactions with staff and students; D.T. and I had published about 25 papers together and I had learnt so much about all facets of research but particularly its effective communication.

My first few years were spent working with Soviet visiting scientists and research students, mainly on organic polymers: self-lubrication with Vefa Mustafaev, composite wear with Albert Pogosian, rubber lubrication with Colin Mclune, polymers under hydrostatic stress with Emyr Jones Parry, and the friction of Langmuir–Blodgett films with David Evans and Barry Scruton. Our laboratories were on a false floor in the top of the Old Free School—sadly, even the floor has now gone. The whole place was like that, with one being lucky to have a cupboard around one's desk—I did graduate to that eventually. The rest of PCS was the same—interestingly the group at that time had just three established staff members—Tabor, Abe Yoffe and John Field. Each had a part of the space devoted to their research interests. There were many postdoctoral workers, and I was much the junior of that set. They included Mike Murray, Tony Lee, Nic Gane, Peter Fox, Jean-Jacques Camus, Ted Davis and Yao Liang, all of whom provided a major input to the group's quality of work and its productivity. There was always enough money and resources to undertake even the most adventurous work.

In 1974 we were moved to west Cambridge. There were all sorts of technical problems such as the debris apparently produced by the air circulation units—for example, David Evans and I could not reproduce the clean mica surfaces produced by Jacob Israelachvili. John Pethica and Tony Lee found their high-vacuum scanning electron microscope could not work in the new area because of a Faraday cage effect. Tabor and I had shared many students by then, and also some significant research contracts. He passed on PhD supervision of several students to my charge. My first major supervision was that of Josef Amuzu (shear properties of organic films), which followed up many facets on the interface dissipation process in the adhesion model of friction popularized by Tabor and Philip Bowden. Andrew Briggs came to work on the then new fashion in rubber tribology of the Schallamach wave and facets of adhesion. Ian Roselman had completed

* In fact, the total reached about 150 at its peak before Dr (now Professor) Liang took his group into the IRC on Superconductors. Dr (now Professor) Athene Donald and Dr Richard Jones (now Professor at Sheffield) formed Polymers and Colloids with Sir Sam Edwards, and Richard (now Sir Richard) Friend (FRS 1993), Dr (now Professor) Richard Phillips and Dr Howard Hughes set up their Optoelectronics Group. Such was the 'tree' from the acorn that Bowden and Tabor planted!

his thesis on the friction and wear of carbon fibres (a continuation of the work carried out by Mike Pascoe), and Stephen Kremnitzer began a PhD on the lubrication of synthetic polymer fibres. The work with Mustafaev naturally led to the research with Jacob Klein on the centre of mass diffusion of aliphatic species in polyethylenes. It will have escaped some people but David Tabor and indeed Philip Bowden undertook very little work on wear or indeed fluid lubrication. Wear studies were certainly extremely time-consuming to undertake, and fluid lubrication was then highly mathematical. I felt that they did not suit Bowden and Tabor's style.

An appreciation by Arthur Stripe (Principal Assistant, PCS, 1985–95)

David was known to his staff as a quiet man who appreciated the high levels of skills of the many assistant staff in many disciplines who served the laboratory. Many had day-to-day contact with D.T., as he was fondly known: for example, Alan Peck, the laboratory photographer, had to keep up with the prolific output from David, which kept Alan at full stretch all the time; others had less frequent contact but were always made aware of his appreciation of their skills.

David was a man of quiet humour, much appreciated by those who recognized his talent for sharp observation of an individual's foibles, particularly in the annual monologues that he gave at the PCS Christmas party, to which all looked forward, all hoping to be included—which meant you had 'arrived' in PCS.

On a more personal note, much fun was had by Alan Peck and myself during the time that David was attempting to give up smoking; he would arrive at the laboratory early each morning and from whomever he met first—Alan or myself—David would 'borrow' a cigarette, saying, 'I am giving up, but not quite yet.' This went on daily for some months but always with a smile as though he felt we believed him.

David was a quiet, dignified, very likeable man, and it was a great honour to have known him and to have worked for him.

Honours

1963 Elected FRS
1972 IMechE Tribology Gold Medal
1975 IOP Guthrie Medal
1992 Royal Medal, Royal Society
'Tabor Laboratory' housed the new Polymers and Colloids Group in the Cavendish
1995 Foreign Associate, US National Academy of Engineering
2006 Installed in the American Chemical Society Hall of Fame, for rubber science

Acknowledgements

I am grateful to David Tabor's sons, Dr Daniel Tabor (Daventry) and Professor Michael Tabor (University of Arizona), for such an informative account of David's family history and early career; to Professor Kenneth Johnson FRS (Engineering, University of Cambridge), surely an 'honorary' member of PCS after his many collaborations with Tabor and others, for his account of the friction studies; and to Sir Sam Edwards FRS (Cavendish) for his comments on Tabor in Caius College. I am also grateful to the following, formerly PCS, for their reminiscences: Professor Tony Atkins (University of Reading), Dr Bill Allison (Cavendish), Professor Brian Briscoe (Imperial College, University of London), Brian Dicker (Bristol), Professor Athene Donald FRS (Cavendish), Professor Edward

Freitag (Eidgenössische Technische Hochschule, Zürich), Dr Keith Fuller (Malaysian Rubber Producers), Dr Nicholas Gane (Isle of Lewis), Dr Dougal Goodman (The Foundation for Science and Technology), Professor Ian Hutchings (Manufacturing Science, University of Cambridge), Professor Jacob Israelachvili FRS (University of California at Santa Barbara, USA), Professor Kevin Kendall FRS (University of Birmingham), Professor John Pethica FRS (University of Oxford), Dr Alan Roberts (Malaysian Rubber Producers), Arthur Stripe (Cambridge) and Professor John Williams (Engineering, University of Cambridge). Finally, I thank Mrs Karen Scrivener for typing and collating this memoir, and Dr Stephen Walley for compiling Tabor's reference list.

The frontispiece photograph was taken in 1977 by Godfrey Argent and is reproduced with permission.

References to other authors

Archard, J. F. 1957 Elastic deformation and the laws of friction. *Proc. R. Soc. A* **243**, 190–205.

Berman, R. (ed.) 1965 *Physical properties of diamond*. Oxford: Clarendon Press.

Briscoe, W. H., Titmuss, S., Tiberg, F., McGillivray, D. J., Thomas, R. K. & Klein, J. 2006 Boundary lubrication under water. *Nature* **444**, 191–194.

Derjaguin, B. V. & Toporov, J. P. 1974 The role of adhesion in the rolling and sliding friction of polymers. In *Advances in polymer friction and wear* (ed. L.-H. Lee), pp. 771–779. New York: Plenum.

Field, J. E. (ed.) 1979 *The properties of diamond*. London: Academic Press.

Field, J. E. (ed.) 1992 *The properties of natural and synthetic diamond*. London: Academic Press.

Frost, H. J., Goodman, D. J. & Ashby, M. F. 1976 Kink velocities in dislocations in ice. *Phil. Mag.* A **33**, 951–961.

Goodman, D. J., Allan, A. J. & Bilham, R. G. 1975 Wire strainmeters on ice. *Nature* **255**, 45–46.

Goodman, D. J., Frost, H. J. & Ashby, M. F. 1981 The plasticity of polycrystalline ice. *Phil. Mag.* A **43**, 665–695.

Greenwood, J. A. & Williamson, J. B. P. 1966 The contact of nominally flat surfaces. *Proc. R. Soc. A* **295**, 300–319.

Greenwood, N. N. & Spink, J. A. 2003 An antipodean laboratory of remarkable distinction. *Notes Rec. R. Soc. Lond.* **57**, 85–105.

Hird, J. R. & Field, J. E. 2004 Diamond polishing. *Proc. R. Soc. A* **460**, 3547–3568.

Johnson, K. L., Kendall, K. & Roberts, A. D. 1971 Surface energy and the contact of elastic solids. *Proc. R. Soc. A* **324**, 301–313.

Jost, H. P. (ed.) 1966 *Lubrication—tribology. Education and research. A report.* London: HMSO.

Klein, J. 1978 Evidence for reptation in an entangled polymer melt. *Nature* **271**, 143–145.

Klein, J. 2007 Frictional dissipation in stick-slip sliding. *Phys. Rev. Lett.* **98**, 056101.

Palmer, A. C., Goodman, D. J., Ashby, M. F., Evans, A. G., Hutchinson, J. W. & Ponter, A. R. S. 1983 Fracture and its role in determining ice forces on offshore structures. *Ann. Glaciol.* **4**, 216–221.

Ponter, A. R. S., Palmer, A. C., Goodman, D. J., Ashby, M. F., Evans, A. G. & Hutchinson, J. W. 1983 The force exerted by a moving ice sheet on an offshore structure. Part 1. The creep mode. *Cold Regions Sci. Technol.* **8**, 109–118.

Raviv, U., Giasson, S., Kampf, N., Gohy, J.-F., Jerome, R. & Klein, J. 2003 Lubrication by charged polymers. *Nature* **425**, 163–165.

Raviv, U. & Klein, J. 2002 Fluidity of bound hydration layers. *Science* **297**, 1540–1543.

Reynolds, O. 1876 On rolling friction. *Phil. Trans. R. Soc.* **166**, 155–174.

Roberts, A. D. 1972 Direct measurement of electrical double-layer forces between solid surfaces. *J. Colloid Interface Sci.* **41**, 23–34.

Sanderson, T. J. O. 1988 *Ice mechanics. Risks to offshore structures*. London: Graham & Trotman.

Van Bouwelen, F. M. 2003 Electron microscopy analysis of debris produced during diamond polishing. *Phil. Mag.* **83**, 839–844.

Wadhams, P., Squire, V. A., Goodman, D. J., Cowan, A. M. & Moore, S. C. 1988 The attenuation rates of ocean waves in the marginal ice zone. *J. Geophys. Res.* **93** (C6), 6799–6818.

Bibliography

The following publications are those referred to directly in the text. A full bibliography is available as electronic supplementary material at http://dx.doi.org/10.1098/rsbm.2007.0031 or via http://journals.royalsociety.org.

(1) 1939 (With F. P. Bowden) The area of contact between stationary and between moving surfaces. *Proc. R. Soc. A* **169**, 391–413.

(2) (With F. P. Bowden & L. Leben) The influence of temperature on the stability of a mineral oil. *Trans. Faraday Soc.* **35**, 900–904.

(3) 1941 (With F. P. Bowden) The contact of colliding surfaces and the influence of lubricant films. *J. Coun. Scient. Ind. Res. Aust.* **14**, 152–160.

(4) (With F. P. Bowden) The contact of colliding surfaces. *Engineer* **172**, 380–382.

(5) 1942 (With F. P. Bowden) The mechanism of metallic friction. *Nature* **150**, 197–199.

(6) 1943 (With F. P. Bowden & A. J. W. Moore) The ploughing and adhesion of sliding metals. *J. Appl. Phys.* **14**, 90–91.

(7) (With F. P. Bowden) The lubrication by thin metallic films and the action of bearing metals. *J. Appl. Phys.* **14**, 141–151.

(8) 1945 (With F. P. Bowden) Friction and lubrication. *Annu. Rep. Chem. Soc.* **42**, 20–46.

(9) (With F. P. Bowden & J. N. Gregory) Lubrication of metal surfaces by fatty acids. *Nature* **156**, 97–98.

(10) The frictional properties of some white metal bearing alloys: the role of the matrix and the hard particles. *J. Appl. Phys.* **16**, 325–337.

(11) 1950 (With F. P. Bowden) *The friction and lubrication of solids*, part I. Oxford: Clarendon Press.

(12) 1951 *The hardness of metals*. Oxford University Press.

(13) 1955 (With K. R. Eldredge) The mechanism of rolling friction. I. The plastic range. *Proc. R. Soc. A* **229**, 181–198.

(14) 1957 Friction, lubrication and wear of synthetic fibres. *Wear* **1**, 5–24.

(15) 1958 (With L. E. Raraty) The adhesion and strength properties of ice. *Proc. R. Soc. A* **245**, 184–201.

(16) 1959 (With H. G. Howell & K. W. Mieszkis) *Friction in textiles*. London: Butterworths.

(17) Junction growth in metallic friction: the role of combined stresses and surface contamination. *Proc. R. Soc. A* **251**, 378–393.

(18) 1960 (With J. A. Greenwood & H. Minshall) Hysteresis losses in rolling and sliding friction. *Proc. R. Soc. A* **259**, 480–507.

(19) 1964 (With F. P. Bowden) *The friction and lubrication of solids*, part II. Oxford: Clarendon Press.

(20) 1965 (With A. G. Atkins) Mutual indentation hardness apparatus for use at very high temperatures. *Br. J. Appl. Phys.* **16**, 1015–1021.

(21) (With A. G. Atkins) On 'Indenting with pyramids'. *Int. J. Mech. Sci.* **7**, 647–650.

(22) (With A. G. Atkins) Plastic indentation in metals with cones. *J. Mech. Phys. Solids* **13**, 149–164.

(23) (With F. P. Bowden) Deformation, friction and wear of diamond. In *Physical properties of diamond* (ed. R. Berman), pp. 184–220. Oxford: Clarendon Press.

(24) 1966 (With A. G. Atkins) Hardness and deformation properties of solids at very high temperatures. *Proc. R. Soc. A* **292**, 441–459.

(25) (With A. G. Atkins) The plastic deformation of crossed cylinders and wedges. *J. Inst. Metals* **94**, 107–115.

(26) (With A. G. Atkins & A. Silvério) Indentation hardness and the creep of solids. *J. Inst. Metals* **94**, 369–378.

(27) (With P. Barnes) Plastic flow and pressure melting in the deformation of ice. I. *Nature* **210**, 878–882.

(28) 1968 (With P. Barnes) Plastic flow and pressure melting in the deformation of ice. I. In *Proc. Commission of Snow and Ice*, pp. 303–315. Berne: International Association of Scientific Hydrology.

(29) (With A. D. Roberts) Fluid film lubrication of rubber: an interferometric study. *Wear* **11**, 165–166.

(30) *Gases, liquids and solids*. London: Penguin.

(31) (With R. F. Willis) Thin film lubrication with substituted silicones: the role of physical and chemical factors. *Wear* **11**, 145–162.

(32) 1969 Frank Philip Bowden. *Biogr. Mems Fell. R. Soc.* **15**, 1–38.

(33) (With R. F. Willis) The formation of silicone polymer films on metal surfaces at high temperatures and their boundary lubricating properties. *Wear* **13**, 413–442.

(34) (With R. H. S. Winterton) The direct measurement of normal and retarded van der Waals forces. *Proc. R. Soc. A* **312**, 435–440.

(35) 1970 (With J. C. F. Walker) Creep and friction of ice. *Nature* **228**, 137–139.

(36) 1971 (With P. Barnes & J. C. F. Walker) The friction and creep of polycrystalline ice. *Proc. R. Soc. A* **324**, 127–155.

(37) (With K. Kendall) An ultrasonic study of the area of contact between stationary and sliding surfaces. *Proc. R. Soc. A* **323**, 321–340.

(38) (With A. D. Roberts) The extrusion of liquid between highly elastic solids. *Proc. R. Soc. A* **325**, 323–345.

(39) 1972 (With J. Israelachvili) Measurement of van der Waals dispersion forces in the range 1.4 to 130 nm. *Nature Phys. Sci.* **236**, 106.

(40) (With J. Israelachvili) The measurement of van der Waals dispersion forces in the range 1.4 to 130 nm. *Proc. R. Soc. A* **331**, 19–28.

(41) (With C. M. Pooley) Friction and molecular structure: the behaviour of some thermoplastics. *Proc. R. Soc. A* **329**, 251–274.

(42) 1973 (With J. Israelachvili) Van der Waals forces: theory and experiment. *Prog. Surf. Membrane Sci.* **7**, 1–55.

(43) (With J. Israelachvili) Shear properties of molecular films. *Nature Phys. Sci.* **241**, 148–149.

(44) (With J. Israelachvili) The shear properties of molecular films. *Wear* **24**, 386–390.

(45) 1977 Wear: a critical synoptic review. *Trans. ASME: J. Lubric. Technol.* **99**, 387–395.

(46) (With J. A. Williams) The role of lubricants in machining. *Wear* **43**, 275–292.

(47) 1978 (With D. J. Goodman) Fracture toughness of ice: a preliminary account of some new experiments. *J. Glaciol.* **23**, 651–660.

(48) 1979 Adhesion and friction. In *The properties of diamond* (ed. J. E. Field), pp. 325–350. London: Academic Press.

(49) 1992 Tribology and physics. *J. Phys. D: Appl. Phys.* **25**, A1–A2.

(50) (With J. E. Field) Friction of diamond. In *The properties of natural and synthetic diamond* (ed. J. E. Field), pp. 547–571. London: Academic Press.