SIR ALFRED BRIAN PIPPARD
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Sir Brian Pippard was a brilliant experimental physicist with a deep understanding of physics and physical processes; he used this understanding to make pioneering contributions to condensed matter physics. He will be particularly remembered as the first experimenter to map a Fermi surface and for his non-local theories of electromagnetic response in normal metals and superconductors, the latter predating the theory of superconductivity of Bardeen, Cooper and Schrieffer. Most of his career was spent at the Cavendish Laboratory, University of Cambridge, where he held the Cavendish Professorship of Experimental Physics from 1971 to 1982. From 1966 to 1973 he was the first President of Clare Hall, then a newly founded graduate college. He died aged 88 years on 21 September 2008.

FAMILY BACKGROUND AND EDUCATION

Alfred Brian Pippard, always known as Brian, was born at Earl’s Court, London, on 7 September 1920, the second son of Professor Alfred John Sutton Pippard (FRS 1954), a distinguished civil engineer, who was President of the Institution of Civil Engineers from 1958 to 1959. His elder brother, Dr John Sutton Pippard, is a psychiatrist who performed distinguished work at Claybury Hospital, Essex. His father was an engineering consultant in London when Brian was born, and he subsequently held the posts of Professor of Engineering, University College, Cardiff, 1922–28, Professor of Civil Engineering, Bristol University, 1928–33 (where he was associated with the experimental testing of aircraft structures, especially the R100 and R101 airships), and finally Professor of Civil Engineering and Head of Department at Imperial College, London, 1933–56. The family moved as his father took up these various appointments. From 1920 to 1923 they were in London, from 1923 to 1928 in Penarth, from 1928 to 1933 in Bristol and from 1933 to 1939 in Wimbledon.
Brian was fortunate in having an outstanding education at the various schools he attended. After junior school at Westbourne House, Penarth, 1925–28, and Bristol Grammar School, 1928–30, he attended Clifton College Preparatory School from 1930 to 1934. He was awarded an entrance scholarship to Clifton Senior College, which he attended from 1934 to 1938. At first Brian and his brother were day boys, but from 1933 they were boarders in School House.

Brian spoke warmly of his school days, remarking that he did not recollect a single bad teacher and that some of them were quite exceptional. Science teaching at Clifton College in those days was the best in the country. As Brian has written (30):

E. J. Holmyard taught like a polished don, W. C. Badcock was a martinet who loved physics, F. B. Finter’s enthusiasm verged on intellectual bullying, and there were others of less unusual character. These were all men whose gifts would have taken them, in post-war years, away from teaching into research or industry. It was my good fortune that in the inter-war years school teaching was still regarded as a proper occupation for a gentleman and scholar, and the pay was sufficient.

His housemaster in School House was Cecil F. Taylor, ‘a survivor of the Bloomsbury group who brought to us a civilised contact with literature and art’. This part of Brian’s education was to be reflected in the elegance and stylishness of his writings.

Music played a central role in Brian’s life. He began learning the piano at the age of five years but it was only at Clifton Preparatory School at the age of ten years that his imagination was sparked by the great classical composers. He was lucky to have inspired teachers; the first was Miss Rachel Story, with whom he learned a new simple piece of the classical repertoire every lesson. After a year she recommended him to the Director of Music, Douglas Fox, who was to be his piano teacher for the next seven years. Fox had lost his right arm in World War I but was a musician and teacher of outstanding ability who had performed the Ravel left-hand concerto in concert at a time when it was rarely played. Brian became a really excellent pianist and his passion for the piano remained with him all his life. Fox’s love of the Viennese classics was reflected in Brian’s enthusiasms, and he seriously suggested that Brian should consider a career in music. But Brian realized his own limitations as a pianist and musician when compared with his contemporary at Clifton, David Willcocks. The result was that he decided at about the age of 15 years that his future lay in the sciences, a decision he never regretted. Nonetheless, his interest in music was really serious. He made early attempts at composition, but later destroyed most of his efforts. However, among his papers he preserved a few examples that, in his words, ‘show occasional glimpses of interesting ideas’.

After he had passed the School Certificate Examination in the summer of 1935, he was allowed to study physics, chemistry and double mathematics, as well as the compulsory course in English literature. He thoroughly enjoyed the physics and chemistry but believed he could not compete with the best students in mathematics. When he was in the sixth form, quantum mechanics was only 10 years old and rumour had it that mastering the new physics demanded high ability in mathematics. As a result, he considered that physical chemistry would be an appropriate career choice.

He won a minor entrance scholarship to Clare College, Cambridge, in December 1937 and in his first year, 1938–39, he read physics, chemistry, physiology and mathematics in the Preliminary Examination to Part I of the Natural Sciences Tripos with outstanding academic success. By the beginning of his second year at Cambridge, World War II had broken out. Brian’s tutor at Clare, Tenney Spooner, informed him that the country did not need chemists but that there was a demand for physicists. Furthermore, if he switched from chemistry to
physics, he would be allowed to dispense with the second year of Part I and proceed directly
to Part II Physics. In addition, by making the switch to physics he could avoid being called
up. Brian responded to this broad hint and immediately found that physics was indeed what
he had always wanted to do.

The physics courses were strongly based on experiment and the interpretation of experi-
mental data with modest mathematical content. This was a legacy of Ernest Rutherford’s
tenure of the Cavendish chair from 1919 until his death in 1937. Brian had little enthusiasm
for higher mathematics for its own sake but enjoyed mastering more elementary methods that
provided answers to well-defined physical problems. As he remarked (30), ‘I enjoy using
(mathematics) for solving particular problems, but as a basic approach to thought—as a way
of life—it is utterly foreign.’ In fact, Brian mastered a great deal of mathematics relevant for
physics, for example, from Pauling and Wilson’s *Introduction to quantum mechanics*.

He had been expecting to take Part II of the Natural Sciences Tripos at the end of his second
year, but the government was not yet ready to employ physicists and so he continued with
his studies at Cambridge for a third year. He attended lectures in the mathematics faculty by
A. H. (later Sir Alan) Wilson (FRS 1942) on thermodynamics, which he considered the best
he had heard on the subject, and which had a strong influence on his future undergraduate
text *Elements of classical thermodynamics* (8). Many of the best physicists were by now away
from Cambridge supporting the war effort, so he spent much time in the university library
seeking out physics that he enjoyed. At the end of the academic year 1940–41, he graduated
with first-class honours in Part II of the Natural Science Tripos and was awarded the 1941
Murgoci Prize at Clare College.

**THE WAR YEARS**

In spring 1941 Brian, along with all the physicists in his year at Cambridge, was interviewed
by C. P. Snow to judge his suitability for research in support of the war effort. He was
assigned to work on radiolocation, subsequently known as radar. He was next interviewed
by John (later Sir John) Cockcroft FRS, then the Chief Superintendent of the Air Defence
Research and Development Establishment (ADRDE) at Christchurch, Hampshire. During
the interview, Brian remarked that he had not find electronic circuitry interesting and this
may have been a contributing factor in the decision to assign him to the group working on
aerials and transmission lines. A year later, the group moved from the south coast to Malvern
for reasons of security. The ADRDE and the Telecommunications Establishment were con-
solidated at Malvern, where the combined organization was renamed the Radar Research and
Development Establishment (RRDE).

The four-year period during which he was employed as a Scientific Officer at the RRDE
was devoted to mastering and developing microwave radar techniques. During these years,
the development of magnetrons and klystrons provided new powerful sources of microwave
radiation that, combined with parabolic antennae, could provide higher angular resolution than
the existing radar arrays, which operated at much longer metre wavelengths.

At that time, understanding of the physics of radio propagation and microwave components
was rudimentary and had to be developed quickly. Brian was fortunate in his trainers and
supervisors. Soon after he joined the ADRDE, he attended what he considered an exceptional
course of special lectures on the physics of waves and radio by Mr J. A. Ratcliffe (FRS 1951),
who after the war returned to the Cavendish Laboratory as a lecturer and Head of the Radio Group. For most of his career as a Scientific Officer, Brian’s Head of Section was Dr John Ashmead, who had just completed his PhD at the Cavendish. He was an excellent physicist, in Brian’s words ‘a man of imagination and integrity, a wise counsellor’. After the war, he too returned to the Cavendish as a university lecturer. The four years devoted to uninterrupted research in radio and microwave techniques at the RRDE provided an ideal training in research techniques. In particular, he developed the discipline of performing experimental work with economy and imagination. In his words (30),

In the ten post-war years during which my Cambridge research was largely on microwaves and metals, I stuck to the same basic technique and needed to waste no time on fancy electronics; my war years were well spent, learning and applying the most economical methods.

Much of the effort was devoted to determining the beam patterns of microwave antennae in different configurations and to developing flexible systems that would scan rapidly across targets of interest. Little was known about waveguides at that time. A delightful example is provided by Brian’s work on a primitive 6-foot parabolic antenna installed on a tower 200 feet high at Christchurch. The microwave signals were generated by a magnetron at ground level, connected to the antenna by a long vertical waveguide. The signal reflected from a distant object returned to the receiver by the same route. To enable the antenna to scan the sky, a rotating joint was included in the waveguide and attached to a bent circular copper waveguide, which fed the signal to the focus of the antenna. The system did not work well. As the antenna rotated on its axis, signals could be detected from some directions but not from others. The solution was suggested by Mr J. M. C. Scott, the Head of the Theory Section, who also returned to the Cavendish Laboratory as a university lecturer after the war. Scott came up to Brian and said softly, as was his manner, ‘Circularly polarised waves are reflected with the opposite sense of rotation.’ This was the solution. Linearly polarized waves propagated up the vertical copper tube to the antenna, but the bent sections could convert them into circularly polarized waves. On reflection, these waves returned through the same arrangement of waveguides and were converted into signals, linearly polarized in a direction perpendicular to those transmitted. Another example of the experience gained at the RRDE concerned improving the polar diagram of a parabolic reflector used to detect the returning signals at the focus of the antenna. Brian’s understanding of optics enabled him to realize that placing a circular reflector of the right size but shifted forward by one-sixth of a wavelength would eliminate unwanted sidelobes. This elegant solution worked beautifully. The sequel to this story is delightfully told by Brian (30):

At the end of the War, I was assigned to a small interservice committee charged with drawing up a glossary of new terms generated by radar. The chairman was an ageing, but still sharp, civil servant called Bainbridge-Bell and referred to as BB. At one point someone asked, ‘What do we call that plate you stick in a paraboloid to kill reflections?’ and another answered, ‘We call it the Pippard plate.’ ‘Oh,’ I said, with a false-modest giggle, ‘I don’t think we can call it that.’ ‘No,’ said BB, ‘I don’t think we can.’ So, we cooked up ‘apex matching plate’ on the spot, in defiance of all principles of lexicography, and dished my first chance of immortal fame.

A final example of Brian’s ingenuity concerned John Ashmead’s idea of using a spherical antenna rather than a parabolic reflector so that the feed could be moved in the focal plane and scan the area about the radar target. Brian was responsible for the feed design. Because of its similarity to a pig, it was christened the Hoghorn (1, 2). The name came to the Glossary
Committee after the war. ‘What a disgusting name,’ said BB. ‘Who was responsible for that?’ ‘I was,’ I said in a small voice. ‘Well, you ought to be ashamed of yourself.’ Despite this, the name stuck and the concept was used after the war in television and communications relay towers.

GRADUATE STUDENT TO CAVENDISH PROFESSOR

The immediate postwar years provided a cornucopia of opportunities for innovative research. Basic physics had been on hold for six years, during which a vast array of new experimental capabilities had become available. Many of the very best physicists had been deeply involved in the practical problems of radar, atomic weapons, materials research and so on. As Brian commented (30),

I was … thrown into the newest and most exciting area of instrumental design. Electromagnetism and wave optics were central and the four years I spent on radar were a very positive course of education, during which I developed skills that were immediately useful when, in 1946, I returned to Cambridge as a research student under David Shoenberg.

Brian was awarded a Stokes studentship at Pembroke College, only later discovering that he was not a preferred candidate but had been awarded the studentship because he could take charge of the chapel choir (32). He became a member of the Royal Society Mond Laboratory, built for Piotr Kapitza FRS in the 1930s, which was then the centre of low-temperature physics research in Cambridge. John Ashmead persuaded David Shoenberg (FRS 1953) that Brian should repeat measurements made by Heinz London (FRS 1961) on the surface resistance of superconducting tin at low temperatures (London 1940), and Brian took full advantage of this opportunity. He brought back to Cambridge not only a highly developed set of experimental and interpretative skills, but also klystrons, crystal rectifier cartridges and bits and pieces of waveguides—the authorities apparently had no objection to the recycling of the fruits of wartime research in a civilian context. With his experience of microwave circuits and the new technologies, what had for London been a demanding set of experiments was converted into what Brian called the ‘easy task’ of repeating the experiments with much enhanced precision.

From his war work Brian was already familiar with the skin effect, the restriction of microwave fields to a thin surface layer when electromagnetic waves are incident on a metal surface, and he now studied the phenomenon in superconducting metals, which lose all electrical resistance below a sharply defined transition temperature near absolute zero. At that time, superconductivity was still a mystery, although in 1935 Heinz London and his brother Fritz had already hinted that a superconductor might be a quantum fluid, a system of many electrons all governed by an effective single-particle wavefunction, as though many particles had entered the same state (London & London 1935). If this function were sufficiently rigid, meaning that it was completely unperturbed by an applied magnetic field, the superconductor should show a local relation $J = \Lambda A$ between the current density $J$ and the magnetic vector potential $A$, analogous to Ohm’s law $J = \sigma E$ in the normal metal. Consequently, there would be a limited penetration depth for the magnetic field, corresponding to the microwave skin depth.

To measure the surface resistance as the temperature was lowered through the superconducting phase transition, Brian built microwave resonant circuits of superconducting
materials such as tin and mercury (3). By measuring the bandwidth, or \( Q \)-factor, Brian successfully observed a marked decrease in microwave absorption when the materials were cooled through their transition temperatures.

As well as confirming and expanding the current models of microwave response in superconductors, his doctoral work contained the seeds of two further developments. First, he noticed that, by combining his bandwidth measurements with measurements of the shift in resonant frequency between the normal and superconducting states, he could obtain complete information on the surface impedance, and hence an unambiguous value for the complex conductivity of his samples. In the two-fluid language of the time, this gave full information on the density of superelectrons and also showed that at microwave frequencies lossy processes continued among the normal electrons, even in the superconducting state (4).

Second, he reaffirmed Heinz London’s observation (London 1940) that in normal metals the surface resistance did not continue to decrease with falling temperature as the direct current resistance did, but reached a limit. London had already suggested that this effect arose when the mean free path of electrons became greater than the skin depth. Brian set about developing a theory of this ‘anomalous skin effect’ by using a simplified model in which he regarded as effective only those electrons moving at such a small angle to the surface that they remained within the skin depth between scatterings. These electrons would experience a more or less constant field and so make a full contribution to the surface current. In the limit of long free paths his model showed that the surface conductivity should be independent of the free path: increases in free path are compensated for by a decrease in the proportion of effective electrons. He also found that the surface resistance became a simple function of the density of conduction electrons, and varied as the two-thirds power of the frequency (5).

At this point he found he had inadequate mathematical tools to perform a complete analysis. As he confessed (30):

> I had no notion of how to extend classical arguments so that they applied to degenerate Fermi gases, nor had I heard of the Boltzmann equation—not that such knowledge would have been anything but a hindrance.

He therefore sought help from Ernst Sondheimer, a theoretician friend, who formulated the problem more precisely with the use of three-dimensional Fermi statistics and the Boltzmann equation. Sondheimer found the integro-differential equation that describes the motions of the electrons and solved it with the assistance of G. E. H. Reuter. The full theory of the anomalous skin effect was published, and it fitted all Brian’s data remarkably closely (Reuter & Sondheimer 1948). This theory was the appropriate exact exposition of Brian’s ‘ineffectiveness concept’.

An important feature of this work on the anomalous skin effect was the understanding that it was a ‘non-local effect’: the current at a point depended not only on the electric field at that point but also on the field at neighbouring points through which the electrons had travelled since last being scattered. Thus, the simple version of Ohm’s law had to be replaced by one that took account of these non-local contributions. The new version was presented in what Brian referred to as ‘tidy form’ by Robert Chambers. Brian fully acknowledged the contributions of Chambers and later Volker Heine (FRS 1974) for the ‘patient and learned way they held my hand, teaching me both physics and the right way to formulate what were, for them usually, rather simple problems.’

In 1947 Brian was appointed a university demonstrator and returned to Clare College as a research fellow and director of studies in physics. After his doctorate he became interested
in the geometry and dynamics of normal–superconducting boundaries, and introduced the
important idea that their stability was determined by whether the magnetic penetration depth
was larger or smaller than a characteristic ‘coherence length’ that, like the penetration depth,
diverged at the transition temperature ($T_c$). This was of considerable importance at the time,
because it led the way in the West, by way of laminar models, towards a more rational under-
standing of type II superconductors. However, this laminar picture was superseded within a
few years by the lattice of quantized flux lines proposed by A. A. Abrikosov (ForMemRS
2001), based on the phenomenological theory of superconductivity proposed by Lev Landau
(ForMemRS 1960) and Vitali Ginzburg (ForMemRS 1987) (Ginzburg & Landau 1950),
which itself slightly preceded Brian’s work. Unknown to Brian, their theory too contained a
coherence length, and they had drawn similar conclusions about the normal–superconducting
interface energy. However, their theory was based on treating the transition to superconductiv-
ity as a second-order phase transition, and the Ginzburg–Landau coherence length was related
to an expansion parameter whose microscopic significance was far from clear. Brian, in con-
trast, argued more concretely. He reasoned that the superconducting wavefunction could not
be completely rigid, as the London brothers had assumed. The magnetic field would surely
perturb the wavefunction to some extent, and this perturbation would be expected to spread
through the system. However, the superconducting condensation occurred only below the
transition temperature $T_c$, suggesting that it must be a weak interaction involving only those
electrons within $kT_c$ of the Fermi surface. Such a group of electrons would be expected to get
out of phase with each other at distances greater than $\hbar v_F/kT_c$, and Brian suggested that the
coherence length was probably of this order, typically $1\mu m$.

At about the same time, Brian made two significant new observations on the penetration
depth (7). First, he measured the skin depth of tin alloyed with indium to reduce the free path
of electrons. He observed that the alloying not only increased the normal state skin depth as
expected, but also, and in much the same way, increased the superconducting penetration
depth. However, it had little effect on the transition temperature or the condensation energy,
suggesting that the density of superelectrons was not much altered. Second, he measured the
anisotropy of the penetration depth in pure tin and found that it varied with surface orienta-
tion in a manner that was not possible for the local London relation between supercurrent and
vector potential, but was very similar in form to the anisotropy of the normal state skin depth,
governed by Chambers’s non-local equation. Brian drew the bold conclusion that he must be
observing non-local effects in the supercurrent, and in a celebrated paper of 1953 proposed (7)
that the local London relation $J = \Lambda A$ should be replaced by a non-local equation analogous to
Chambers’s formula. In this picture, the supercurrent depended on distant values of the vector
potential, the range being limited by an electromagnetic coherence length $\xi$. In the dirty limit,
he assumed that $\xi$ must be essentially the electron free path, but in pure materials he argued
that $\xi$ could not be unbounded, but must be limited by a new length $\xi_0$, which he thought might
be of the same order as the coherence length that he had introduced in considering the nor-
mal–superconducting boundary energies, $\hbar v_F/kT_c$. With the experience gained from his work
with Sondheimer, he was able in this paper to solve the penetration depth analysis exactly, and
his suggestions fitted the data remarkably well.

In early 1957 Brian made a short visit to Moscow with a delegation of British scientists,
where he encountered Landau and Ginzburg, and Abrikosov, who expounded his flux lat-
tice ideas (figure 1). Landau, whom Brian admired very greatly, describing him as ‘beyond
envy’, strongly rejected Brian’s proposal, partly because Ginzburg–Landau theory led to no
non-locality of the supercurrent, although it did describe non-local behaviour of the order parameter. As Brian recalled (30):

I was severely taken to task by Landau and Ginzburg and all the Russians; we had a most marvellous shouting match at one another. What I do love about the Russians is they can shout at you without offence and can be shouted back at. It was purely intellectual shouting as far as the physicists were concerned.

During the same period, Brian had been in correspondence with John Bardeen (ForMemRS 1973) in Illinois, not only about his own ideas on non-locality but also about those on the experimental evidence then emerging for a gap in the electronic density of states in superconductors. Later that year, Bardeen, Cooper and Schrieffer (BCS) published their revolutionary pairing theory of superconductivity (Bardeen et al. 1957). In their paper Bardeen and his colleagues were at pains to demonstrate that their new theory confirmed Brian’s model almost exactly, the length $\xi_0$ being interpreted as the scale over which pairs of electrons are correlated.

Once the Moscow theoreticians had taken time to study the BCS theory, they generously sent Brian a private message conceding that he had been right. Much later, when one of us was a guest in Moscow, Ginzburg stressed on several occasions that, in his view, Brian’s work was Nobel Prize level physics.

In 1952 Lars Onsager (ForMemRS 1975) had given an interpretation of the de Haas–van Alphen effect, the oscillations of the magnetization of metals with applied magnetic field, in terms of the ‘Fermi surface’, a theoretical three-dimensional boundary in momentum space within which the conduction electrons of a metal were thought to be contained at absolute zero.
Alfred Brian Pippard

(Onsager 1952). The shape of this surface is important in determining many of the properties of the metal, for example its electrical and thermal conductivity, Hall coefficient, magneto-resistance and cyclotron resonant frequency. In 1954 there was no experimental determination of the shape of this surface for any metal. In the same year, Brian first showed that, in the extreme anomalous limit, the surface resistance provides a measure of one component of the curvature of the Fermi surface, averaged around the effective zone. He concluded that it might be possible, by making sufficient observations of the surface resistance in different orientations, to determine the geometry of the Fermi surface, at least in a fairly simple case such as that of copper. He made preparations for performing experiments with this aim during the sabbatical year he was about to take in Chicago.

An important interlude needs to be included at this point. Brian had continued with his music making and what followed is best expressed in his own words (30):

It was my good fortune to be taken one Sunday morning to coffee and cakes at the Oast House in Malting Lane where Billy and Sybil Drew held open house every Sunday and anybody who could was encouraged to play or sing. It became a regular occasion for me and I was only too happy to join in, especially as accompanist if no one better was there. This was not only a fine way of meeting people and hearing a lot of new music, but Billy’s god-daughter Charlotte Dyer lived with them and was studying calligraphy at the Art College. We married in 1955.

Charlotte and Brian left almost immediately for Chicago. Before Brian arrived, Morrel Cohen had arranged that the Institute for the Study of Materials grow a very large single crystal of copper. The success of the experiment depended on cutting extremely smooth surfaces through the crystal at different angles, and this was expertly performed in the Institute. Brian had brought his own apparatus with him from Cambridge but it did not work well, so he designed a new experiment, which was built in the Institute workshop. This delayed the start of the experiments, and during the intervening months he delivered a course of lectures that later became the core of his undergraduate text Elements of classical thermodynamics (8). The data were taken successfully and the analysis was performed on his return to Cambridge. From these data, Brian was able to determine the curvatures of the Fermi surface in different crystal orientations and so map out the Fermi surface of copper, a widely recognized tour de force (9). This was the first time that any Fermi surface had been obtained experimentally. It is not insignificant that on his bookshelves were many texts on three-dimensional geometry.

Brian had continued his progression up the academic ladder. He was appointed a university lecturer in 1950 and elected to the Fellowship of the Royal Society in 1956. In 1959 he became a University Reader and was awarded the Hughes Medal of the Royal Society of London. The John Humphrey Plummer Professorship became vacant, and he was appointed to that chair in 1960.

The investigations involving normal electrons orbiting Fermi surfaces required strong magnetic fields and, up to 1959, the Mond Laboratory had investigated such phenomena with the use of pulsed fields. However, by that time so many new phenomena were under investigation that Brian decided to design a facility that would provide steady fields of 10 teslas over a 2-inch bore. He was awarded his first research grant of £43,000, and the new Magnet Laboratory, tucked into a corner of the old Cyclotron Laboratory, was opened in 1961. Its design was workmanlike and brute-force, using up to 2 MW of power, a large transformer and a bank of silicon rectifiers delivering 27 kA at 75 V. The magnet windings were flat coils of 75 mm x 1.5 mm copper strip through which cooling water was driven by powerful pumps from a large reservoir in the basement (Adkins 1961).
During the Magnet Laboratory period, Brian wrote extensively on Fermi surface phenomena. For example, in 1962 he published an important note showing that, in the de Haas–van Alphen effect, the field acting on the electrons is the magnetic induction and not the applied field, which explained why the magnetic moment was in some circumstances multi-valued, leading to the development of domains of different magnetization (14). He also introduced the novel idea of magnetic breakdown, in which the classical picture of electrons orbiting around the Fermi surface was abandoned, and the possibility was allowed that they passed by quantum mechanical tunnelling through momentum space to a nearby piece of surface (12, 15). This idea made sense of the data on the de Haas–van Alphen effect that were threatening to become incomprehensible. He developed important ideas in acoustic–magnetic resonance (10). In 1962, his book *The dynamics of conduction electrons* (16) established his reputation as an omni-competent expert on Fermi surface matters.

With David Shoenberg, Brian also made effective contact with Soviet physicists interested in related problems, introducing their work to the West, an important development in the days before the automatic translations of Soviet journals by the American Physical Society. Brian’s ineffectiveness concept was taken up by Russian physicists in their formulation of cyclotron resonance in metals (Kaner & Azbel’ 1957), and in the discovery of the resonant penetration of microwaves through flat plates (Gantmakher & Kaner 1965). He had important interactions with I. M. Lifshitz (ForMemRS 1982) on the general theory of galvano-magnetic effects in Fermi surfaces of arbitrary shape. He became widely admired by the Soviet physicists.

In the Magnet Laboratory itself, Brian and his students conducted some remarkable and difficult experiments, particularly on magnetoresistance and helicons. The very complex subject of magnetoresistance remained for Brian a major preoccupation, culminating in his definitive text *Magnetoresistance in metals* published in 1989 (28), and still influencing current work in cuprate superconductors. Although so much was achieved in the Magnet Laboratory, it was a short-lived enterprise because reliable commercial superconducting magnets became available, and no attempt was made to reproduce the facility when the Cavendish moved to west Cambridge.

In 1961 Brian’s graduate student Brian Josephson (FRS 1970) was working on an experimental project concerning the magnetic field dependence of the penetration depth, when, in his spare time, he developed a new theory of quantum tunnelling between superconductors (Josephson 1962). He submitted this work as a Fellowship dissertation to Trinity College and it subsequently earned him the Nobel Prize in Physics. Brian Pippard always asserted that, at the time, he could not understand Josephson’s proposal, but encouraged him to consult Philip Anderson (ForMemRS 1980), who grasped its importance and defended it against the trenchant criticisms of Bardeen and others (22).

Nonetheless, once the existence of the Josephson effects had been established experimentally, and the principle of the superconducting quantum interference detector, or SQUID, had been established (Jaklevic et al. 1964), Brian promptly encouraged his student John Clarke (FRS 1986) to develop a Cambridge version, the superconducting low inductance galvanometer, or SLUG (Clarke 1966). Although Brian at this stage claimed to be no longer working in superconductivity, he encouraged two further students, David Tindall and John Shepherd (FRS 1999), to measure the boundary resistance present at superconducting–normal boundaries to electrical and thermal currents, using SLUGs to measure the tiny voltages involved. Those of us still in the field noticed promptly enough that Brian’s powers in making penetrating and unexpected theoretical suggestions were undiminished: the resulting paper in 1971 contained
no fewer than three far-reaching ideas (19). First, it had been pointed out by A. F. Andreev that when a low-energy electron excitation approaches a region where there is a superconducting energy gap, it is reflected as a hole excitation, and vice versa (Andreev 1964). As Brian had predicted, at low temperatures in clean materials the thermal current consequently shows a large boundary resistance, because the energy flow is stopped by the boundary; however, the electric current does not, because in Andreev reflection a new superconducting pair enters the superconductor, and the current continues unimpeded. Second, near $T_c$, at which some excitations have enough energy to pass over the energy gap, they enter the superconductor and create a ‘charge imbalance’, an excess of electrons over holes, or vice versa, and an electrical boundary resistance now arises, which turns out to depend on how long it takes this imbalance to decay. Third, when the superconductor contains enough impurity scattering centres, instead of Andreev reflection occurring, some electron excitations tunnel through to a scattering centre, are scattered back and re-emerge as electrons, so contributing an anomalous electrical boundary resistance, even at low temperatures. As usual, Brian’s theoretical suggestions fitted the data remarkably well.

A little earlier, in 1961, Brian was invited to give a banquet speech at an IBM international conference on superconductivity (11). He took as his title ‘The cat and the cream’, and startled his audience by asserting that physics, apart from fundamental particle physics, was largely worked out. Perhaps it was tongue in cheek, or perhaps he felt it so in a personal sense, for his extraordinary research productivity did decline somewhat thereafter. But this must not be exaggerated. He was always interested, his views were eagerly sought, and he continued to contribute important ideas.

CAVENDISH PROFESSOR: THE MOVE TO WEST CAMBRIDGE

In 1966 Brian became the first president of the newly founded Clare Hall. The College was created in response to the Bridges Report, which had noted the problems facing distinguished visitors to Cambridge who could not be accommodated as guests of the colleges. Clare College took the remarkable step of endowing Clare Hall as a graduate college that could offer hospitality to distinguished visitors and their families in Cambridge. Brian was the ideal choice as first president, with his somewhat iconoclastic views. He was responsible for overseeing the design of the college by the distinguished architect Ralph Erskine, including an adequately large living room in the President’s Lodge to accommodate his grand piano. As soon as the building was completed in 1968, he, Charlotte and their three daughters, Corinna, Deborah and Eleanor, moved into the Lodge.

Many of the preconceptions about Cambridge college life were swept away. Clare Hall was designed for visitors with families, and spouses were accorded the same privileges as Fellows. Although graduate students were included more or less as an afterthought in the original plan of the college, their numbers increased steadily, the rough rule eventually being that the total number of graduate students should be roughly the same as the total number of all classes of Fellow. Brian succeeded in making the college an informal and happy home for graduates, distinguished visitors and their families. His humanitarian instincts came to the fore when the college provided a home for Rudi Dutschke, a left-wing German student activist who had been shot during a Berlin street protest. Brian defended ‘Red Rudi’ against Conservative attempts to have him deported.
During the late 1950s, Nevill (later Sir Nevill) Mott FRS had become convinced of the need to move the Cavendish to new buildings in west Cambridge, and Brian quickly became the protagonist in promoting this enterprise within the university and raising the money for it. This turned out to be a very large task, and the building did not begin until the late 1960s. Brian was deeply involved in all aspects of the planning of the new buildings, working actively with Ian Nicol to ensure that the architecture was practical and the right services were provided. The design was intended to be flexible so that new experiments and instruments could be easily housed, and experience shows how important this has been. Almost every experimental area of the laboratory was reconfigured and redeveloped at least three times in the course of the succeeding 35 years. The complex matter of moving the laboratory and all its equipment from central to west Cambridge was managed successfully with John Payne.

In 1971 Brian was elected Cavendish Professor of Experimental Physics, in succession to Nevill Mott. It was characteristic of him that he gave his inaugural lecture at the undergraduate Cambridge Physics Society (20). It consisted of a set of physics experiments whose outcomes were to be predicted by a show of hands by the audience. As he intended, most of the audience got the answers wrong. In fact, several of these experiments foreshadowed his later interest in the physics of chaos. These ideas are also adumbrated in his books on *Response and stability* (26) and *The physics of vibration* (23), although he did not make the leap to the universality of chaotic phenomena.

As Cavendish Professor he automatically became the head of department, but he changed that rule in the late 1970s when he decided to split the posts. In 1978 he stood down as head of department and Alan (later Sir Alan) Cook FRS took over. He served as a member of the General Board of the Faculties from 1965 to 1968 and from 1975 to 1978. Although choosy about what he undertook at national level, he was President of the Institute of Physics from 1974 to 1976. He was knighted in 1974. When the University of Cambridge offered a very generous early retirement package in 1982, he took that opportunity.

In retirement he maintained to the end of his life an active interest in areas of physics that he found absorbing, becoming particularly intrigued by nonlinear phenomena of all sorts in classical physics. His books *The physics of vibrations* vol. 2 (24), *Response and stability* (26) and *Magnetoresistance in metals* (28) were published during this period. A major undertaking was his editorship of the three-volume work *Twentieth century physics*, which was published in 1995 (31). Besides contributing chapters, he took upon himself the task of editing a large fraction of the articles. He wrote 74 book reviews for magazines such as the *Times Literary Supplement, Nature* and the *London Review of Books* from the time of his retirement until 2005.

In April 1994, he was involved in a serious accident when he was knocked off his bicycle after a walk with Charlotte at Wandlebury County Park. He suffered a mildly fractured skull and a broken shoulder, as well as losing the sight of his left eye. He made an excellent recovery and was soon back playing the piano. He died of a stroke in 2008.

**THE STRUCTURE OF UNIVERSITY DEGREES AND TEACHING**

Brian devoted considerable effort to attempting at national level to change the structure of university degree courses in physics. In the 1970s the three-year courses were becoming increasingly crammed, and he considered it unrealistic to believe that the government would
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approve increasing the undergraduate degree to four years. He came up with the concept of a
two-plus-two physics course, the first two years being designed for those whose future needs
would require only a general understanding of physics, whereas the full-four year course
would be for those who sought a professional career in physics (17). He devoted a very large
effort to promoting this idea, but it gained surprisingly little support in the university and
elsewhere. His thinking was well ahead of its time. In fact, the solution came in the 1990s
when a successful change was made to a three-or-four-year physics course, which has proved
to be very successful.

During his tenure as head of department, Brian took a strong interest in the teaching
programme. The role of the Teaching Committee was strengthened and he was proactive
in the reform of undergraduate physics teaching in Cambridge. High lecturing ability was
required in promotions and new appointments. Project work for final-year undergraduates
was introduced in the 1970s, as well as options in advanced topics in the final year. It is
not surprising that his very decided views on what constituted real physics resulted in hot
discussions in the committee. Both of the present authors were long-standing members of
the teaching committee and we could certainly claim to have learned more real physics
in these debates than in a whole course of lectures. There were some subjects that Brian
abhorred—four-vectors, operator formalism and the grand canonical ensemble, to mention
but a few. There were outbursts such as ‘There is more physics in magnetism than in the
whole of relativity.’ These views were sincerely held and he was able to get away with it
because of his deep understanding of how simple concepts of classical physics and wave
mechanics could be used to understand complex problems. His horror was of mathematical
formalism that obscured what he considered to be the underlying physics content. Of course,
compromise was reached: there was no way in which the majority of the committee were
going to deprive the brightest students in the UK of the most valuable techniques of modern
theoretical physics, but it was a real battle.

A delightful way of appreciating Brian’s individual approach to excellence in the teaching
of physics is to read his series of short papers for the European Journal of Physics, of which
he was founding editor from 1991 to 1994. These illustrate not only his deep understanding of
physical phenomena but also his puckish sense of humour. In his short paper ‘How to make a
celt or rattleback’ (29), he remarked:

This letter describes an easy way to make a model which can be shown on an overhead projector.
Since the essential part, which is otherwise the most difficult to make well, is a portion of a wine
bottle, there is every reason to take pleasure in the construction.

Equally characteristic is the splendid paper ‘Demonstration experiments in critical behav-
our and broken symmetry’ (25). The first example, that of a rudimentary rope ladder that is
loaded and the bottom rung rotated through 360°, asks the student to estimate the torques
experienced on performing that rotation and then on returning it to its original position. This
is a delightful example of broken symmetry. Brian set it as a ‘physical insight’ problem for
the final-year examination in general physics. No student got anywhere near the correct
answer—one of us knows, because he had to mark the question. Indeed, he had to build a rope
ladder to be sure he had found the right answer.
ACHIEVEMENT, INFLUENCE AND PERSONALITY

As an experimenter, Brian was exceptional and his experimental designs were always a model of effective simplicity. For instance, one of us had the good fortune to take over the apparatus with which he had measured the skin depth in tin–indium alloys, the frequency of the klystron source having to be stable to one part in a million over periods of up to 10 minutes. Brian achieved this, not by some elaborate servo system but by employing no more than well-designed draught protection, a simple micrometer tuner with weight and pulley to eliminate backlash, and a stack of carefully aged high-tension batteries as power supply. He was always hands-on and he tackled extraordinarily difficult problems at times. They always worked as planned, for him and for his many research students.

We have already described the importance of his work. But it is also worth emphasizing the breadth of his interests: he frequently took a close interest in areas beyond his immediate concerns, and made useful contributions to them. A good example is his 1969 paper on the mechanism of the peak effect (18), a strengthening near a particular magnetic field of the critical current density in type II superconductors. He was the first to point out that a lattice of quantized flux lines will be more effectively pinned when it is not too rigid. He once set a graduate student to study convection in gaseous helium at low temperatures, having realized that it was possible in this way to work in a range of the significant dimensionless parameters inaccessible in any other classical fluid (Threlfall 1975). He also investigated in considerable detail the perturbations of the Foucault pendulum, and designed and had built a drive and mount that eliminated many of them (27). His parametrically driven Foucault pendulum is now a feature in the London Science Museum (figure 2).

It would be difficult to exaggerate Brain’s formative influence on his graduate students, many of them distinguished and dispersed around the world, or indeed on physicists generally; he had numerous valuable and constructive contacts in both the USA and the USSR, and his fertile mind brought many distinguished names to Cambridge as visitors.

In trying to assess Brian as a thinker, it must be said that he certainly disliked mathematical formalism, feeling that it often obscured the essence of things. Yet despite this professed antipathy, he was expert at using relatively elementary mathematics to solve problems that he chose to study. He loved unexpected solutions, such as phase-locking in nonlinear systems—not least in Josephson junctions. As a teacher he had irrational horrors of the grand canonical ensemble and four-vectors in special relativity. He always preferred to think of quantum problems in terms of semi-classical wave packets, never eigenfunctions and eigenvalues; and he refused to learn second quantization, which is necessary for a full appreciation of BCS and Josephson theory, just as he refused to learn to drive. His strength was his unerring instinct for the right, often subtle, illuminating idea. His concepts of ineffectiveness and coherence length, for instance, although qualitative and heuristic, were essentially correct, and were immediately grasped by a whole generation of physicists and applied to a wide range of new discoveries. Another characteristic of his thought was a love of phenomenological theories and illuminating general principles, approaches that did not require fundamental microscopic understanding but were based on some key insight into the nature of the phenomena. These include theories such as classical thermodynamics itself, or the quantum fluid idea, or Ginzburg–Landau theory, or his own non-local theory of superconductors, or even the Kramers–Kronig transform and dimensional analysis, both of which he used extensively. Of physicists he had met and worked with, he particularly admired Onsager and Landau, two theorists who combined
mastery of the illuminating idea, which he loved, with mastery of difficult formalism, which he probably felt was beyond him and not to his taste.

He had a strong sense of style in spoken and written English, and an equally strong sense of humour. As a supervisor and lecturer he was always stimulating, and his perceptive *Elements of classical thermodynamics* (8) and the challenging *Cavendish problems in classical physics*, which he edited (13), excited and tormented generations of students. He taught a wide range of courses, notably thermal physics, the wave mechanics course in the new Advanced Half-Subject in 1958, delivered from memory, and various versions of the opening course in Part IA, which led to his undergraduate text *Forces and particles* (21). It also has to be said that his teaching approach sometimes proved too quirky and personal for some students, who needed first to grasp the essentials.
Brian was capable of great kindness, and many of us owe him a great deal for his intellectual stimulation and his friendship. But he also had an enormous boyish relish in simply being clever, which all who knew him will remember vividly. Partly this was because of his remarkable memory and speed of thought. One of us recalls a dinner at Clare Hall during the Test match season, when the slowness of the sport was lamented and compared with Japanese Kabuki theatre. Brian’s instant response was, ‘Exactly so! How very often it’s just Noh Play today!’

ACKNOWLEDGEMENTS

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HONOURS AND MEDALS

1956 Elected Fellow of the Royal Society
1959 Hughes Medal of the Royal Society
1961 Holweck Medal and Prize of the Institute of Physics
1968 Member of the Swann Committee on Scientific Manpower
1969 Dannie–Heineman Prize of the Göttingen Academy of Sciences
1970 Foreign Honorary Member of the American Association for the Advancement of Science
          Guthrie Prize of the Institute of Physics
1973 Honorary Fellow, Clare College
1974–76 President, Institute of Physics
1975 Knight Bachelor
1993 Honorary Fellow, Clare Hall
2005 Lars Onsager Lecturer and Medallist of the Norwegian University of Science and Technology, Trondheim

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