Jack Hobart Piddington 1910–1997

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Jack Hobart Piddington was one of Australia's most distinguished radio scientists. After undergraduate studies in Sydney, he completed his doctorate at the University of Cambridge under Professor E.V. Appleton. During the Second World War he played a leading role in the secret development of our radar defences, notably with a highly successful air warning system. Afterwards he contributed to Australia's leadership in the emerging science of radio astronomy. The major part of his career was devoted to theoretical astrophysics, more specifically to cosmic electrodynamics in which field he made a number of innovative contributions.

Family Background and Early Life

Jack Hobart Piddington was born on 6 November 1910 in Wagga Wagga, NSW. He and his younger sister, Jean, were the only children of Frederick Clarence (Clarrie) Piddington and Leone Mabel (Connie) Millenet. Connie was the daughter of Jean Henri Millenet, a baker from Switzerland, and Charlotte Jones from Wiltshire, England. Clarrie was a wool classer, and it seems that he spent little time with his young family. He left when Jack was about five, leading to divorce in 1916, and died in Perth at the age of about 35.

Jack's father came from a remarkable family. His great-grandfather, William James Killock Piddington, was born in London in 1830 and moved with his parents to Australia when he was about six. The family settled in Hobart, where William eventually became a Wesleyan parson. He was appointed in 1852 to Bathurst, NSW, the first of many postings. In 1855 he married Ann Burgess of Hobart and they had seven children. The eldest, William Henry Burgess Piddington, became a bank manager before turning to politics. Elected to the New South Wales Legislative Assembly, he was active in the campaign for Federation before his premature death aged 44.

Another brother, Albert Bathurst Piddington, graduated BA from the University of Sydney with First Class Honours and a University Medal in classics. Initially he taught at Sydney Boys' High School and later lectured at the University (1889–94), and after qualifying in law, was admitted to the Bar in 1890. His reformist and radical outlook led to election to the Legislative Assembly (1895–98). In the early years of the new century he was appointed to chair various State and Federal Commissions. In 1912 he was offered a seat on the High Court of Australia by the Attorney General, W.M. Hughes, but declined when he realised it was a political appointment. He was appointed a King's Counsel in 1913, and was active in the Advisory Council of Science and Industry and the Commonwealth Institute of Science and Industry,[1] arguing against Hughes that the Institute should be free from political control.

Jack's grandfather, Frederick Hobart, the sixth of the seven children, became a clergyman, first as a Methodist like his father, and later as an Anglican. At the end of his career, he was Archdeacon of Tamworth. His wife was Blanche (née Bellair), and their son, Frederick Clarence, was Jack's father.

Following the divorce, Jack's mother, Leone, was faced with the financial problem of educating him. Jack's primary schooling was at the Wagga Rural School, where his interests were more on outdoor activities

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than his studies: 'has ability – won't work' according to an early school report. However, this changed in his final year of primary school when there was an incentive in the form of two scholarships for secondary school, one for twenty-five pounds and the other for twelve pounds ten shillings, both of which he won.

After his secondary schooling to Intermediate level in Wagga, Jack moved with his mother and her three sisters to Bondi, where he was accepted into Sydney Boys' High School, a selective school of high standards. There his skills in mathematics and science developed. In the 1929 Leaving Certificate examinations, he was dux of the school, winning the headmaster's prize (Albert Cup). He gained first place among the bursary winners, winning the Barker Scholarship No. II and the Horner Exhibition in Mathematics. These were sufficient to enable him to enrol in Engineering at the University of Sydney, overcoming his mother's worry that she could not afford the university education his talents clearly deserved.

Jack's remarkable prize-winning career continued through his undergraduate career. He won the George Allen Scholarship and the K.K. Saxby Prize for Mathematics I and the Smith Prize for Physics I in his first year (1930), and the Barker Scholarship No. I for Mathematics II and the Slade Prize for Practical Physics II in his second year (1931). After transferring to the Faculty of Science in 1932 he graduated BSc with First Class Honours in Mathematics. He returned to Engineering with the C.G. Caird Scholarship, and was awarded the Percy L. Weston Prize for Electrical Engineering I. He completed his BE in 1934 with First Class Honours and the University Medal. Over these five years, with prize money, scholarships and earnings from tutoring, his net income averaged more than the basic wage, then four pounds ten shillings per week.

Jack had also taken part in many sporting activities. He rowed for his faculty, won the inter-faculty hammer throw and also threw the javelin and the discus with some success. He was a champion rifle shot, representing in the King's Shoot. Probably his greatest love was tennis, in which he was captain of the Varsity VI team.

In the two years after graduation Piddington was engaged in radio and electronic research in Professor J.P.V. Madsen's Department of Electrical Engineering. For this work he was awarded an MSc degree in 1936, along with the Peter Nicol Russell Medal, for a thesis entitled 'The Frequency Stability of Valve Oscillators'. In his last year he was appointed an officer of the Radio Research Board, which was under the direction of the Council for Scientific and Industrial Research. During this period he published five research papers [1–5] on electronic devices.

In 1937 Piddington was awarded a Walter and Eliza Hall Engineering Fellowship, which was for three years, intended to be two years abroad and one year back in Sydney. He left Sydney in July 1936 on the P & O liner *Majola* on his way to England to continue his radio research work.

Cambridge and the Cavendish Laboratory

Piddington planned to use the fellowship to work at the University of London under Professor E.V. (later Sir Edward) Appleton, who later won a Nobel Prize for his discovery of the ionosphere. The plans were for Piddington to collaborate initially with Dr D.F. Martyn, then a member of the Radio Research Board, who had left Sydney six months earlier. Piddington and Martyn collaborated on the polarization of radio echoes and published a joint paper [7] in the *Proceedings of the Royal Society* in 1937.

In Cambridge, Piddington carried out research on the reflection of radio waves by the ionosphere and troposphere under Appleton's supervision. In 1938, he was awarded a PhD by the University of Cambridge; his PhD thesis was entitled 'The Scattering of Wireless Waves'. He published four papers [8–11] on his PhD work, the first [8] of which was co-authored by Appleton.

During his time in Cambridge Piddington also did an experiment for the British Air Ministry with the collaboration of the BBC and the RAF. Although not told its purpose, he soon realised that it was to supply information for Britain's secret development of radar. The experiments included measurement of the reflectivities of aircraft. Piddington built the receiver for this experiment. In his memoirs he remarks:

> So when we came back to Australia, we knew all the principles of radar and we also knew the amount of power we'd need in transmitters and qualities of the receivers to make a set that would be of any use in picking up aircraft.

He put this knowledge to good use during the war years.

On a more personal note concerning the trip to England, Piddington in his own memoirs remarked:

> With me was Catherine Wynne-Dyke, whom I met a year or so earlier – we were going to live together in London – it was quite a simple arrangement, but while I was on the ship I was informed that [Professor Appleton had] taken a Chair at Cambridge University and this opened a whole new set of problems. Any student at Cambridge University was totally restricted in their activities. Either they had to live in a College and be in doors by 10 o'clock each night, or they had to live with an accredited landlady, who practically locked him in his room at 10 o'clock every night. So Catherine and I got married in London and that made it quite simple. She was then the 'accredited landlady.'

Piddington recalled his days in Cambridge with his wife:

> When we went to Cambridge, we took a two-storey nice little house – a semi-detached – for twenty-seven shillings and sixpence a week and had two interesting years in a new country and with lots of travelling. I did a motor cycle tour of Germany and a few other countries, with another student, and then Catherine and I did a 5500 mile trip in a dilapidated little car which we eventually sold for thirty-five shillings. We got as far as Hungary, Rumania, Bulgaria and Yugoslavia – places that were not tourist areas, by any means, in those days. It was with considerable hardship, but extremely interesting.

He also commented that he knew Ernest (later Lord) Rutherford well: 'We used to go to his home at night and have parties occasionally'. Jack and Catherine's son John Denis was born at Cambridge in March 1938, and Appleton was his godfather. Jack and Catherine's marriage did not last long after their return to Australia, where they were divorced.

The War Years: Radar

After returning to the University of Sydney in 1938, Piddington's research for the Radio Research Board included the development of a high-power pulse transmitter and receiving system, ostensibly for ionospheric investigations but also capable of aircraft detection. This was the first radar system in Australia, but it was never used as such. With war imminent, the Australian Government established the CSIR Radiophysics Laboratory (RPL) to develop radar for the Defence Forces. Piddington was one of the first appointments to the staff. A copy of the letter of application, dated 28 August 1939, exists in Piddington's files, 'for the position of research physicist in the Radio Physics Section of the CSIR'. This letter contains a detailed summary of his research experience, and mentions that an unpublished paper on his secret work for the British Air Ministry on radar (described as 'some work with ultra-short wireless waves with co-operation from the BBC transmitter in London and the Royal Air Force') was in the hands of members of the Australian Radio Research Board.

At that time, air raid warning radar did not have a high priority in Australia's defence strategy and RPL's first radar was developed for the Army, to direct the fire of coastal batteries against enemy warships. Piddington was particularly involved in the design of the precision range-measuring system and other electronic units. At the end of 1940 he accompanied D.F. Martyn and Colonel Whitelaw to select ShD sites near coastal gun installations. The locations ranged from Rottnest Island in the west, along the southern and eastern coasts, to Darwin in the north, as well as Thursday Island in the Torres Strait and Port Moresby in New Guinea. The performance of this 200 MHz Shore Defence (ShD) radar, which was eventually installed at 17 locations around Australia, impressed the British Command in Singapore. Air Chief Marshal Sir Robert Brooke Popham invited Piddington to recommend suitable sites for sets in Singapore, Hong Kong, Burma and Malaya. Three months later, the Japanese entered the war before any ShD sets were installed in the Far East.

After the Japanese bombing of Pearl Harbor, the provision of air warning radars became urgent. Piddington summarized his involvement in the following words quoted from a newspaper article by Ian Lindsay in 1952:[2]

> We had foreseen that air warning was likely to be the major role of radar, but the RAAF had worked on the basis that they would get the air-warning sets from England. Pearl Harbor put a sudden end to that idea. On Pearl Harbor day I designed an air-warning set in two hours, and with 14 others worked on it. Five days later we had the set completed, installed, and tested at Dover Heights – and that rough set provided air warning for Sydney for the next nine months.

This feat was only possible because of Piddington's inspired solution to a major problem. ShD sets were available, but greater range was needed on aircraft and no valves giving higher power at 200 MHz existed in Australia. Piddington sacrificed the high ShD range accuracy and resolution for receiver sensitivity by drastically increasing pulse length, with a commensurate reduction of bandwidth. A necessary reduction of pulse repetition frequency introduced a complex 'echo visibility' effect and an experimental trial was essential. One of those closely involved in the project was Brian Cooper who comments as follows:

> My chief recollection of working with Jack was during the hectic period at the start of the Pacific War. About a dozen RPL staff could be called on for the job and I was given responsibility for the transmitter. We quickly found enough resources around the laboratory to produce a very 'haywire' experimental set in about five days. This was installed at Dover Heights on a 36 element array used earlier in the development of the ShD system. Aircraft were detected almost immediately at distances which established the feasibility of the design.

In the ensuing week, there was a crash program at the RPL, assisted by several RAAF radar mechanics, to produce three pre-production models by the end of January 1942. Three identical sets were made by HMV Ltd. and delivered to RPL for testing in March/April. Meanwhile, the New South Wales Railways was busy producing a lighter version of the ShD aerial structure, using available jigs and fixtures and an early electrical driving mechanism. The second set produced by RPL was handed over to the RAAF at the end of January 1942. It was flown to Darwin for installation at Dripstone Caves, a coastal site 10 km north of the town. The officer responsible for radar defence, Wing Commander A.G. Pither, had chosen the site and formed a unit to set up and operate the station. The RAAF were unable to bring the station into operation before the devastating Japanese air raid on 19 February 1942. Piddington knew nothing of this until mid-March when, with Brian Cooper, he was called to Darwin to help. Cooper recalls this visit:

> We arrived in Darwin on or about 18 March, after a two-day civil airline flight from Sydney via Adelaide. I think the RAAF team had done well with the installation under difficult circumstances. However, they had great problems matching and phasing the array, because they lacked adequate training and practice in the use of the impedance measuring unit provided. It took us two or three days to go through the matching and other adjustments and on 20 March, during this period, a small group of Japanese aircraft flew in and bombed the airfield. By 22 March, the station was fully operational and a large raid was detected by the radar. It was successfully intercepted by US fighters, 20 miles out to sea, and dispersed with losses.

It was just seven weeks since the equipment had been handed over to the RAAF.

Piddington was bitter at the failure to get the station working before the first raid. Due to confused planning of the three loads of equipment flown to Darwin, major delays occurred.[3] The aerial, still incomplete pending delivery of the dipole array, was lying on the ground when the Japanese attacked. During his visit, Piddington was concerned over the delays caused by the installation officers' problems with adjusting the array, but seems to have remained unaware of the extent of the pre-raid difficulties. This subsequently resulted in some over-simplified accounts of events that tended to trivialize their efforts, as in Mellor's version.[4] Twenty-five years later, Piddington refused an invitation from Air Commodore Pither (ret.) to attend the unveiling on 19 February 1967 of a memorial at the site of the radar station. The wording on the plaque recognized the roles of the RAAF and the US Air Force and included the words 'Designer: Dr J.H. Piddington'. Some days before the ceremony, the controversy between them was triggered again by an article giving Piddington's views in a Sydney newspaper,[5] followed by Pither's response in a letter to the Editor.[6] This exchange only complicated the issues involved.

In a wider context, Piddington's criticism remains valid. His anger and frustration were directed not at the men on the site, but at the higher levels where the problems and confusion at Darwin originated. Because of long complacency about the ready availability of British air warning radars, the RAAF had shown no interest in having RPL develop a local design, as the Army had done for coastal defence. When on 9 November 1941 the RAAF became responsible for setting up and operating a warning network, it was just one month before Pearl Harbor.

Pither, placed in charge, faced an enormous task, with just a few British sets in the country. Only Piddington's innovative radar, later designated the AW Mark I, offered the promise of a rapid supply of hardware. When Sir John Madsen, Chairman of the Radiophysics Advisory Board, offered 'all the assistance we can',[7] Pither refused to grasp this opportunity to enlist RPL's engineering and scientific resources to help organize and implement the Darwin installation. Instead he sent partly-trained and inexperienced radar officers to what was then the RAAF's first remote site, far from technical advice and assistance. When the radar first began operating about the end of February 1942, its performance was poor. Even then Piddington was not called immediately and none of the raids in the next two weeks were detected.

Out of the Darwin disaster came eventual success. With the radar performing efficiently, the station was soon functioning smoothly as a unit in an integrated defence system that gradually repelled the Japanese raids. Piddington's AW Mark I electronics, combined with a RAAF-initiated, light-weight (LW) aerial structure designed by the New South Wales Railways, became the LW/AW radar widely used by both the Australian and US Forces in the South-West Pacific.

Piddington was afterwards thanked by the Air Board for his contributions to air warning radar.[8] He went on, early in 1943, to form the nucleus of the Radar Countermeasures Group at RPL, with the initial task of detecting and jamming Japanese radar. Later that year he experimented at Dover Heights with a system of air-warning with height finding. In 1944, F.W.G. White sent him to England for two months, where he was attached to the Telecommunications Research Establishment to investigate the latest developments in radar and its application to civil aviation. He also advised the War Office on the need for simplified radar equipment in the Far East. Returning via the USA, he spent three months at the Radiation Laboratory, Boston on radar and civil aviation developments there.[9]

Radio Astronomy

After the war, Piddington worked for a time on the application of radar techniques to surveying and civil aviation. He then turned to the emerging science of radio astronomy and in 1947 began a fruitful collaboration with Harry Minnett. As most research was then concentrated at metre wavelengths, where the signals were strongest and many discoveries were being made, they elected to pioneer microwave astronomy. Piddington's interests now tended more towards theoretical problems and Minnett concentrated mostly on the development of observing equipment with the assistance of a technician, J.V. Houndman.

It was decided to explore the potential of the switched-radiometer principle in which the received radiation was compared with that radiated by a resistive reference at ambient temperature. A series of radiometers with a variety of war-surplus aerials were developed to explore the microwave spectrum. The first operated at a frequency of 24,000 MHz and later ones at 10,000, 3000 and 1210 MHz. The aerial for the 24,000 MHz equipment was a 1.1 m diameter metal searchlight mirror, ideal for that frequency. With this equipment, installed on the roof of the Laboratory, extensive measurements were made of radiation from the Moon [15] and the Sun [17].

The variation of thermal radiation from the Moon's surface was measured over three lunar cycles. The maximum did not occur at full moon or the minimum at new moon, as had been found in the 1930s by thermocouple measurements of surface temperature at long infra-red wavelengths. Instead they lagged by 45° of lunar phase and the amplitude of the oscillation was much less. The Moon's crust is partially transparent to microwaves and the surface temperature is transmitted to the lower layers by thermal waves. This suggested that while the infra-red radiation originated in the surface layers the microwaves came from below the surface, where the temperature variation would be smaller and would lag behind that on the surface. Minnett developed a model for a uniform crust, but no combination of parameters reproduced both the observed amplitude and phase.

Early polarization measurements of light reflected from the Moon's surface had suggested the possibility of a layer of dust covering the surface. J.C. Jaeger at the University of Tasmania and A. Harper at the National Measurement Laboratory, Sydney, pointed out that a layer of dust or fine gravel, in the near vacuum on the Moon, would have a very low thermal conductivity and used this to explain early thermocouple observations during an eclipse. Piddington then modified Minnett's model to include a thin blanket of dust or fine gravel over the uniform rock crust and found that prediction now agreed with the observations. It was estimated that the microwave radiation emerged from an average depth of about 40 cm below the Moon's surface and that on average the thickness of the overlying blanket of dust was of the order of 1 mm [15]. The predictions about the nature of the Moon's surface were confirmed twenty years later by Neil Armstrong's footprints in 1969.

The measurements of microwave solar radiation implied equivalent disk temperatures that rose progressively from 10^4 K at 24,000 MHz [17] as the frequency decreased. This corresponds to higher temperatures at higher levels in the chromosphere where the radiation originated. At 3000 MHz there was evidence from a solar eclipse of one small area with a temperature of 10^6 K and another small area that appeared to emit partially circularly polarized radiation [16]. This observation was made with a novel rotatable screen in front of the paraboloid aperture that was the microwave equivalent of a quarter-wave plate in optics.

Progressively, as the frequency decreased, a slowly-varying (*S*) component of radiation, proportional to sunspot area, became larger. Piddington and Minnett suggested that the *S* component is due to localized regions at temperatures of about 10^7 K, often in the vicinity of sunspots. The radiation from a model hot region was analysed in detail to obtain the emission spectrum and polarization characteristics [19]. The results agreed reasonably with the observed data available at that time and the theory is essentially the one currently accepted for the *S* component.

With the most sensitive receivers and the biggest aerials then available, it was not possible at the two highest microwave frequencies to detect cosmic sources. Even at 3000 MHz, the experimental difficulties were still considerable, and mostly only upper limits of flux density could be established for a particular known source. With the move to a frequency of 1210 MHz, however, it was possible for a time to borrow a wartime paraboloid with an aperture section of 5.5 x 4.9 m that was used by W.N. Christiansen for solar studies at Potts Hill near Sydney. Also, a rotary capacitance switch was developed and it became technically feasible to switch between the big aerial and a second paraboloid, 1.7 m in diameter. This was used as reference instead of a resistive load at ambient temperature, as in the preceding radiometers. By directing the small aerial to a uniformly cold part of the sky, the two aerial temperatures were equal in the absence of a source in the beam of the big aerial. In this fully balanced form, independence from gain variations was finally achieved.

It was now possible to study a number of sources far beyond the solar system [21]. Measurements were made of known discrete galactic sources such as the Crab Nebula, Centaurus A, and Taurus A, and the extragalactic source, Cygnus X, which had not previously been isolated from the general galactic background. The flux density spectrum in the range 100–1210 MHz suggested that the source was the result of thermal emission from clouds of ionized interstellar gas [23]. (HII regions had been identified by Strömgren from optical data several years earlier.) Measurements in the central region of the Milky Way led to the discovery of a new discrete source, Sagittarius A, now known to be the nucleus of our Galaxy.

In 1952 Piddington published his first book, *Radio Astronomy* [54]. His objectives in writing the book were summarized in the Preface: 'In this book I have tried to give a complete but not detailed account of the science of radio astronomy which may be useful as an introduction to this subject for scientists working in other fields. At the same time by minimizing mathematical processes and trying to simplify the descriptions of physical principles involved I have tried to make it informative to readers with little scientific training.'

Early Work in Theoretical Astrophysics

Piddington had developed an interest in the theory of the phenomena that generate the observed radio signals, and he decided to give up observational astronomy to concentrate on theoretical astrophysics. He described this decision as follows:

> In 1956, I gave up observational work and started 30 or more years of mainly theoretical work on the objects that we had been observing up till then. I started this off with a long trip around the world which was necessary to talk to various people and visit labs in Cambridge, Stockholm, Paris, a whole string of them in America, Leiden and so on. I was away for about 7 months and that journey was really the beginning of the end of my marriage to Nancy MacDougall.

Jack and Nancy Gould MacDougall had married on 18 June 1948; she was the eldest daughter of Gould MacDougall (deceased) and Miriam May Guille. They had two daughters before they divorced in 1962.

Piddington's earliest work in theoretical astrophysics involved interpretations of radio astronomical observations [24,25,26, 27,29]. The interpretation of radio emissions from the Sun stimulated an interest in solar physics that was a major interest throughout his subsequent career. However, the area of his first major successes resulted from his investigations into a more fundamental problem: the properties of partially ionized gases.

The physics of ionized gases, now called 'plasma physics', had developed rapidly in the 1940s and early 1950s, based initially on a fluid description called magnetohydrodynamics (MHD). The high electrical conductivity of plasmas implies that any magnetic field, **B**, is frozen into the fluid so that they flow as a single entity. The conductivity is actually anisotropic with three different components (the 'parallel' component refers to the electric field parallel to **B**, the 'Pedersen' component to that perpendicular to **B**, and the 'Hall' component to that along **E** x **B**). In a fully ionized gas, the conductivity is finite because of resistance due to collisions between electrons and ions, but in a partially ionized gas, which contains both ionized and neutral particles, collisions between electrons or ions and the neutral atoms or molecules can play a dominant role. This complicates the physics substantially, because the electrons and ions are tied to the magnetic field lines but the neutral particles are unaffected by the magnetic field. Electrodynamic forces affect only the ionized component, accelerating it relative to the neutral component, while collisions between ions and neutrals provides a drag that tends to reduce any relative flow between the ions and neutrals.

Piddington's first paper [30] in this field in 1954 made several important contributions to the physics of partially ionized gases, and to its astrophysical implications. The main point in this paper concerned the dissipation of magnetic energy resulting from collisions between ionized and neutral particles. This basic result is that the power dissipated per unit volume due to an electric current, **J**, flowing across the magnetic field lines is $|\mathbf{J}|^2/\sigma_c$, where the relevant conductivity is $\sigma_c = \sigma_P + \sigma^2_H/\sigma_P$ in terms of the Pedersen (P) and Hall (H) conductivities. This result was discussed further by T.G. Cowling, who had derived the result independently. In the definitive paper in 1956 Cowling acknowledged Piddington, stating 'I do not query Piddington's result, [and] I give a simpler and more general derivation'. The foregoing expression for σ_c has subsequently become known as the 'Cowling conductivity'. This expression for σ_c applies to both fully and partially ionized gases, but its implications are much more important when ion-neutral collisions play a significant role.

Piddington pointed out two important related effects associated with relative motions of the ionized and neutral components. One concerns the damping of MHD waves. He showed that the drag on the ions by the neutrals in an MHD wave leads to a much stronger damping of the waves than in a fully ionized gas [30,31,49]. This damping is important in a variety of astrophysical contexts, amongst which Piddington emphasized the heating of the solar corona and solar flares. (However, it is now thought that there is no neutral gas in the solar corona.) The other effect associated with relative motions of the ionized and neutral components in partially ionized gases is now called 'ambipolar diffusion'. The fact that the magnetic field is tied directly only to the ionized component of the gas allows magnetic field lines to slip through a weakly ionized gas due to a relative motion between the ionized and neutral components. An important application is to the theory of star formation, as originally introduced by L. Mestel and L. Spitzer in 1956. In a sufficiently weakly ionized medium such as a molecular cloud, the Lorentz force gives the ions and electrons plus the inductively coupled magnetic field a significant diffusion velocity with respect to the gravitationally contracting neutral bulk, so that the galactic magnetic field is not an insuperable barrier to the fragmentation of the cloud into proto-stars. Leon Mestel (private communication) recalls that the importance of ambipolar diffusion was finally recognized at an international meeting in Stockholm in 1956, where Piddington contributed significantly to the discussion of it. However, he did not discuss this topic in his published conference paper [49]. Mestel regrets that Piddington did not share the credit for his independent recognition of the importance of ambipolar diffusion.

A related problem that Piddington first considered in the mid-1950s is the origin of the magnetic fields in cosmic plasmas. His first attempt to explain how magnetic fields might be generated was in a paper on MHD shock waves presented to the Royal Astronomical Society [43]. His idea was viewed with scepticism by H. Bondi and F. Hoyle in the subsequent (published) discussion. Leon Mestel was referee of a more detailed paper which he recalls contained a mixture of penetrating foresight (on the generation of toroidal magnetic fields by differential rotation, especially by collapsed objects) and elementary errors relating to the physics of shock waves. Mestel rejected the paper, and Piddington later published a revised version of it, with the erroneous section omitted, in the *Australian Journal of Physics* in 1957 [44]. Piddington abandoned the idea. The origin of cosmic magnetic fields remained an enigma for him throughout his career, as he was sceptical and critical of more conventional 'dynamo' explanations for magnetic fields in various cosmic objects.

In this early phase of his theoretical work, Piddington pursued his interest in the properties of waves in plasmas further. He discussed the transport of energy by MHD waves, and their ability to heat plasmas [32,33,34]. He proposed that the heating of the solar corona is due to Alfvén waves [38,41,49]. The idea is that there is a flux of Alfvén waves from below the solar surface that transports energy into the corona where it is dissipated due to neutral-ion collisions. This explanation of the heating in the Sun's upper atmosphere won him the University of Melbourne's 1958 Syme Prize for the best Australian research in physics. (Much later Piddington reviewed this early work at an IAU Symposium in Surfers' Paradise in 1973, and the lengthy subsequent discussion of his paper gives insight into the influence of his contributions [107].) Another topical problem in cosmic electrodynamics in the 1950s was the acceleration of high energy particles, notably cosmic rays, and Piddington addressed this problem several times through his career. He argued that models for acceleration of particles due to electric fields along the magnetic field (as proposed by Giovanelli) are untenable [26], on which point Cowling emphatically agreed with him. Piddington argued for a form of Fermi acceleration to account for galactic cosmic rays [44,92].

Returning to the properties of waves in plasmas, Piddington generalized the magneto-ionic theory, developed originally by Appleton and others to describe radio propagation in the ionosphere, by including an electron pressure term and ion inertia in the equations [33,35]. In a series of papers [36,37,39,47] he also discussed the growth of space-charge waves due to two counterstreaming distributions of ions. In the simplest case [47] he was concerned with solutions of the equation $\omega_0^2/(\omega - kv)^2 + \omega_0^2/(\omega + kv)^2 = 1$, where ω_0 is the plasma frequency of each stream which have velocities $\pm v$. At the time this problem presented conceptual difficulties in the interpretation of the solutions with imaginary k for real ω. Piddington's discussion predated the resolution of these conceptual difficulties in the early 1960s and, viewed in retrospect, although some of his arguments were perceptive, not all his conclusions were correct.

Magnetospheric Physics

Piddington's interest in the ionosphere began with his collaborations with D.F. Martyn [7] and E.V. Appleton [8] in the late 1930s, and he returned to this interest briefly in 1951 [20], and again in the early 1960s [59,63,65,67]. However, this interest in the ionosphere, which extends from several tens to a few hundred kilometres about the Earth's surface, became secondary to his interest in the magnetosphere, which extends to several Earth radii and much further in the geomagnetic tail. The magnetosphere is the region above the ionosphere that is threaded by magnetic field lines that are tied to the Earth.

The first magnetospheric model that Piddington developed was concerned with the interaction between the solar wind and the outer boundary (magnetopause) of the magnetosphere. The solar wind consists of hot solar plasma flowing past the Earth at several hundred kilometres per second, containing its own 'interplanetary' magnetic field. In 1960 Piddington argued [51,52] that there is friction between the solar wind and the boundary layer that causes the Earth's magnetic field to be dragged back to form a long tail, sometimes referred to as the tear-drop model. This prediction of a geomagnetic tail was confirmed five years later by observations with the NASA spacecraft IMP 1 (Interplanetary Monitoring Platform). It was a major success for him but, from an historical viewpoint, he received relatively little recognition for it.

Another feature of Piddington's original model is that the magnetosphere is closed, in the sense that the magnetic field lines always remain tied at both ends to the Earth. The magnetic flux tubes are dragged from the sunward side of the magnetosphere into the tail, maintaining their identity as closed magnetic structures. This is in contrast to a model proposed by J.W. Dungey in 1961 that involves magnetic reconnection between the geomagnetic and interplanetary field lines. A magnetic flux tube connected to the Earth and a magnetic flux tube in the solar wind come together on the sunward side of the Earth and reconnect to form two open magnetic flux tubes. The open magnetic flux tubes are dragged to the tailward side where a compensating reconnection occurs. The open (reconnection) model was much more widely favoured than Piddington's closed model.

Piddington used the closed model of the magnetosphere to discuss a variety of magnetospheric phenomena, notably geomagnetic storms [58,62,64,68,69,74,79], the ring current [76] and auroras [59,60]. Years later he reflected on his model in what was his last paper [125] on the topic: 'The model was of the type now known as the closed magnetosphere, in which the geomagnetic field is everywhere separate from the interplanetary magnetic field. Geomagnetic field lines on the daytime magnetopause are driven by frictional forces through the dawn and dusk meridians and then drawn out into the tail.' He acknowledged that he had missed an opportunity to predict that the frictional force he had invoked would also cause another important effect – magnetospheric convection, as described by W.I. Axford and C.O. Hines in 1961. He also acknowledged that his closed magnetospheric model was less favoured than the alternative open model, and he argued that the observational evidence strongly, but not exclusively, favoured the closed model.

Jupiter also has a magnetosphere, and it had been known since the mid-1950s that Jupiter is a very bright, bursty radio source at decametric wavelengths. A very surprising correlation between Jupiter's decametric radio emission and the position of the moon, Io, was discovered by E.K. Bigg in 1962. (Io is the innermost of the four moons of Jupiter discovered by Galileo with the then newly-invented telescope.) This correlation was surprising because the radio emission originates near Jupiter's surface and not near Io, requiring that some disturbance must propagate from Io to Jupiter and excite the radio emission there. The explanation of this 'Io effect' became a major unsolved problem in cosmic electrodynamics. In a paper in *Nature* in 1968 Piddington and J.F. Drake [85] provided an explanation that quickly became accepted. The idea is that Io is a good electrical conductor, and that this allows the magnetic flux from Jupiter that threads Io to become frozen-in to Io. Piddington and Drake referred to this magnetic flux as Io's force tube (IFT). The IFT is dragged through the Jovian magnetosphere by Io, and this sets up a current system that closes through the ionosphere of Jupiter. This model has many features in common with Piddington's earlier ideas on the formation of the geomagnetic tail and on magnetospheric storms. This basic idea was developed in more detail by P. Goldreich and D. Lynden-Bell in 1969, and later by Piddington himself [98]. Although a later development (the discovery of the Io plasma torus) implied that the theory in its original form cannot be strictly correct, this is regarded today as requiring a modification of the original idea rather than invalidating it. The explanation of the Io effect was another major success for Piddington.

The Later Years

Piddington's creative period as a theoretical astrophysicist continued well into his 60s. The emphasis in his later work was on two related problems: the structure and evolution of a magnetic field on the Sun, and the role of magnetic fields in astrophysics.

Piddington's early interest in solar physics developed into a major research interest in the 1970s, after he had transferred from the Division of Radiophysics to the Division of Physics at Lindfield, where the Chief, Dr Ron Giovanelli, was an international authority on solar physics. The relationship between Piddington and Giovanelli in the 1970s was supportive, although Piddington had criticized Giovanelli's work in the 1950s and been strongly supported by Cowling at the time. (Piddington's last published work was a memoir on Giovanelli's life [136].) Piddington had strong views against some of the conventional ideas relating to the solar magnetic field, including theories for the solar dynamo, sunspots and solar flares. In a series of nine papers [111–121], he developed his own model for solar magnetic structures, notably sunspots, based on a 'flux-rope theory', and he also formulated a model for solar flares [109,127] based on his model for coronal heating by Alfvén waves. Although these papers from late in his career contain some compelling arguments, the ideas remain largely qualitative, with neither analytic nor numerical models to support them.

Piddington's ideas on the role of magnetic fields in astrophysics were unconventional. It was well known that the very high electrical conductivity implies that dissipation of magnetic energy should be very slow in cosmic plasmas, far too slow to explain many phenomena. This was first recognized in connection with sunspots and solar flares, and led to its being postulated that there exists an enhanced form of dissipation, sometimes called 'turbulent diffusion'. Piddington rejected this view, and this led him to have deep reservations concerning theories that relied implicitly or explicitly on such enhanced dissipation. It led him to criticize widely accepted conventional models, including those for the open magnetosphere, the solar dynamo and the solar cycle, the formation of sunspots, solar flares, and the galactic dynamo. He objected to dynamo theories for both the solar [93,96,103] and galactic [92,97,98, 99,113] magnetic fields, arguing that the turbulent diffusion required for dynamos to operate effectively is untenable, and argued strongly for the importance of primordial magnetic fields. Although he had earlier recognized that a primordial field is untenable in some cases, notably the Crab Nebula [45], the Galaxy [44] and the Universe itself [55], he invoked primordial magnetic fields in a 'unifying' model for the formation of galaxies. The idea, first proposed in 1964 [66], is that as galaxies form, the primordial intergalactic magnetic field is wound up and compressed until it becomes dynamically important. The shape and structure of a galaxy are determined primarily by the initial orientation of the angular velocity of rotation of the protogalaxy and the intergalactic field. In particular, he argued that this model could account for the spiral structure of spiral galaxies [78,105], and that it favoured an electrodynamic model for radio galaxies and quasars [66,73,80,90]. The widely accepted theory of spiral structure is based on density waves, and he argued against the conventional explanation and in favour of the magnetic model [100]. He also argued that the galactic field plays a similar role in stellar magnetic fields [108]. These models received little support from others.

Many of Piddington's ideas were unconventional and it is not surprising that he was involved in several controversies. The most notable of these was in connection with turbulent diffusion and the dynamo problem, and involved him in exchanges with E.N. Parker and T.G. Cowling. The controversy concerning turbulent diffusion came to a head at an International Astronomical Union Symposium in Prague in 1975, where Piddington's paper [119] was presented in absentia by Ron Giovanelli. The strength of the feeling on this topic is illustrated by the way Parker prefaced his comments on Piddington's paper: 'I would like to point out the origin of Piddington's divergent view, the "Piddington heresy", in contrast to the orthodox dogma...'. Nevertheless, Parker later remarked: 'Piddington has put his finger on a weak spot in turbulence theory. There is no formal deductive answer to his objection to turbulent diffusion.' Piddington was not present to defend his views but did so in a later paper [130] where he chose to quote from a panel summary critical of his Prague paper [119], presumably to emphasize the extreme nature of the opposition to his theories: 'to doubt the existence of turbulent diffusion is to doubt the validity of Maxwell's equations'. The only personal interaction one of the present authors (DBM) had with Jack Piddington was in 1981 when paper [130] was in press; on a preprint of that paper Piddington wrote a personal comment that made his opinion clear: 'This is one of the most important and controversial areas in astrophysics – and maybe in plasma physics in general'. Piddington's exchange with Cowling [132,134] was more temperate. After quoting from private correspondence, Piddington [134] summarized: 'Cowling and I are in agreement to the extent that convective motions destroy cosmic magnetic fields...[only on] the actual time of decay...there is disagreement.' However, as he went on to emphasize, it is the assumed very slow decay that was central to his theories.

Recollections of Jack Piddington

An early recollection of Jack Piddington appears in the late Joan Freeman's autobiography.[10] She remembered him during the Second World War as an 'uncommunicative but impressively capable looking man'. Harry Minnett knew Jack from 1940 and recalls: 'He was a mild mannered and reserved person and it took some time to get to know him. In public he seemed imperturbable, but this was rather a facade. Jack was essentially a kindly, somewhat shy man, who in his early career was an expert electronic engineer and designer. He was also an accomplished mathematical physicist and always confident about the correctness of his scientific ideas.' Jack's daughter-in-law, Mrs Edna Piddington Turner, found him 'a very private man', who had to be coaxed into writing his memoirs.

Personal recollections provided by several former CSIRO scientists whose careers overlapped with Jack Piddington's after he became a theoretical astrophysicist, showed ambivalent attitudes towards him. Few had close relations with him, Harry Minnett being a notable exception. One scientist comments that Jack Piddington was 'not a popular figure in Radiophysics', perhaps because he was older than most and stuck to himself; a second mentions 'a triangle of antagonism between John Bolton, Ruby Payne-Scott and Jack Piddington'; but a third says that although he was rather reclusive he got on OK with everyone. Harry Minnett acknowledges these antagonisms, and describes them as 'creative tensions' between 'very different personalities'. One well-known antagonism was that between the then Chief of the Division of Radiophysics, Taffy Bowen, and an earlier Chief, D.F. Martyn, with whom Piddington was close friends. Martyn was ill-suited in a management role, and his appointment as Chief was clearly a mistake. Len Hibbard attributed the appointment to Madsen 'trying to run the show' by having his own 'yes-men' in positions of influence. Bowen did not value Piddington's theoretical talent and wanted him to leave. Paul Wild, who joined the Division of Radiophysics after the end of the Second World War and was later Chief of the Division himself, recalls the period when Martyn was Chief being described by Bowen as the 'Martyn/Piddington regime', but those who remember the period have no recollection of Piddington influencing Martyn on management issues.

Paul Wild recalls that Jack Piddington took an active part in the regular radio astronomy meetings at Radiophysics, chaired by Joe Pawsey. He often had the chore of reading Piddington's papers, which he found tiresome and frustrating. He recalls Piddington as being prolific, often with several papers in preparation at the same time, and that the initial versions he got to read were usually sloppily put together. His impression was that the strict system at Radiophysics helped limit Piddington's papers, and prevented some getting through at all, which did not seem to bother Piddington. Leon Mestel recalls 'from conversations with several Australians that Piddington was not universally popular, largely because of the way he pursued science. He was criticized for circulating half-worked out material for comment, but then not giving adequate acknowledgement.'

In addition to Piddington, in the 1950s there were at least two others at Radiophysics with interests in theoretical astrophysics, Kevin Westfold and the late Steve Smerd. However, despite some overlap in their research interests with Piddington, neither had significant interactions with him. Brian Robinson and Jim Roberts, who joined Radiophysics in 1954 and 1961 respectively, recall that in about 1966 Piddington started a weekly discussion group in theoretical astrophysics that lasted about two years. This group also included the late Max Komesaroff. Piddington acknowledged discussions with Roberts and Komesaroff in some of his papers at the time. According to Brian Robinson, these meetings stopped when the Division of Radiophysics moved from the grounds of the University of Sydney to Epping: Piddington transferred to the Division of Physics which remained at the University site.

In the late 1950s the Upper Atmospheric Section of CSIRO was created for D.F. Martyn at Camden, southwest of Sydney, and Piddington was a regular visitor there. Bob Duncan, who joined the Section as a junior scientist, recalls that Martyn tried unsuccessfully to get Piddington to transfer to Camden. Duncan remembers that Piddington was almost always a speaker at the weekly colloquia in Camden. Piddington expressed rather extreme opinions on the role of magnetic fields in astrophysics, frequently being critical of the then somewhat fashionable (although controversial) but less extreme views of Alfvén (who later won a Nobel Prize). The closeness between Piddington and Martyn is indicated by an incident that Bob Duncan recalls from the early days at Camden: Piddington persuaded Martyn to have shock treatment for his mental illness, and Piddington and Duncan signed the required form for this to occur.

Another who took part for about a year (1962) in the colloquia at Camden was Keith Cole who comments (in the third person) that he 'had been inspired in 1960 by Piddington's work on the interaction between the solar wind and the magnetosphere. However, Piddington didn't agree with some of Cole's conclusions, identifying them as similar to those of Axford and Hines, with whom Piddington also had a dispute. Piddington's theory was essentially that of transient interaction whereas those of Cole and of Axford and Hines dealt with a steady state. The credit for the concept of magnetospheric convection went primarily to Axford and Hines. In Cole's opinion, Piddington did a lot of outstanding things, but did not reap the credit he deserved for his major contributions. His prediction of the magnetotail of the Earth was independent of that of Frank Johnson which is much more referenced. Also his seminal work on interaction of the solar wind with the geomagnetic field to produce magnetic disturbance represented a new complexion of research in the field. Cole's impression of Piddington was of a modest man, of strong will and opinion, who was not bothered by the relative lack of recognition of his work.'

The close friendship between Piddington and D.F. Martyn continued from the 1930s until Martyn's death in 1970. Martyn nominated Piddington for election as a Fellow of the Australian Academy of Science in 1959 (the nomination was seconded by L.G.H. Huxley and supported by T.M. Cherry, K.E. Bullen and A.R. Hogg), and the citation covered essentially all his work, including that on war-time radar. Piddington was elected FAA in 1963. The closeness of the friendship is indicated further by a happy event in Jack Piddington's life: on 30 September 1965 he married Patricia Olive Devereux in the District Registrar's Office at St Leonards with David and Margot Martyn as witnesses. After Martyn's death, Piddington wrote a memoir on his life [95].

Epilogue

In looking back over Piddington's professional career, it separates into three periods that were very different not only in the work he did but also in the way he interacted with others. In the wartime years his expertise in radar led him to to interact widely with his professional colleagues, led to significant involvement with industry and the military, and involved travel to diverse places. The years in radio astronomy made him part of a group of outstanding young scientists who were engaged in the excitement of creating a new field of science. His collaboration with Harry Minnett on radio observations of the Moon and their interpretation was one highlight of his career. In contrast, a notable feature of his later career in theoretical astrophysics is that so much of it was carried out in relative or almost complete isolation. He was isolated on the international stage, and locally he had no collaborators, seemingly more from choice than from lack of opportunity. It is remarkable that all but one of his papers on theoretical astrophysics have him as sole author. The exception is the original paper [85] on the Io effect, written while he was visiting the University of Iowa, and this exception was perhaps the highlight of his career as a theoretical astrophysicist.

Piddington formally retired from the CSIRO in 1975, on reaching the retirement age of 65. He continued his research and his association with the CSIRO as a Senior Research Fellow until 1984. His last published paper was in 1983. He had been a sportsman all his life, and he continued to participate in sport after his retirement. He played competition tennis for over sixty years at the top of second grade and occasionally in first grade. His services to the sport and his love for the game were rewarded by a Life Membership of the White City Club. After retirement he turned to lawn bowls and played at Pennant level. He enjoyed the challenge, particularly when his eyesight was failing in his latter years. He died peacefully on 16 July 1997 from cancer.

Jack Piddington lived for many years in Mosman with his wife Patricia. She survives him along with his son Denis (who changed his name to Piddington Turner) from his first marriage, his daughters Jan and Anthea from his second marriage, seven grandchildren and eight great-grandchildren.

Curriculum Vitae

Educated:

Wagga High School and Sydney Boys' High School

University of Sydney: BSc 1931, BE 1933, MSc 1935

University of Cambridge: PhD 1938

Research Fellowship, University of Sydney 1934–36

Appointments:

Walter and Eliza Hall Fellow, 1936–38

Sydney University Fellow, 1938–39

CSIR (subsequently CSIRO) Division of Radiophysics, 1939-66

Radar Consultant, British Army Malaya, Hong Kong, Burma, 1941

Radar Consultant, Air Ministry UK, 1944

CSIRO National Measurement Laboratory (subsequently Division of Applied Physics) Chief Research Scientist to 1975; Senior Research Fellow 1975–84

Visiting Professor, University of Maryland, 1960

Visiting Professor, University of Iowa, 1967

Consultant Astronomer, Kitt Peak National Observatory, 1974

Fellowships etc:

Fellow of the Royal Astronomical Society

Member of the International Astronomical Union

Fellow of the Australian Academy of Science (elected 1963)

Member of the editorial board of *Astrophysics and Space Science*

Fellow of the Astronomical Society of Australia (to 1983)

T.K. Sidney (Summertime) Award, Royal Society of New Zealand, 1959

David Syme Research Prize, University of Melbourne, 1958

Acknowledgements

In writing this memoir we have made extensive use of factual information from letters, certificates and newspaper cuttings collected into a document 'Jack's Story' by his daughter-in-law Mrs Edna Piddington Turner. We are most grateful to Mrs Patricia Piddington for access to the original papers. Leon Mestel helped considerably with the discussion of some of Piddington's earlier work on cosmic electrodynamics. Others who provided contributions or comments include Wilbur Christiansen, Bob Duncan, Keith Cole, Brian Cooper, Len Hibbard, Jim Roberts, Brian Robinson, Kevin Westfold and Paul Wild.

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The following list of publications is essentially that in Piddington's curriculum vitae. The details could not be checked for a few of the entries which are technical reports or are in obscure journals. Piddington also wrote a number of technical and other reports, copies of which are retained in the library of the CSIRO Division of Radiophysics. A list of these with their 'RP' or 'TI' numbers in each of the years 1940 to 1946 contains 14, 19, 4, 7, 5, 3 and 5 reports, respectively. Most of these reports are related to radar.

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2. 1936 A fundamental suppression type harmonic analyser. *Proc. Inst. Radio Eng.* **24**, 591–596
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4. 1936 A temperature compensated dynatron oscillator of high frequency stability. *CSIR Bull.* No. 95
5. 1936 An electric harmonic analyzer of the fundamental suppression type. *CSIR Bull.* No. 95
6. 1937 The polarization of radio echoes. *CSIR Bull.* No. 110
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